THE

WORKS

OF THE HONOURABLE

ROBERT BOYLE.

VOLUME III.

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TO

THE RIGHT HONOURABLE

JOHN Lord CARTERET,

One of HIS MAJESTY's Principal Secretaries of State,

THIS

THIRD VOLUME

OF THE

WORKS

OF

The Honourable *ROBERT BOYLE*

Is humbly dedicated,

By his LORDSHIP's

Most devoted and

Most obedient Servant,

Andrew Millar.

A

CONTINUATION

OF

NEW EXPERIMENTS

PHYSICO-MECHANICAL,

TOUCHING THE

SPRING and WEIGHT of the AIR, and their EFFECTS.

THE FIRST PART.

Written by way of LETTER, to the Right Honourable the Lord CLIFFORD AND DUNGARVAN.

WHERETO IS ANNEXED

A Short Difcourfe of the ATMOSPHERES of CONSISTENT BODIES.

THE PREFACE.

HAVING at the beginning of the treatife, whereof this is a continuation, acquainted my readers with feveral things, that belong, in common, as well to the following experiments, as to thofe there publifhed; it will not be neceffary for me to trouble the reader with a repetition of what he may have met with there already, nor to acquaint him in this addrefs with any other particulars, than thofe that concern the experiments I am now about to prefent him.

I doubt not but it will be remembered by fome, that I feemed in the above-mentioned book to have promifed a fecond part of it, or a large appendix to it: but intimations of that kind do many times refpect only the thing itfelf, leaving the giver of them free in point of time: and I wanted not fufficient inducements to delay a while to perform my promife, if I made any. I had, indeed, partly before the book already referred to came from the prefs, and partly fometime after, made divers other trials, in order to a fupplement of it: but being obliged to make fome journeys and removes, which allowed me no opportunity to profecute the experiments, I had made no very great progrefs in my defign, before the convening of an illuftrious affembly of virtuofi, which has fince made itfelf fufficiently known under the title of THE ROYAL SOCIETY. And having then thought fit to make a prefent, to perfons fo like to employ it well, of the great engine, I had till then made ufe of in the phyfico-mechanical experiments about the air; and being unable afterwards to procure another fo good, I applied my ftudies to other fubjects, and gave over, for a great while, the care of making more experiments of that kind: and the rather, becaufe that finding by the very favourable reception thofe I had publifhed had met with among the curious in feveral parts of *Europe*, that they were like to be confidered and perufed, I thought I might fafely leave the profecution of them to others, who would probably come more frefh and untired to fuch an exercife of their curiofity.

BUT, obferving that the great difficulties men met with in making an engine, that would exhauft and keep out a body fo fubtle as air, and fo ponderous as the atmofphere, (befides, perhaps, fome other impediments) were fuch, that in five or fix year I could hear but of one or two engines, that were brought to be fit to work, and of but one or two new experiments, that had been added by the ingenious owners of them; I began to listen to the persuasions of those that suggested, that unless I resumed this work myself, there would scarce be much done in it. And therefore having (by the help of other workmen than those I had unsuccessfully employed before) procured a new engine, less than the other, and differing in some circumstances from it, we did (though not without trouble enough) bring it to work as well as the other, and, as to some purposes, better. And having once got this, I made haste to try with it those experiments, that belong'd to the designed continuation, and do now make up this book.

I hope, that to such readers as the following papers are principally intended for, I shall not need to make an apology, either for the plainness of my style, (wherein I aimed at perspicuity, not eloquence,) or for my not having adorned or stuffed this treatise with authorities, or sentences of classic authors, which I had neither the leisure to seek, nor thought I had any great need to employ, though it had been far more easy, than perhaps it would have proved, to borrow from them things, that would have been very proper to a treatise, where my main design was, to make out, by practical experiments, divers things, among others, that have not hitherto been advantaged by that way of probation, nor perchance thought very capable of it; so that I shall have obtained a great part of what I aimed at, if I have shewn, that those very phænomena, which the school-philosophers, and their party, urge, and sometimes triumph in, as clear proofs of nature's abhorrency of a vacuum, may be not only explicated, but actually exhibited, some by the gravity, and some also by the bare spring of the air. Which latter I now mention as a distinct thing from the other; not that I think it is actually separated in these trials, (since the weight of the upper parts of the air does, if I may so speak, bend the springs of the lower) but because, that having in the already published experiments, and even in some of these, manifested the efficacy of the air's gravitation on bodies, I thought fit to make it my task in many of these, to shew, that most of the same things, that are done by the pressure of all the superincumbent atmosphere acting as a weight, may be likewise performed by the pressure of a small portion of air, included indeed (but without any new compression) acting as a spring.

The present first part of our continuation might, I confess, have been, not inconveniently, divided into two parts. For first, it contains some experiments, that are already related in the printed book, though they be here so repeated, as to be confirmed, illustrated, or improved, by being reiterated either with better instruments, or with better success, than when they were made in my large receiver, which holding (if I misremember not) about eight gallons, could not easily be so well exhausted as those small receivers I often since employed. And secondly, the other, and far more numerous sort of experiments, related in this first part, are new, and superadded. And yet I forbear to assign each of these two sorts a place by itself, because I could not conveniently set down my trials otherwise than as they came to hand among my notes; and I considered, that in divers places the new ones and the old ones being mentioned together, might serve by their neighbourhood to illustrate or confirm each other. And however, at another edition of our Continuation, it will be a very easy task, if it appear to be a requisite one, to give the improvements of the former experiments, and the superadded new ones, distinct titles and places.

As for the mechanical contrivances I employed in making the following experiments, though most of them have had the good fortune to meet with an approbation, and some of them with more than that, from no mean virtuosi and mathematicians; yet as I expect, that critical readers will judge, that in some experiments more artificial instruments might have been made use of, so I hope that they will not look upon those I was reduced to employ, as always the best that ever I could have directed, since it sufficiently appears by divers passages of the following experiments, that they were not made at *London*, but in places where the want of a glass-house, and other accommodations reduced me to make my trials not after the best manner I could devise, but in the best way I could then and there put in practice. And let me add on this occasion to what I have elsewhere said to the like purpose, that it is both a great discouragement to many ingenious men, and no small hindrance to the advancement of natural philosophy, that some nice criticks are so censorious in exacting from attempters the very best contrivances, and many, that would be attempters, stand too much in awe of such mens judgments; for though in very nice experiments the exactness of instruments is not only desireable and useful, but, in some cases, necessary; yet in many others, where the production of a new phænomenon is the thing aimed at, they are to be looked upon as benefactors to the history of nature, that perform the substantial part of a discovery, though they do it not by the most easy and compendious ways deviseable, or attain not to the utmost preciseness, that might be wished, and is possible. For such performances, notwithstanding their being short of perfection, make discoveries to the world of new and useful things; which though others, that are more lucky at contrivances, and have better accommodations, may compass by more compendious ways, or with greater preciseness, yet still the world is beholden to the first discovery for the improvement of it, as we are to *Archimedes* for the first devising a way to find, by weighing bodies in water, how much gold or how much silver a mixture of those metals does contain, though (if historians have not injured that great man in the relation) he went a more laborious and less accurate way to work than modern hydrostatians, who (as I elsewhere shew) may perform the same thing by a far better way, which yet, probably, we should not have thought of, if that attributed to *Archimedes* had not preceded, and afforded us a fundamental notion. And that the not being so dexterous at contriving the ways to effect a thing, is no sure argument, that a man has not a true and solid knowledge of it, we may easily learn from *Euclid*, whom our geometricians generally and justly acknowledge to be their master, and to have enriched the world with many useful truths, and solidly demonstrated all his propositions, though divers of his modern commentators have found out more compendious ways for effecting several of his problems, as well as of demonstrating divers of his theorems, especially since the excellent invention of specious algebra, by whose help that accurate mathematician Dr. *Wallis* has, besides other specimens upon intricate propositions, clearly demonstrated the ten first and for the most part perplexing theorems of the second element, in little more than as few lines. In sum, in experiments that are very nice, accurate contrivances and instruments are industriously to be sought, and highly to be valued; and even in such other experiments, as are frequently to be reiterated, the most commodious and easy ways of performing them are very desirable: but those practical compendiums, though very welcome to them that would repeat trials, are not so important to the generality of readers, as being but useful to save pains, not necessary to discover truths; to which men may oftentimes do good service, without any peculiar gift at mechanical contrivances, since in most cases they may be looked upon as promoters of natural philosophy, who devise experiments fit to discover a new truth, if the attempt succeeds, and propose ways of bringing it to trial, which though perhaps not the most skilful or expeditious, are yet sufficient and practicable, the increase of physical knowledge being the product of the things themselves that are discovered, whatever were the instruments men employed about making the discoveries.

As for the cuts, I endeavoured to make their relations, and descriptions of most of the experiments, so full and plain, as to need a few schemes as might be to illustrate them: but though I hope, that they, who either were versed in such kind of studies, or have any peculiar facility of imagining, would well enough conceive my meaning only by words; yet lest my own accustomance to devise such trials, and to see these made, should make me think them more easily intelligible than most readers will find them, I advised with a learned friend or two, fit to be consulted on such an occasion, what experiments were requisite to be illustrated with diagrams, and to such I took care they should be annexed. Only I forbore to add to the figure of each instrument alphabetical explications of its parts, as judging that troublesome work less easy for me, than it would be for such readers as this tract is designed for, to understand what is delivered by the help of a little attention in conferring the schemes of the instruments with the verbal accounts of the experiments they relate to. But there is one particular about the cuts may require both to be given notice of and excused; which is, that having occasion to alter the method of my experiments, when I began to foresee, that I should be obliged to reserve divers things for another opportunity; and being myself absent from the engraver for a good part of the time he was at work, some of the cuts were misplaced, and not graven in the plates, in which, according to the present series of experiments, they might most properly have been put.

But perhaps I may (for I am not sure of it) more need the reader's pardon for (unknowingly) troubling him in this continuation with some passages, that he may have already met with in the book it refers to: which though I had not read over for some years before, I chanced not to have at hand, when divers of the following papers were written; and though afterwards I recovered it, yet the indisposition of my eyes made me think it unfit rather to tire them by reading over the whole book, than to trust to the reader's good nature (in case I should need it) for the pardon of a few unintended repetitions.

I doubt not, many readers will be inquisitive to know, why this treatise is stiled the first part of a Continuation. To give these some account of the title, I must put them in mind, that in the already published experiments I intimated, that two sorts of trials might be made by the help of our engine: the one, such as needed but a short absence of the air, and the other such as required, that the air should not only be withdrawn for a while, but kept out for a considerable time, from the bodies whereupon the trial is made. Of the former sort of experiments are these this present book does (as well as that heretofore published did) consist of. And though I have been so much called upon, and troubled for certain writings, whereof I have made such mention in those that passed the press, as some readers interpreted to be an engagement, that it made me think fit, when I satisfied their demands, to be thenceforward very shy of making the publick any promise; yet I was induced not to alter the title of this treatise, partly because it may intimate to the curious, that there are yet a great many things to be performed by our engine, besides the productions of it I have hitherto presented them; and partly because, though I still persist in my former averseness to make promises to the world; yet it is very possible, that if God grant me life and health, I may, in due time, present my friends with what may serve for a second part of our Continuation, consisting of experiments, that require a longer absence of the air from the bodies to be wrought upon: and I shall think, if this first part prove not unexceptable to the curious, that the latter will be not unwelcome to them, as being designed to consist of sets of experiments, which by their being most of them new, and some of them odd enough, may perchance afford some not despicable hints to the speculative. But the very nature of these experiments requiring, that some of them should be long in making, my friends could not reasonably expect a quick dispatch of work of this kind, though I should not meet for the future with such intervening impediments, as have hitherto diſturbed it, (as want of inſtruments, of health, of leiſure, and of the liberty, which is ſo requiſite in this caſe, of ſtaying long enough in one place:) notwithſtanding all which difficulties I have by ſnatches been able through God's bleſſing to make forty or fifty of deſigned trials, being ſuch as require the leaſt of time to be performed in, though I now think not fit to mention any of them, as well for other reaſons, as becauſe though they be made by the help of our engine, yet they require a peculiar apparatus of inſtruments, very differing from thoſe we have hitherto mentioned, and not to be intelligibly deſcribed without many words and divers figures. In the mean time, leſt the induſtrious ſhould be diſcouraged by a ſurmiſe, that there is nothing left for them to do by the help of our engine, at leaſt as to the firſt ſort of experiments, I ſhall inform them, that I had thoughts to have added divers others of that kind to theſe that now come forth, and particularly two cluſters of pneumatical trials, the one about reſpiration, and the other about fire and flames; but ſeveral of my notes and obſervations being at preſent out of the way, my having neither health nor leiſure to repair theſe inconveniencies, and proſecute trials of that ſort with any aſſiduity, makes me chuſe rather to reſerve them for an appendix, than to make thoſe that now come abroad ſtay for them. Which will not, I preſume, be the more diſliked, becauſe by taking this courſe I may, in delivering of the phænomena of nature, imitate nature herſelf, of whom it is the Roman philoſopher's ſaying, *rerum natura ſacra ſua non ſimul tradit*.

Seneca quæſt nat. lib. 7. c. 31.

Some ADVERTISEMENTS touching the ENGINE itſelf.

THOUGH the engine already publiſhed, and that which I employed in the following trials, have the ſame uſes, and agree both in the ground and the main part of their conſtruction, yet they differ in ſome particulars fit to be taken notice of: for after I had preſented the great engine I formerly made uſe of to the Royal Society, partly the difficulty of procuring ſuch another of that ſize and make, and partly the deſire of making ſome improvements invited me to make ſome alterations in the ſtructure; ſome of them ſuggeſted by others, (eſpecially by the ingenious Mr. *Hook*,) and ſome of them that I added myſelf, as finding that without them I could not do my work. Wherefore it will not be amiſs to point at the chief differences between the former and the latter engine, and to intimate ſome of the conveniences and inconveniences that attend them.

As for the conſtruction of the ſecond engine itſelf, ſince it is preſumed, that the readers of this book have already peruſed that, of which this is a Continuation, and underſtood the contrivance of the inſtrument, that belongs to it, it was preſumed ſufficient to exhibit in the firſt plate the delineation of the entire engine ready to be ſet at work; and in the ſecond, the figures of the ſeveral metalline parts, that compoſe it, before they are ſet together. For though theſe have not verbal and alphabetical explications annexed to them, yet the ſight of them may ſuffice to make thoſe, that have an imagination fitted to conceive mechanical contrivances, and are acquainted with the former engine, comprehend the ſtructure of this; which, alphabetical explications would ſcarce make ſuch readers do, as are not ſo qualified: only two things there are, which being of ſome difficulty, as well as of importance to be conceived, I ſhall here particularly take notice of. The firſt of which is, that in regard the ſucker is to be always under water, and the perforation *p q*, that paſſes perpendicularly quite through it, and ſerves together with the ſtick *r s* for a valve, is to be ſtopt at the bottom of the cylinder, as at *n o*, when it is full of water, it was requiſite to make the ſtick *r p* of a conſiderable length, as two or three foot. The other and chief thing is, that in the ſecond plate, the pipe *A B*, whoſe end *B* bends upward, is made to lie in a groove or gutter purpoſely made in the flat wooden board *c d e f*, on which the receivers are to reſt; which ſquare board I cauſed to overlaid with very good cement, on which I took care to apply a ſtrong plate of iron, of the bigneſs and ſhape of the board, leaving only a ſmall hole for the erected part of the pipe to come out at, which I added, not only to keep the wooden board the better from warping, but becauſe I knew (what will perhaps be thought ſtrange) that the preſſure of the atmoſphere on one ſide of the board, when there is no preſſure or but very little on the other, will enable many aerial particles to ſtrain through the very wood, though of a good thickneſs, and imbued with oil to choak the pores. To this iron-plate we ſometimes fit a lip turning up about it, to hinder the water, that on ſome occaſions will come from the receiver, from falling on the room; and (to add that upon the by) though the ſtop-cock *g h i k*, that belongs to the hitherto mentioned pipe, may be inſerted at *I*, into the barrel or cylinder *l m n o*, by the help of ſoder, yet we choſe as a much better way to have the branch *I*, of the ſtop-cock, made like a ſcrew, which being once firmly ſcrewed into the barrel, is not apt to be broken off, and may be more eaſily mended, if any thing happen to be out of order, which the engine is the moſt liable to be in or about the pipe; partly becauſe it may fall out, (though but very rarely, if due care be but taken) that the air will inſinuate itſelf between the wooden board and the iron-plate, and ſo get up (where the pipe bends upwards) into the cavity of the receiver; and partly becauſe the pipe being for a juſt reaſon made but ſlender, and the part of it that looks upward very ſhort, it happens not very unfrequently, that when we imploy receivers with narrow orifices, where the cement muſt lie cloſe to the opening of the pipe, it happens, I ſay,

ſay, that the cement, eſpecially if it be much ſoftned by heat, is ſucked (as they ſpeak) into the pipe, and ſo choaks it up; or elſe that ſome part of the body included in the receiver is drawn to the orifice of the pipe, and lying upon it as a cover hinders the free paſſage of the air into the barrel; againſt which inconvenience, to add that upon the by, we uſe amongſt other expedients to place juſt about the orifice of the pipe a ſmall cover of tin, like that of a little box, which covers it at the top to hinder any thing from lying immediately upon the pipe, and has a ſmall opening or two in the ſide, to give the air of the receiver free acceſs to the pipe.

THE ſquare and hollow wooden part of this engine, diſcernable in the firſt plate, is ſo made, that it may contain not only the cylinder, but ſo much water, as will always keep the cylinder quite covered with that liquor; by which means the ſucker, lying and playing always under water, is kept ſtill turgid and plump, and the water being ready at hand to fill up any little interval or chink, that may happen to be between the ſucker and the inſide of the barrel, does, together with the newly mentioned plumpneſs of the ſucker, very much conduce to the exact keeping out of the air. But this advantage is not without ſome inconvenience; for divers times, if great care be not taken in turning the ſtop-cock, the water will be impell'd into the receiver, and much prejudice ſundry experiments, when the included bodies are ſuch that may be ſpoiled or impaired (at leaſt for the preſent) by that liquor. The ſmalneſs of our cylinder is a convenience in regard of the facility it affords to make and diſpatch thoſe many experiments, that may be performed in ſmall receivers, though it make thoſe more troubleſome and tedious, that require the exhauſtion of large and capacious ones.

THE flat plate (mentioned a little above) has this great conveniency in many experiments, that the receiver needs no ſtop-cock of its own; for ſuch a veſſel being made all of an entire piece of glaſs, and whelmed on upon the plate well covered with cement, can better keep out the air, than if there were a ſtop-cock, at which the air does but too frequently get in: but beſides that in divers experiments ſuch receivers do uſually require to be wide mouthed, whereby a greater compaſs is to be fenced againſt the ingreſs of the air, ſeveral experiments cannot ſo conveniently be tryed in this ſort of receivers.

BUT becauſe, that though this ſecond form of our engine hath as to ſeveral purpoſes its peculiar conveniencies and advantages, yet ſome virtuoſi may be furniſhed with the other already, and ſome may conceive it the more clearly of the two, or may judge it preferable for their particular deſigns; I ſhall here intimate, that for moſt of the experiments, if not all, that follow in this treatiſe, they may make uſe of, or at leaſt make a ſhift with the firſt engine, with a few alterations; whereof the chief is to be this, that to the upper part of the great cylinder, on the ſide oppoſite to the iron-rack, there is to be faſtned ſuch a ſquare board, and ſuitable iron-plate, as is uſed in the ſecond engine, betwixt which board and plate is to be lodged ſuch a pipe as was lately deſcribed, being either a continuation of the outward branch of the ſtop-cock, or elſe firmly faſtned to it by ſodering or ſcrewing: for by this means, when the ſucker is depreſſed, the air will through the cavity of this pipe, and the ſtop-cock whereto it is annexed, paſs freely, by virtue of its own ſpring, out of the receiver into the exhauſted cylinder; though this, and the ſucker that moves in it, being not kept, as in the ſecond form of the engine, under water, the greater care will be needed to keep the air from inſinuating itſelf between them. A good cement, to faſten the receivers to the often mentioned plate of iron, is a thing of no ſmall moment in making the following experiments, of which we imploy differing compoſitions for differing purpoſes, ſome of which are not neceſſary to be mentioned in that part of this work, that now comes forth; but that, which in almoſt all the following trials we chiefly make uſe of, is a well wrought mixture of yellow bees wax and turpentine, which compoſition, as it ſerves better than moſt others to keep out the air, ſo it has the conveniency, which is no ſmall one, of ſeldom needing to be heated, and ſeldomer to be much ſo; eſpecially if we imply a little more turpentine in winter than in ſummer, in the former of which ſeaſons, as much, or very near as much of that ingredient as of the wax does well, for as in ſummer a mixture of three parts of wax to about two of turpentine is more proper.

A CONTINUATION OF NEW EXPERIMENTS PHYSICO-MECHANICAL, TOUCHING THE SPRING and WEIGHT of the AIR, and their EFFECTS.

My Dear Lord,

SINCE I have already in proper places of the physico-mechanical experiments about the air, which I formerly presented your lordship, given you a sufficient account of several things touching the scope, occasion, &c. of my attempt; it will not be necessary to make a solemn preface to the ensuing experiments. And therefore presuming upon an acceptance, which the favourable entertainment, which your lordship, as well as the publick, was pleased to give my first trials of this kind, encourages me to expect, I shall, without troubling you with any further preface, immediately fall upon a continuation; especially since your lordship will perhaps wonder, that you have not received it much sooner, as, indeed, you should have done, if I had been befriended with accommodations and leisure.

EXPERIMENT I.

About the raising of mercury to a great height in an open tube, by the spring of a little included air.

DIVERS ways have been proposed to shew both the pressure of the air, as the atmosphere is a heavy body, and that the air, especially when compress'd by outward force, has a spring, that enables it to sustain or resist a pressure equal to that of as much of the atmosphere, as can come to bear against it; and also to shew, that such air as we live in, and is not condensed by any human or adventitious force, has not only a resisting spring, but an active spring (if I may so speak) in some measure, as when it distends a flaccid or breaks a full-blown bladder in our exhausted receiver.

But observing, that there seems to want a visible experiment to convince those, that are not so easily satisfied with reasons, though drawn by just consequence from physical or mechanical truths, or even from other experiments; taking notice, I say, hereof, I made the following experiments; not so much to prevent or remove a scruple no better grounded, as to have a new way of making an estimate by some known and determinate measure of the force of the bare spring of the air, both in its natural state, (as it is said to be, when not compressed nor rarified, more than the free air we breath,) and according to its several degrees of expansion.

We took then a vial, with a neck not very large; and having filled about a fourth part of it with quick-silver, we so erected and fastened a long and slender pipe of glass, open at both ends in the neck of the vial, with hard sealing-wax, that the lower end reached almost to the bottom of the quick-silver, and the upper more than a yard above the vial. Then having blown in a little air, to try, whether the instrument did not leak, (which it is very difficult to keep such instruments from doing,) we conveyed it into a long and slender receiver, fit for such an use; and having withdrawn the air as well as we could, we found, according to our expectation, that the spring of the air, included in the viol, impelled up the quick-silver into the erected pipe, to the height of 27 inches; and having suffered the external air to return into the receiver, the quick-silver subsided in the tube, sometimes almost, and sometimes quite as low as the stagnant quick-silver in the vial. See Plate III. Fig. 1.

For the better illustration of this experiment, thus summarily related, but with the like success, as to the main, several times repeated, we will subjoin the following observations and notes.

I. That we tried this experiment several times, and the last time in the presence of the famous Savilian geometer, Dr. *Wallis*, who saw the quick-silver in the pipe impelled up to 27 inches, being one himself of the measurers. And though at other times we found it to be much about the same height with the last, yet once it seemed plainly to be a pretty deal higher; which yet we specified not, because a mischance took off the mark, which we had made to measure the height by.

II. Having once, to try the stanchness of the vial, blown in so much air, (without taking out any thing as we use to do in the like case) that the air in the cavity of the viol raised and kept the quick-silver 3 inches high in the pipe, when we went on with the rest of the experiment, according to the way above described, we found, by emptying the receiver of air, that we were able to raise the quick-silver in the cane 30 inches, or somewhat more above that in the vial.

III. SOMETIMES it may happen, that the mercury, when taken very soon out of the receiver, will not appear to have subsided to its first lowness, which perhaps it will not sink to in some while after: which is not to be wondered at, since in such a receiver, which contains but little air, the heat of the cement and the iron, imployed to melt it quite round the receiver, may impart a little warmth to the air in the vial, which will after return to its former temper. But this accident is neither constant nor necessary to the experiment.

IV. IT is very remarkable, that if the receiver be fitly stopped, and slender enough; upon the turning of the stop-cock, to let out the air at the first exsuction, the mercury will be impelled up by the spring of the air in the viol, suddenly flying abroad or stretching itself, so that it will be raised several inches above the height it will rest at afterwards, and will make several vibrations up and down before it come to settle, just as the mercury does in the Torricellian experiment, (the bare pressure of the little air doing here to the mercury what the weight of the atmosphere does there,) and such motions of the mercury will be made four or five subsequent exsuctions, upon the withdrawing of the air in the receiver. But as these grow lesser and lesser, as the spring of the included air grows fainter, so none of them is any thing near so considerable as the vibrations made upon upon the first suck.

V. AGREEABLE hereunto we observe, that at the first exsuction, when the spring of the included air was yet strong, the mercury would be raised by our estimate above half, if not $\frac{2}{3}$ of the whole height, whereto it will at length be brought, (though that must be according to the bigness of the receiver, and other circumstances,) and the subsequent exsuctions do still add less and less proportions of height to the mercurial cylinder, and that for two reasons: the one, because the more there is of mercury impelled into the tube, the greater weight of mercury presses upon the included air: and the other, because the air has so much the more room in the vial to expand itself, whereby its spring must be proportionably weakened.

LASTLY, when we made most of these trials, I had the curiosity to observe the height of the mercury in a good barometer; and thereby found, that the air was then but light; its greatest height reaching but to 29 inches, and $\frac{3}{8}$, and its height soon after the trial, whereof Dr. *Wallis* was a witness, amounting but to 29 inches.

TO make an estimate of the quantity of air, that had raised the quick-silver to 27 inches, we took the vial, that was imployed about this experiment; and having counterpoised it, whilst it was empty, we afterwards filled it with water, and found the liquor to weigh 5 ounces, 2 drams, and about 20 grains; and then having poured out the water, till it was sunk to a mark, which we had made on the outside of the glass, to take notice how high the quick-silver reached, that we poured in: and lastly, weighing the remaining water, equal in bulk to the quick-silver, we found it to amount to 1 ounce, 2 drams, 14 grains; so that the air, that had raised up the mercury, possessed (before its expansion) in the vial the place but of 4 ounces, and a few odd grains, i. e. of about $\frac{1}{4}$ of a pint of water. And as for the pipe also, imployed about the same experiment, we found its cavity to have about $\frac{1}{8}$ part of an inch in diameter.

IT was one of the uses I hoped to make of this experiment, that by comparing the several degees of expansion of air included in the vial with the respective and increasing heights of the mercury, that was impelled up into the pipe, some estimate might be made of the force of the spring of the air weakened by several degrees of dilatation; but for want of conveniencies I forbore to venture upon such nice observations, especially because the pressure of the dilated air, that remains in the receiver, and is external to the air included in the vial, must also be taken into consideration.

ANOTHER use of our experiment may be this; that it may supply us with a considerable argument against some learned men, who attribute the suspension of the quick-silver in the Torricellian experiment to a certain rarified matter, which some call a funiculus, and whereto others give other names; which rarified substance they suppose to draw up and sustain the quick-silver, in compliance of nature's abhorrency of a vacuum. For in the experiment under consideration, the quick-silver being not only sustained at the height of 27 inches in the tube, but elevated thither; if the cause of this be demanded, it will be answered, according to their hypothesis, that the air in the receiver, external to that of the vial, being, by reason of the sucking out of some of it by the pump, more rarified than that in the vial, it draws up to it the quick-silver in the cane, and the more it is rarified, the higher it is enabled to draw it. But then I demand, whence it comes to pass, that though we can, by persevering to pump, more and more rarify the little remaining air, or the aereal substance in the receiver, that in the vial not appearing to be also rarified, yet the air in the receiver does not by virtue of its superadded rarefaction, whereby it exceeds that of the air in the vial, pull up the quick-silver to a greater height in the tube than 27 inches: for, that this is not the greatest height, to which mercury may be raised by this rarified substance, our adversaries must not deny, whotell us, that in the Torricellian experiment it sustains a mercurial cylinder of 29 inches, and $\frac{1}{2}$, and can raise a cylinder of 29 inches to 29 $\frac{1}{2}$, or higher, in case that the cylinder be made to vibrate up and down in the tube.

AND as for those, that will in such cases, as our experiment suggests, have recourse only to that which they call the *fuga vacui*, they may please also to consider, that since the quick-silver remains the same, its ascension in the tube will not be available for what they think to be nature's purpose; for, whether it reach higher or lower in the tube, it will adæ- See the latter part of the following Experiment.

quately

quately fill no more ſpace in one poſture, or in one figure, than in another, in what part ſoever of the cavity of the receiver it be placed.

EXPERIMENT II.

Shewing, that much included air raiſed mercury in an open Tube, no higher than the weight of the atmoſphere may in a baroſcope.

IN the former experiment, by reaſon of the ſmalneſs of the vial, that was employed about it, there was ſo little air included, that the expanſion of it, ſo far as was requiſite to impell up the mercury in the pipe to the above mentioned height of 27 inches, may be probably ſuſpected to have very much weakened its ſpring, and therefore it may be thought, that (eſpecially conſidering the great force, that ſeveral of our experiments manifeſt impriſoned air to have,) if there were a greater quantity of air included in the veſſel, ſo that the expanſion, ſufficient to raiſe the mercury to the former height, would not need to be conſiderable, (becauſe that the capacity of the tube being but the ſame, the whole included air will be ſo much the leſs expanded, by how much the more of it there is,) it ſeemed probable, that the ſpring of the air, being but a little weakened by ſo ſmall a dilatation, would remain ſtrong enough to raiſe a much taller cylinder of mercury in the tube, and perhaps make the liquor run over into the receiver.

But though this ſuggeſtion ſeem probable enough, yet when I conſidered, that the weight of the atmoſphere is able to ſuſtain a cylinder of quick-ſilver but of 30 inches, or thereabouts (in perpendicular height) and conſequently, that the preſſure of ſuch a mercurial cylinder is equivalent to that of an atmoſpherical cylinder of the ſame bore; it was not difficult to conclude, that ſince the air in a vial, before the mouth is cloſed, has a ſpring but equal in ſtrength to the weight of the atmoſpherical pillar that leans upon it, (for if the ſpring were too ſtrong for the weight that leans on it, ſome of the air would get out of the vial) a greater vial, and conſequently a greater quantity of included air would not be able by its ſpring to elevate and ſuſtain a longer cylinder of mercury, than the weight of the atmoſphere is able to do; nor indeed altogether ſo much, becauſe of ſome little (though but little) diminution of the ſpring by ſome (though but a ſmall) expanſion, that the included air ſuffers, by ſucceeding in the place of mercury, that is impelled up.

TO clear therefore this matter by an experiment, we took a ſtrong glaſs-bottle, capable of holding about a quart of liquor; and having put into it a convenient quantity of quick-ſilver, we erected in it a very long and ſlender pipe of glaſs, open at both the ends, and reaching at the lower end beneath the ſurface of the ſtagnant mercury; and having faſtened this pipe in the neck of the bottle, by choaking up that neck very accurately with good cement, that none of the included air might be able to get out, we conveyed the whole into a receiver, like that imployed about the firſt experiment in ſhape, but much larger, that it might be able to contain ſo great a veſſel; and then the engine being ſet at work, we quickly raiſed the quick-ſilver to a greater height than formerly; and when we ſaw it come to a ſtand, we did by the help of ſome marks, made before-hand on the pipe, and by the help of a very long and well divided ruler, meaſure, with as much care and accurateneſs as the figure of the veſſels would allow us to do, the height of the mercurial cylinder, which we found to be 29 inches, and about $\frac{7}{8}$, to which abating half an inch, which was raiſed, before the pump was employed, by ſome air, that had been blowed into the bottle, to try whether it were ſtanch; deducting, I ſay, this half inch of quick-ſilver, which remained in the tube after the external air was let in, (as well as it had been there before the receiver was exhauſted,) out of the newly mentioned number there remained 29 inches, and near $\frac{3}{8}$, for the height of the mercury, raiſed by the ſpring of the air, ſhut up in the bottle; and then conſulting with the above-mentioned baroſcope, which ſtood in a window in another part of the houſe, I found, that the weight of the atmoſphere did bear a mercurial cylinder of about 29 inches and $\frac{3}{4}$, which was higher by $\frac{3}{8}$ than that to which the ſpring had raiſed the quick-ſilver in the exhauſted receiver: and the difference perhaps would have been greater, if the place, where the experiment was made, had not by its warmth added ſome little matter to the ſpring of the air; and if alſo we could have kept the mercury ſo long elevated, as to give it leave to diſcharge its ſelf of thoſe ſmall bubbles, which it is almoſt impoſſible in ſuch experiments as this to free quick-ſilver from, without ſome help from time.

LASTLY, though we cauſed the pump to be plied, to try whether we could not, by the more diligent exſuction of the receiver, raiſe the quick-ſilver above the height of that, which thea tmoſphere kept ſuſtained in the baroſcope; yet our labour gave us but a confirmation, that the ſpring of the air would not raiſe the mercury higher, than did the weight of the atmoſphere, which may not a little confirm the ſecond obſervation.

N.B. THIS was not the only nor the firſt experiment we made of this kind; but this being carried on without miſchances, (with which divers others were attended,) and made with much care, I thought fit to ſet down this inſtead of all, intimating generally about the reſt, that they ſeemed to agree well for the main with that, which is here recited. Only there is one thing relating to thoſe other experiments, that ſeems not altogether unworthy to be taken notice of; which is, that when our trials were made in veſſels, that contained a conſiderable quantity of air, though upon the exhauſtion of the receiver, the ſpring of the included air could not raiſe the quick-ſilver to the top of the pipe, yet ſometimes by other effects it manifeſted itſelf to be very ſtrong, as once or twice by the blowing out or breaking the cork

or

or cement, and other matter that was imployed to ſtop the glaſs it was ſhut in; and once by an accident too memorable to be here paſſed over in ſilence.

I had one day invited Dr. *Wallis* to ſee ſuch an experiment as I have been relating, made with (not a vial, but) a bottle of green glaſs, (ſuch as we uſe now for wine,) and four or five pounds of mercury. After this learned perſon and I had continued ſpectators as long as we thought fit, we withdrew into another room, where we had not ſat long by the fire, before we were ſurprized by a ſudden noiſe, which the perſon, that occaſioned it, preſently came running in to give us an account of, by which it appeared, that this ingenious young man, (whom I often imploy about pneumatical experiments, and whom I mentioned to your Lordſhip, becauſe *J. M.* has the honour to be ſomewhat known to you,) being deſirous in our abſence to ſatisfy the curioſity he had to know, whether the quick-ſilver could not be raiſed higher in the pipe than I had foretold, plyed the pump ſo obſtinately, that at length the bottle being not, it ſeems, every where equally ſtrong, the impriſoned air found it more difficult to make the quickſilver run over at the top of the pipe, than to break the bottle in the weakeſt place; and accordingly did not only throw off a piece of the bottle, but threw it with ſuch violence againſt the large and ſtrong receiver, as broke that alſo, and rendered it unſerviceable for the future. But the doctor and I laying together the pipe, which happened to be broken into but few pieees, concluded by the place, to which we were told it reached when this accident happened, that it had not exceeded, nor indeed fully equalled the height, to which the weight of the atmoſphere might have raiſed it.

EXPERIMENT III.

Shewing that the ſpring of the included air will raiſe mercury to almoſt equal heights in very unequal tubes.

HAVING ſhown in the two former experiments, that the active ſtrength of the air's ſpring is very conſiderable, I thought good alſo to examine, whether or no to the other reſemblances in operation between the weight of the free air, and the preſſure of the included air, this alſo may be added, that as the gravitation of the atmoſphere is able (as we ſhall hereafter prove) to ſuſtain the mercury at the ſame height in leſſer and greater tubes, ſealed at the top; ſo the preſſure of the included air may be able to ſuſtain the mercury at the ſame height in ſlenderer and in larger tubes, though in the latter it muſt ſuſtain a far greater weight of mercury than in the former; provided allowance be made for the weakening, which the ſpring of the included air muſt be ſubject to, by reaſon that, to ſucceed in the place of a large cylinder of mercury impelled up into the greater tube, it muſt expand it ſelf more, and conſequently have its ſpring more weakened, than if the tube were ſlender.

TO proſecute this experiment, I thought on a peculiar ſhape of veſſels, which, if I had been where there is a glaſs-houſe, I would have cauſed to be blown for the more convenient trying of two pipes of different bores at the ſame time. But though I wanted this accommodation, I thought I might well enough ſhew what I intended by imploying ſucceſſively two tubes of very differing ſizes, provided the veſſel for the including of the air were the ſame.

WHEREFORE taking the glaſs bottle, made uſe of to try the former experiment, and erecting in it after the manner above deſcribed a cylindrical pipe of glaſs, a good deal larger than the former, (if not as large again) we proſecuted the experiment as we had made it, with the ſlender tube above mentioned, and found, that we were able, by the ſpring of the air in the bottle, to raiſe the quick-ſilver to a conſiderable height, which, meaſuring as well as the veſſel would allow us, was, by the leaſt eſtimate that was made of it, (which was mine) 28 inches, and $\frac{1}{8}$, by which it appeared to want ſomewhat above an inch of the height of the mercurial cylinder, which the weight of the atmoſphere could have ſuſtained, as appear'd by the barometer, wherein the quickſilver at that time was about 29 inches, and $\frac{1}{4}$ high; which difference was no more than I expected, conſidering, that, whereas the weight of the atmoſphere is ſtill the ſame, when the mercury is at its full height (and that whether the pipe be great or ſmall) in a ſealed tube; the ſpring of our included air muſt needs be weakened the larger the tube is, and the higher the liquid metal is impelled in it; ſo that it ſeemed a conſiderable phænomenon, that the ſpring of ſo little air ſhould be able to raiſe the mercury as high within an inch or thereabouts in a wider as in a ſlenderer tube, ſince the diameter of the cavity of the former being by our eſtimate double to that of the latter, (into which the ſlender pipe could eaſily be put as into a caſe too big for it:) the greater mercurial cylinder may be ſuppoſed to have weighed near four times as much as the leſſer; I ſay, near, becauſe there was an inch difference in their heights: but in caſe theſe had been equal, then the ſolidities of the cylinders would have been to one another as their baſes; and ſince theſe, being circular, are in duplicate proportion to their diameters, that is, as the ſquares of their diameters; it is plain, that if the diameters be as one to two, the ſquares of them muſt be as one to four; and theſe cylinders conſiſting of the ſame mercury, their weights will have the ſame proportions with their ſolidities, and conſequently would be as one to four, making the abatement formerly intimated for the inch and a little more of mercury, by which the larger cylinder came ſhort of the height of the former.

N.B. 1. This and the two former experiments tried by us with quick-ſilver may be alſo tried with water; but beſides that we could hardly procure tubes long enough for ſuch trials, we were not very ſollicitous about it: for if we attentively enough conſider, what has

has been already delivered, and the proportion in specifick gravity betwixt water and quick-silver, (whereof the latter is near 14 times as heavy, bulk for bulk, as the former) it will not be difficult to foresee the event of such experiments, which he, that has a mind to make, should be furnished not only with long tubes, but with capacious vessels to shut up the air in; else the air will be so far expanded before the water has attained near the height, to which the weight of the atmosphere may raise it, that the experiments will not seem to succeed near so well with water, as ours did with quick-silver.

2. We thought it worth trying, whether, when the included air had raised the great cylinder of mercury to the utmost height, it could elevate it to, by the spring it then had, it would not be brought to raise the quick-silver yet higher, if, notwithstanding the expansion it had already, there were an agitation made by the heated corpuscles of the same air. And in pursuance of this curiosity having caused an hot iron and a shovel of kindled coals to be held near the opposite parts of the receiver, we perceived after a while, that the mercury ascended ⅛ of an inch or better above the greatest height it had reached before. But conjecturing, that it would have risen higher, were it not, that whilst the application of the hot bodies was making, some particles of air had unperceivably stolen into the receiver, I caused the pump to be plied again to withdraw the air, I suspected to have got in, by which means the mercury was quickly raised ⅛ of an inch, or better, by virtue of this adventitious spring, (if I may so call it) which the included air acquired by heat; and I made no doubt, that it might have been raised much higher; but I was unwilling by applying a less moderate heat to hazard the breaking of my glasses, in the place I then was in, where such a mischance could scarce have been repaired.

EXPERIMENT IV.

About a new hydraulo-pneumatical fountain, made by the spring of uncompressed air.

I SHALL now add such an application of the principle, whereon the former experiment was grounded, as I should scarce think worth mentioning in this place, were it not, that besides that divers virtuosi seem not a little delighted with it, it may for ought I know prove to be of some philosophical use (to be pointed at hereafter.)

We took a glass-bottle with a convenient quantity of water in it, and fitted this bottle with a slender glass-pipe open at both ends, and about three foot long, which was so placed, that the lower orifice was a good way beneath the surface of the water, and the pipe it self passed perpendicularly upwards through the neck of the bottle, which neck was, by the pipe and by good hard cement imploy'd to fill the space betwixt the pipe and the inside, so well and firmly closed, that no water or air could get out of the bottle, nor no external air could get into it, but by passing through the pipe. This instrument was conveyed into a large receiver shaped like a pear, of which a good part of the blunt end, and a small part of the sharp end are cut off by sections parallel to the horizon, and consequently to one another. And because this receiver was not (nor ought to be) long enough to receive the whole pipe, there was cemented on to the upper part of it a smaller receiver of white glass, of such a length and bigness, that the upper end of the pipe might reach to the middle of its cavity, or thereabouts, and that the motions of the springing water might have a convenient scope, and so be the better taken notice of.

See Plate IV. Fig 2. This figure was designed only to make some representation of the difference, that would appear, if instead of making the fourth experiment with water, as in the foregoing figure, the trial was made with quicksilver.

This double receiver being cemented on to the engine, a little of the air was by one suck of the pump drawn out from it, by which the pressure of the remaining air being weakened, it was necessary, that since the air included in the bottle had not its spring likewise weakened, it should expand it self, and consequently impel up the water in the same bottle through the pipe, which it did so vigorously, as to make it strike briskly at first against that part of the top of the smaller receiver, which was just over the orifice of the pipe. But after it had a while made the water thus shoot up in a perpendicular line, as the spring of the air in the bottle grew by that air's dilatation to be weakened, the water would be impelled up less strongly and less directly, till the air in the bottle being as much expanded as that in the receiver, the ascent of the water would quite cease, unless by pumping a little more air out of the receiver we renewed it again.

About the making of this experiment these particulars may be noted.

I. It is convenient, that the upper part of the pipe be made (as it easily may be at the flame of a lamp) very slender, that the water having but a very small orifice to issue out at, may be spent but slowly, and thereby make the experiment last so much the longer.

II. You may, if you please, instead of making the upper part of the pipe slender, as was just now directed, cement on to it a top either of glass or brass, consisting of three or more very slender pipes, with a pin-hole at the end of each, that one of these pointing directly upwards, and the others to the right hand and to the left, the water may spin out several ways at once, by which kind of branched pipes we have sometimes imitated the *Jets d'eau* (as the French call them) and artificial fountains of gardens and grotto's.

III. In regard that so short a cylinder of water, as exceeded not the length of our glass pipe, could not make any considerable resistance to the expansion of the included air, it was thought and found safe enough to imploy instead of a strong glass-bottle a much larger vial, without being sollicitous about its shape, or that it should be very strong, and by this means we could make this pleasant spectacle last a great while, especially if we also made use of the expedient to be mentioned in the following note.

4. If you find, that the included air has by expanding it self too much weakened its spring,

ſpring, whilſt there yet remains with it a good quantity of water in the bottle or vial, you may reinforce the preſſure of the air by only turning the ſtop-cock, and letting in what air you think fit to the exhauſted receiver: for upon the admiſſion of this new air, the air in the receiver will preſs upon the water in the pipe, and having driven it into the bottle again, will follow it thither, till the air in the bottle and that in the receiver have attained an equal ſpring, and then by pumping out a convenient quantity of the air contained in the latter, the air ſhut up in the former will be able to impel up the water as before, till the ſtagnant liquor be depreſſed to the lower orifice of the pipe, at which, when the air of the bottle can get out, the courſe of the water upwards muſt ceaſe.

THE Uſes I made of this new hydraulo-pneumatical fountain (for in it I aim not only at a ludicrous experiment) were principally theſe.

THE firſt was to make it the more probable, that if we had had convenient veſſels, we might by the preſſure of the air included in the bottle have raiſed water about fourteen times as high as we did quick-ſilver in the former experiment, ſince upon but a little weakening of the preſſure of the air in the double receiver, the air in the bottle was able to impel the water forcibly enough, and for a pretty while, to the top of a pipe of about a yard long, and a good deal higher. (But this is but a ſlight uſe.)

THE next thing therefore we deſign'd to ſhew by this experiment was, that in thoſe hydraulo-pneumatical engines, where water is placed between two parcels of air, the water may be ſet a moving as well by the meer dilatation of one of the parcels of the air, as by giving a new force by heat or compreſſion to the other, and whether this mechanical principle of motion may hereafter prove not altogether uſeleſs in engines, we refer to further conſideration.

ANOTHER uſe we made of this experiment was to ſhew ſomewhat relating to the ſpring of the air, which may be worth conſidering, though we ſhall now but barely mention it. If then, when ſome of the air had been pumped out of the receiver, we removed that double veſſel from the bottle, the external air would by its weight haſtily depreſs the water in the pipe, till having driven it to the very bottom, it got up in numerous bubbles through the water, and joined it ſelf with the air incumbent on that liquor: but that which was here obſervable was, that all the external air that was able to get into the bottle, did not do it ſuddenly, but after the firſt irruption we could perceive, that from time to time there would new portions of air leiſurely inſinuate themſelves through the pipe into the bottle, and emerge through the ſtagnant water in bubbles, that ſucceeded one another ſo ſlowly, as to beget ſome wonder, as if the ſpring of the included air having been once put out of its wonted conſtitution by its late expanſion, could not be reduced to it but by degrees by the weight of the atmoſphere, which was ſtill the ſame: or, rather, as if between the ſpring of the included and the preſſure of the external air counterballancing each other, there happen'd ſome ſuch thing as is obſerv'd in an ordinary pair of ſcales, of which one is too much depreſſed, where the motion (which was ſwift enough at firſt) becomes ſo much the ſlower, by how much the weights come nearer to the æquilibrium, which their equality diſpoſes them to reſt in.

BUT the chief uſe deſigned in this experiment was, to obſerve, whether the lines, made by the water in its effluxions, would be of the ſame figure, notwithſtanding the rarifaction of the air in the upper part of the receiver, as if the air had not been at all rarified: and for this purpoſe it is beſt to make one's obſervations towards the latter end of the experiment, becauſe then the receiver being moſt exhauſted, and conſequently having the leaſt of air left in it, the difference made by the change of the denſity of the *medium*, in which the beams of water (if I may ſo call them) move, is like (in caſe there be any) to be beſt diſcerned. And this convenience we had by our way of experimenting, that we could take notice of the lines deſcribed by the ſalient water, as the ejaculation of that liquor grew ſtill fainter and fainter. But though I afterwards invited Dr. *Wallis* to favour me with his opinion about the curve lines of the ſalient water, yet for want of an upper receiver large enough, even he profeſſed himſelf (as I had done) not ſatiſfied about them. Only he ſometimes (as I alſo did) obſerved the ſalient water to deſcribe part of a line perfectly enough parabolical, with which ſort of curves he has been particularly converſant.

THIS made me reſolve for further ſatiſfaction to attempt by another contrivance, (of whoſe ſucceſs, if I can procure the implements I need, your lordſhip may expect an account) what the figures will be not only of ſalient water, but mercury, and other liquors; and that when the receiver is much better exhauſted, than it was neceſſary it ſhould be in the foregoing experiment.

EXPERIMENT V.

About a way of ſpeedily breaking flat glaſſes, by the weight of the atmoſphere.

FOR the more eaſy underſtanding of ſome of the ſubſequent trials, it will be requiſite in this place to mention, among experiments about the ſpring of the air, the following phænomenon belonging to its weight.

THIS is one of thoſe, that is the moſt uſually ſhewn to ſtrangers, as a plain and eaſy proof, both that the weight of the incumbent air is conſiderable, and that the round figure of a receiver doth much more conduce to make an exhauſted glaſs ſupport that weight, than if the upper part of the receiver were flat.

TO make this experiment we provided a hoop or ring of braſs of a conſiderable thickneſs, whoſe height was 2 ½, or 3 inches, and the diameter of whoſe cavity as well at the upper as lower orifice (ſhould have been juſt

3 inches, but through the error of the workman) was 3 inches and $\frac{1}{10}$. To this hoop we successively fastened with cement divers round pieces of glass, such as is used by glasiers (to whose shops we sent for it) to make panes for windows; and thereby made the brass-ring with its glass-cover a kind of receiver, whose open orifice we carefully cemented on to the engine; and then we found, as we had conjectured, that usually at the first exsuction (though sometimes not till the second) the glass-plate would be broken inwards with such violence, as to be shattered into a great multitude of small fragments, and (which was remarkable) the irruption of the external air driving the glass inwards did constantly make a loud clap, almost like the report of a pistol. Which phænomenon, whether it may help us to discover the cause of that great noise, that is made upon the discharging of guns, (for the recoil seems to depend upon the dilatation and impulse of the powder) I must not stay to consider.

EXPERIMENT VI.

Shewing, that the breaking of glass-plates in the foregoing experiment, need not to be ascribed to the fuga vacui.

THOUGH I long since informed you, that in the experiments I then presented your lordship, it was not my purpose to deliver my own opinion, whether there be a vacuum, or no; and though I do not in this tract intend to declare my self either way; yet, that I may on this occasion also shew, that the pressure of the air may suffice to account for divers phœnomena, which according to the vulgar philosophers must be referred to nature's abhorrency of a vacuum, I will illustrate the foregoing experiment by another, the substance whereof is this.

THAT if, instead of the above mentioned brass hoop, both whose orifices are of equal breadth, you imploy a hollow (but taller) piece of brass, or (which is more easily made) of latten, shaped like a conus truncatus, or a sugar-loaf, whose upper part is taken off parallel to the bottom; and if you make the two orifices of a breadth sufficiently unequal, as if the larger being made as wide as that of our brass-hoop, the straiter were less than an inch in diameter; you will find, that if this piece of metal be made use of, as the other was in the foregoing experiment, the flat glass cemented on to the orifice, will be easily broken, as formerly when it is fastened to the wider orifice; but if the straiter orifice be turned upward, the glass that covers it, if it be of a due thickness (though no thicker than the former) will remain entire, notwithstanding the withdrawing of the air from beneath it: which seems sufficiently to argue, that it is not precisely nature's abhorrency of a vacuum, that is the cause, why glasses are usually broken in such experiments, since whether the wider or the narrower orifice be uppermost, and covered, (the metalline part of the vessel being the same, and only varying its posture) the capacity of the exhausted vessel will be equal; and therefore nature ought to break the glass as well in one case as the other, which yet the experiment shews she does not.

WHEREFORE this diversity seems much better explicable by saying, that when the wider orifice is uppermost, the glass that covers it must serve for the basis of a large atmospherical pillar, which by its great weight may easily force the resistance of the glass: whereas when the smaller orifice is uppermost, there leans upon its cover but so slender a pillar of the atmosphere, that the natural tenacity or mutual cohæsion of parts in the glass is not to be surmounted by a weight, that is no greater.

EXPERIMENT VII.

About a convenient way of breaking blown bladders by the spring of the air included in them.

THE foregoing experiments having sufficiently manifested the strength of the airs spring upon fluid bodies, I next thought fit to try, whether the force of a little included air would also upon consistent and even solid bodies emulate the operations of the weight of the atmosphere. In the prosecution of which enquiry we thought fit to make two sorts of trials: the one, where the air is included in the bodies, on which its spring does work; and the other, where it is external to them. Of the first sort are this seventh and the two following experiments; and of the second sort are some other trials, to be comprehended under the tenth experiment.

HAVING formerly mentioned to your lordship, that we were several times able (though sometimes not without much difficulty) to make a blown bladder break with the spring of its own air; I should not think it worth while to say any thing here about the same phœnomenon, but that (besides that it seems odd enough, and is not unpleasant to many spectators) it may deserve not to be wholly neglected, because a good way to break bladders in the much exhausted receiver may sometimes prove an useful expedient, especially in such cases, where the experimenter (who sometimes either is not skilful enough, or well enough furnished with accommodations to regulate the ingress of the air) would very suddenly supply the receiver with fresh air, when it has been much emptied, without danger of letting in too much air from without. Not to mention, that the air, included in the bladder to be broken, may be so mingled with steams, or imbued with divers qualities, as to be much fitter than common air for some particular purposes.

WE shall then for the affinity's sake between this trial and the former, subjoin now the way, by which we seldom failed of breaking bladders in our emptied receivers. For this purpose, the blown bladder, that was to be burst, having the neck very closely and strongly tied, was kept a pretty while in the receiver, whilst the air was pumping out, and then taken out again, that, now the fibres were stretched and relaxed, the capacity being lessened by a new ligature that I ordered to be strongly made near the

the neck, the bladder might be lessen'd though the air were but the same, and the membrane being not so capable of yielding as before, upon the second exhaustion of the receiver the bladder in it would break, far more easily than otherwise, and perhaps be oddly enough lacerated.

WE sometimes also varied this way of disposing bladders to be burst, by omitting the preparatory putting in of the bladder into the receiver, and only taking it in a little near the neck, that, the bladder having not been blown very full at first, the tension of the included air might be greater. But this last way is to be made use of, when the thing we desire is, that the bladder by breaking at a certain time may part with its air, and not when 'tis only to give an instance of the force of the spring of uncompressed air against the sides of the vessel that contain it.

EXPERIMENT VIII.

About the lifting up a considerable weight by the bare spring of a little air included in a bladder.

YOU will easily believe, that the force imployed (in the foregoing experiment) by the air, to break the well blown bladders it is included in, is considerable, if I here add, that a small quantity of air, which will not fill ⅟₅ of a bladder, will not only serve to blow it quite up, but will manifestly swell it, though that effect be opposed not only by the resistance of the bladder it self, but by a considerable weight tied to the bottom of it, as in the following experiment.

WE took a middle sized bladder (of a hog or sheep) and having pressed out the air, till there remained but a fourth or fifth part (by guess) we caused the neck to be very strongly tied up again: also round about the opposite part of the bladder, within about an inch of the bottom, we so strongly tied another string, that it would not be made to slip off by a not inconsiderable weight we hung at it. Then fastening the neck of the bladder to the turning key, we conveyed the bladder and the weight hanging at it into a large receiver, in which when it began to be pretty well exhausted, the air within the bladder being freed from the wonted pressure of the air without it, did by its own spring manifestly swell, and thereby notably shorten the bladder that contain'd it, and by consequence visibly lifted up the weight, (that resisted that change of figure) which exceeded fifteen pound of sixteen ounces to the pound.

AFTER that we took a larger bladder, and having let out so much air, that it was left lank enough, we fastened the two ends of it to the upper part of the receiver, (for which else it would have been too long) and tied a weight (but not the same) so as that it hung down from the middle of the bladder: then exhausting the receiver as before, though the bladder, and this new weight which stretched it, reached so low, as that for a while we could scarce see, whether it hung in the air or no, yet at length we perceived the bladder to swell, and concluded, that it had lifted up its clog about an inch; which was confirmed by the return we permitted of the air into the receiver, upon which the bladder became more wrinkled than before, and the weight descended, which being taken off, and weighed in a statera, amounted to about 28 pounds. We would have reiterated the experiment, but so heavy a weight having broken the bladder, we were discouraged from proceeding any further, especially in regard of the difficulty of bringing by this contrivance the strength of the air's spring to any exact computation; though it sufficiently shews what I designed it should, namely, that the spring of a little included air may be able even in so slight a contrivance to raise a great weight.

WHETHER this experiment may any way illustrate the motion of muscles, made by inflation, contraction, &c. it belongs not to this place to consider.

EXPERIMENT IX.

About the breaking of hermetically sealed bubbles of glass by the bare spring of their own air.

I SHALL premise to the following trials an experiment, wherein uncompressed air is made by its own bare spring to break the solid body itself it is shut up in. And this I the rather set down before the subsequent trials, because in our already published physico-mechanical experiments mention has been made of this trial, as of one, that we could not then make to succeed; we have since, employing smaller receivers, made it often enough prosperously, somewhat to the wonder of eminent virtuosi, who confessed to me they had made frequent and divers attempts to perform the same thing, without ever succeeding in any of them.

BUT it will not be requisite to multiply relations about this particular, and therefore I shall set down but this one, which I meet with among my loose notes.

A large glass bubble hermetically sealed being put into the receiver, and the air drawn out as much as in usual operations, and somewhat more, though I told the company beforehand, that I had several times observed, that such bubbles would not break immediately, but somewhile after the withdrawing the air from about them; yet this continued so long entire after we had left off pumping, that presuming it had been blown too strong, I began to despair of the experiments succeeding; when, whilst we were providing something else to put into the receiver, and, as I guessed, four minutes after the pump had been let alone, the bubble surprized us with its being broken with such violence by the spring of the included air, that the fragments of it were dashed every way against the sides of the receiver, and broken so very small, that when we came to take it up, the powder was by the by-standers compared to the small sand wont to be imployed to dry papers, that have been newly writ upon with ink. The reason why the bubble broke so slowly I cannot now stay to propose, no more than to examine whether the difficulty of breaking vessels of glass, no thicker than these bubbles, proceed from some weakening of the spring of imprisoned air, by its stretching a little the including glass, (for in another case we have observed this glass to be stretchable by the pressure of air) or from hence, that 'twas very hard, as I have elsewhere mentioned, to avoid rarifying the air a little, and consequently weakening its spring, by the heat, that was necessary to be imployed about the sealing up the bubble.

EXPERIMENT X.

Containing two or three trials of the force of the spring of our air uncompressed upon stable and and even solid bodies, (whereto it is external.)

IN prosecution of the inquiry proposed in the title, we made (among others) the following trials.

The FIRST TRIAL.

I. WE took the brass-hoop, mentioned in the fifth experiment (whose diameter is somewhat above three inches) and having caused a glazier to cut some plates of glass, such as are used for making the quarrels of windows, till he had brought them to a size, and a roundness fit to serve for covers to that brass-hoop, we carefully fastened one of them with cement to the upper orifice of the hoop or ring, and then cementing the lower orifice to the engine, so that the vessel, composed of the metal and glass, served for a small receiver; we whelmed over it a large and strong receiver, which we also fastened on to the engine with cement after the usual manner. By which contrivance it was necessary, that when the pump was set on work, the included receiver (of brass and glass) should have its air withdrawn, and yet the air in the larger receiver should not be pumped out but by breaking through the glass, so that the internal air of the metalline receiver (as we may call it for distinction sake) being pumped out, the glass plate, that made part of that receiver, must lie exposed to the pressure of the ambient air shut up in the other receiver, without having the former assistance of the now withdrawn air to resist the pressure: wherefore, as we expected, at the first or second exsuction of the air, included in the small metalline receiver, the glass-plate was, by the pressure of the incumbent air contained in the great receiver, broken into an 100 pieces, which were beaten inwards into the cavity of the hoop.

The SECOND TRIAL.

II. THIS done, to shew, that there needed not the spring of so great a quantity of included air to break such glasses, we took another roundish one, which, though wide enough at the orifice to cover the brass ring and the new glass-plate that we had cemented on it, was yet so low, that we estimated it to hold but a sixth part of what the large receiver, formerly imployed, is able to contain; and having whelmed this smaller vessel, which was shaped like those cups they call tumblers, over the metalline receiver, and well fastened it to the engine with cement, we found, that though this external receiver had a great part of its cavity filled by the included one, yet when this internal one was exhausted by an exsuction or two, the spring of the little air that remain'd, was able to break the plate into a multitude of fragments.

The THIRD TRIAL.

III. BECAUSE the glass-plates hitherto mentioned seemed not so thick, but that the pressure of the included air might be able to give considerable instances of its force; instead of the metalline receivers hitherto employed, we took a square bottle of glass, which we judged to be able to contain about a pint (or pound) of water, and which had been provided to keep subtle chymical liquors in, for which use we are not wont to chuse weak ones. This we inverted, and applied to the engine as a receiver, over which we whelmed the large receiver formerly mentioned; and having cemented it on, as in the foregoing experiments, we set the pump on work to empty the internal receiver (or square bottle) by which means the withdrawing of the air, and the figure of the vessel (which was inconvenient for resisting) suffered the pressure of the air included in the external receiver to crush the vial into a great number of pieces.

AND to vary this experiment, as we did that of breaking the metalline receivers, we took another glass of the shape and about the bigness of the former, and having applied it to the engine as before, and covered it with a receiver, that was little higher than it self, we found, that upon the exhaustion of the air the second square glass was likewise broken into many fragments, some of which were of so great a thickness, as moved some wonder, that the bare pressure of the air was able to break such a vessel, though probably the cracks, that reached to them, were begun in much weaker parts of the glass.

N. B. 1. THE bottoms and the necks of both these square bottles were entire enough; by which it seemed probable, that the vessels had been broken by the pressure of the air against the sides, which were not only thinner than the parts above named, but exposed a larger superficies to the lateral pressure of the air, than to the perpendicular.

2. WE observ'd in one of the two last experiments, that the vessel did not break presently upon the last exsuction, that was made of the included air, but a considerable time after, which it seems was requisite to allow the compressed parts of the glass time to change their places: and this phænomenon I therefore mention, because the same thing, that here happened in the breaking a glass inwards by the spring of the air, I elsewhere observed to have happened in breaking a glass outwards by the same spring.

3. To confirm, that it is the spring of the external receiver's air, that is the agent in those fractures of glasses, and to prevent or remove some scruples, we thought fit to make this variation in the experiment. We applied a plate of glass, just like those formerly mentioned, to the brass-hoop; but in the cementing of it on, we placed in the thickness of the cement

a small pipe of glass about an inch long, whose cavity was not so big as that of a straw, and which being left open at both the ends, might serve for a little channel, through which the air might pass from the external receiver to the internal: over this we whelmed one of the small receivers abovementioned, and then, though we set the pump on work, much longer than would have needed, if this little pipe had not been made use of, we found, as we expected, that the internal receiver continued entire, because the air, whose spring should have broken it, having liberty to pass through the pipe, and consequently to expand itself into the place deserted by the air pumped out, did by that expansion weaken its spring too much, to retain strength enough to break the metalline (or internal) receiver.

BUT here it is to be noted, that either the pipe must be made bigger than that lately mentioned, or the exsuction of the air must not be made by the pump as nimbly as we can, or otherwise the plate of glass may be broken, notwithstanding the pipe; because the air contained in the external receiver having a force much greater than is necessary to break such a plate, it may well happen (as I have sometimes found it do) that if the air be hastily drawn out of the internal receiver, that air, which should succeed in its room, cannot get fast enough out of that external receiver through so small a pipe; and the air remaining in that external receiver will yet retain a spring strong enough to break the glass. To illustrate which, I shall propose this experiment; that sometimes, when I have at the flame of a lamp caused glass bubbles to be blown with exceeding slender stems, if they were nimbly removed out of the flame whilst they were ignited, they would according to my conjecture, be either broken, if they cooled too fast, or compressed inward, if they long enough retained the softness they had given them by fusion. For the air in the bubble being exceeding rarified and expanded, whilst the glass is kept in the flame, and coming to cool hastily when removed from thence, loses upon refrigeration the spring the heat had given it; and so, if the external air cannot press it fast enough through the too slender pipe, there will not get in air enough to resist the pressure of the atmosphere; and therefore, if this pressure find the bubble yet soft, it will press it a little inwards, and either flatten it, or make a dimple in it, though the orifice of the pipe be left open.

EXPERIMENT XI.

Shewing, that mercury will in tubes be raised by suction no higher than the weight of the atmosphere is able to impel it up.

IT is sufficiently known, that the common opinion of philosophers, and especially of those which follow *Aristotle*, has long been, and still is, that the cause of the ascension of water upon suction, and particularly in those pumps, where the water seems of its own accord to follow the rising sucker, is nature's abhorrency of a vacuum. Against this received opinion divers of the modern philosophers have opposed themselves. But as some of them were vacuists, and others plenists, they have explicated the ascension of water in sucking-pumps upon very different grounds; so that many ingenious men continue yet irresolved in this noble controversy. Wherefore though I have formerly made, and now renew a solemn profession, that I do not in this treatise intend to declare either for or against the being of a vacuum; and though I have * elsewhere occasionally acknowledged my self not to acquiesce fully in what either the ancient or the modern philosophers have taught about the adequate cause of suction; (in the assigning of which, I think, I have shewn them to have been somewhat deficient;) yet since I think some experiments, of importance to this controversy, may be better made by the help of our engine, than they have been by any instrument I have yet heard of, I shall now add the trials I made, to shew both, that whether there be, or may be a vacuum or not, there is no need to have recourse to a *fuga vacui* to explicate suction; and also that whatever other causes have by *Gassendus* and *Cartesius* been ingeniously proposed to explicate suction, it seems to depend clearly upon the weight of the atmosphere, or in some cases upon the spring of the air; though I deny not, that other causes may contribute to that pressure of the air, which I take to be the grand and immediate agent in these phænomena.

WE took a brass pipe bended like a siphon, and fitted at the bigger end with a stop-cock, &c. as is delineated in the figure, (which instrument for brevity sake, I often call an exhausting, or sucking siphon) and to the slender end of this we fastned with good cement the upper end of a cylindrical pipe of glass, of about fifty inches long, and open at both ends, and having the lower end open into a glass of stagnant quick-silver, whose upper superficies reached a pretty deal higher than the immersed orifice of the glass cane. These things being thus prepared, we caused the pump to be set on work, whereby the air being by degrees drawn out of the exhausting siphon, and consequently of the glass cane that opened into it; the stagnant mercury was proportionably impelled up into the glass-pipe, until it had attained to its due height, which exceeded not 30 inches. And then, though there remained in the upper part of the pipe above 20 inches unfilled with quick-silver, yet we could not by farther pumping raise that fluid metal any higher. See Plate III. Fig. 2. and the annotations at the close of this experiment.

BY which it seems manifest enough, that whatever many learned men have taught, or others do yet believe about the unlimited power, that nature would exercise, to prevent what they call a vacuum; yet this power has its bounds, and those depend not so much upon the exigency of that principle, which the schoolmen call a *fuga vacui*, as upon the specifick gravity of the liquor to be raised by suction. For confirmation of which, we substituted instead

* The place here meant is a passage in the Author's *Examen* of Mr. *Hobb's dialogue* about the air.

stead of the stagnant mercury a bason of water; and though instead of the many sucks we had fruitlessly imployed to raise the quick-silver above the lately mentioned height, we now imployed but one exsuction, (or less than a full one) which did but in part empty the exhausting siphon: yet the water upon the opening of the stop-cock was not only impelled to the very top of the glass-cane, but likewise continued running for a good while through the exhausting siphon, and thence fell upon the plate of the engine; so that it seemed an odd spectacle to those, that knew not the reason of it, to see the water running very briskly of its own accord, as they imagined, out of the shorter leg of a siphon; especially that leg being perhaps not above a quarter so long as the other. And here I must not omit this considerable circumstance, that though sometimes in the Torricellian experiment, I have observed the mercury to stand at thirty inches, and now and then above it, yet the height of the mercury elevated in our glass-cane appeared not, when measured, to reach fully 29 inches and a quarter; which I thought it was not difficult to render a reason of, from the varying weight of the atmosphere; and accordingly consulting the baroscope (that stood in another room) I found the atmosphere to be at that time somewhat light, the quick-silver in it being in height but 29 inches and an eighth, which probably would have been the very height of the quick-silver raised by the engine, if it had had time by standing to free it self from bubbles.

FROM whence we may conclude, that suction will elevate liquors in pumps no higher than the weight of the atmosphere is able to raise them, since the closeness requisite in the pump of our engine to be staunch makes it very unlikely, that by any ordinary pump a more accurate suction can be effected.

I have nothing to add, about the related experiment but this one; that it may afford us a notable confirmation of the argument we formerly proposed against them, that ascribed the elevation and sustentation of the quick-silver in the Torricellian experiment to a certain rarified air, which the more highly it is rarified, the greater power it acquires to attract quick-silver, and other contiguous bodies; for in our experiment, though by continuing to pump we can rarify or distend more and more the air in the exhausting siphon, yet we were not able to raise the mercury above thirty inches (which exceeds not the height, to which the atmosphere is able to elevate it) and this, though the stagnant mercury being exposed to the free air, it cannot be pretended (as in some other cases it may, though not satisfactorily, be done) that the mercury cannot be raised higher, without offering violence to the body incumbent on the stagnant mercury: for in the experiment we are considering, if nature should raise the quick-silver higher and higher in the pipe, to succeed in the room of the air that is withdrawn, the formerly stagnant mercury, that would on this occasion be raised, might be immediately succeeded by the free and undilated air, so that nature would be put to offer violence to the quicksilver only, which if she were scrupulous to do, what ailed her to raise it (as she did in our trial) against the inclinations of so ponderous a body, to above 29 inches high?

ANNOTATION.

THOUGH the exhausting siphon, mentioned at the beginning of this experiment, may be easily enough conceived by an attentive inspection of the figure; yet because I frequently made use of it in pneumatical experiments, it will not be amiss to intimate here once for all these three particulars about it. 1. That though the bending pipe itself may be for some uses more conveniently made of glass than of metal, because the transparency of the former may enable us to discover what passes in it; yet for the most part we chuse to imploy pipes of the latter sort, because the others are so very subject to break. 2. That it is convenient to make the longer leg of the siphon a little larger at the bottom than the rest of the pipe usually needs to be, that it may the more commodiously admit the shank of a stop-cock, which is to be very carefully inserted with cement; by seasonably turning and returning of which stop-cock, the passage (for the air) between the engine and the vessel to be exhausted is to be opened and shut. 3. That though we sometimes content our selves to apply immediately the brass siphon itself to the engine, by fastening with cement the external shank of the stop-cock to the orifice of the little pipe, through which the exsuction of the air is made; yet the bended pipe alone, if it be not almost constantly held, is so apt to be loosened by the motion of the engine, and the turning of the stop-cock, (which frequently occasions leaks, and disturbs the operation) that for the most part we make use of a siphon, consisting of a brass pipe, and stop-cock, and a glass of 6, 8, or 10 inches in height, and of some such shape (for it need not be the very same) as that represented in the figure: for by this means, though the exhaustion is because of this additional glass somewhat longer in making, yet it is more securely and uninterruptedly carried on by reason of the stability, which the breadth of the lower orifice of the glass gives to the whole instrument. Besides which, we have these other conveniences, that not only the siphon is hereby much lengthened, which in divers trials is very fit; but also, that we may commodiously place in the glassy part of this compounded siphon a gage, whereby to discern from time to time, how much the air is drawn out of the vessel to be exhausted.

See Plate III. Fig 2.

EXPERIMENT XII.

About the differing heights, whereto liquors will be elevated by suction, according to their several specifick gravities.

IF, when I was making the foregoing experiment, I had been able to procure a pipe long enough, I had tried to what height I could raise water by suction, though I would have done it rather to satisfy others than my self, who scarce doubted, but that as water is (bulk for bulk) about 14 times lighter than quick-ſilver: ſo it would have been raiſed by ſuction to about four or five and thirty foot, (which is 14 times as high as we were able to elevate the quick-ſilver) and no higher. But being not furniſhed for the trial I would have made, I thought fit to ſubſtitute another, which would carry the former experiment ſomewhat further. For whereas, in that we ſhewed how high the atmoſphere was able by its whole gravitation to raiſe quick-ſilver; and whereas likewiſe that, which appears in Monſieur *Paſchal*'s experiment, is, at what height the whole weight of the atmoſphere can ſuſtain a cylinder of water: by the way, that I thought on, it would appear (which hath not yet, that I know of, been ſhewn) how a part of the preſſure of the air would in perpendicular pipes raiſe not only the two mentioned liquors, but others alſo to heights anſwerable to the degree of preſſure, and proportionable to the ſpecifick gravities of the reſpective liquors.

To make this trial the more clear and free from exceptions, I cauſed to be made and inſerted to the ſhorter leg of the above mentioned exhauſting ſiphon a ſhort pipe; which branched itſelf equally to the right hand and the left, as the adjoining figure declares. In which contrivance I aimed at theſe two conveniences: one that I might exhauſt two glaſs-canes at the ſame time; and the other, to prevent its being ſurmiſed, that the engine was not equally applied to both the glaſſes to be exhauſted. This additional braſs-pipe being carefully cemented into the ſucking ſiphon, we did to each of its two branches take care to have well faſtened with the ſame cement a cylindrical glaſs of about 42 inches in length (that being ſomewhat near the height of our exhauſting ſiphon above the floor) the lower orifice of one of theſe two glaſſes being immerſed in a veſſel of ſtagnant mercury, and that of the other in a veſſel of water, where care was taken by thoſe I employed, that as the tubes were choſen near of a bigneſs, (which yet was not neceſſary) ſo the ſurfaces of the two different liquors ſhould be near of a height. This being done, we began to pump warily and ſlowly, till the water in one of the pipes was elevated to about 42 inches; and then meaſuring the height of the quick-ſilver in the other pipe above the ſurface of the ſtagnant quick-ſilver, we found it to be almoſt three inches; ſo that the water was about 14 times as high as the quick-ſilver. And to proſecute the experiment a little further, we very warily let in a little air to the exhauſting ſiphon, and had the pleaſure to ſee the two liquors proportionably deſcend, till turning the ſtop-cock, when the water was about 14 inches high, we thereby kept them from ſinking any lower, till we had meaſured the height of the quick-ſilver, which we found to be about one inch.

Plate III. Fig. 3.

WE tried alſo the proportion of theſe two liquors at other heights, but could not eaſily meaſure them ſo well as we did at thoſe newly mentioned; and therefore though there ſeemed to be ſome ſlight variation, yet we looked upon it but as what might be well imputed to the difficulty of making ſuch experiments exactly; and this diſpleaſed me not in theſe trials, that whereas it was obſerved, and ſomewhat wondered at, that the quick-ſilver for the moſt part ſeemed to be ſomewhat (though but a very little) higher then the proportion of 1 to 14 required, I had long before by particular trials found, that though 14 and 1 be the neareſt of ſmall integer numbers, that expreſs the proportion between the ſpecifick gravities of quick-ſilver and water, yet the former of thoſe fluids (or at leaſt that, which I made my trials with) is not quite ſo heavy as this proportion ſuppoſes, though I ſhall not here ſtay to determine preciſely the difference, having done it in another tract, where the method I employed in the inveſtigation of it is alſo ſet down.

THE above-mentioned experiment, made by the help of our engine, as to quick-ſilver and water being confirmable by trials (to be by and by mentioned) made in other liquors, affords our hypotheſis two conſiderable advantages above the vulgar doctrine of the ſchools (for I do not apply what follows to all the pleniſts) who aſcribe the aſcenſion of liquors by ſuction to a traction made *ob fugam vacui*, as they are wont to ſpeak.

FOR firſt it is manifeſtly agreeable to our doctrine, that, ſince the air, according to it, is a fluid, that is not void of weight, it ſhould raiſe thoſe liquors, that are lighter, as water, higher then thoſe that are ponderous, as quick-ſilver; and that anſwerably to the diſparity of their weights. And ſecondly, there is no reaſon, why, if the air be withdrawn by ſuction from quick-ſilver and water, there ſhould be leſs left a vacuum above the one than above the other, in caſe either of them ſucceed not in the place deſerted by the air; and conſequently when the air is withdrawn out of both the forementioned glaſs-pipes, if there would be no vacuum in caſe no liquor ſhould ſucceed it, why does nature needleſly to prevent a vacuum make the water, that is an heavy body, aſcend contrary to its own nature, according to which it tends towards the center of the earth? And if the ſucceeding of a liquor be neceſſary to prevent a vacuum, how chance that nature does not elevate the quick-ſilver as well as the water; eſpecially ſince it is manifeſt by the foregoing experiment, that ſhe is able to raiſe that ponderous liquor above 26 inches higher than ſhe did in the experiment we are now diſcourſing of.

PERHAPS it would not be amiſs to take notice, on this occaſion, that among other applications of this experiment it may be made ſomewhat uſeful to eſtimate the differing gravities of liquors; to which purpoſe I cauſed to be put under the bottom of the forementioned glaſs-pipes two veſſels, the one with freſh water, and the other with the like water impregnated with a good proportion of ſea-ſalt, that I had cauſed to be diſſolved in it, for want of ſea-water, which I would rather have employed. And I found, that when the freſh water was raiſed to about 42 inches, the ſaline ſolution had not fully reached to 40.

BUT though this difference were double

to that, which the proportion and gravity betwixt our sea-water and fresh water would have required; yet to make the disparity more evident, and also because I would be able the better to guess at the proportion of the dissolved salt, by making it as great as I could, I caused an unusual brine to be made, by suffering sea-salt to deliquate in the moist air.. And having applied this liquor and fresh water to the two already mentioned pipes, and proceeded after the former manner, we found, that when the pure water was elevated to near 42 inches, the liquor of sea-salt wanted about 7 inches and a quarter of that height; and when the water was made to subside to the middle of its pipe, or thereabouts, the saline liquor in the other pipe was between 3 and 4 inches lower than it.

I would have tried the difference between these liquors and oil, but the coldness of the weather was unfavourable to such a trial: but to shew a far greater disparity than that would have done betwixt the height of liquors of unequal gravities, I took fair water, and a liquor made of the salt of pot-ashes suffered to run in a sellar *per deliquium*, (this being one of the ponderousest liquors I ever prepared,) and having proceeded as in the former trials, I found, that when the common water was about 42 inches high, the newly mentioned solution wanted somewhat of 30 inches; and when the water was made to subside to the middle of its pipe, or thereabouts, the deliquated liquor was between 6 and 7 inches lower than it.

I had some thoughts, when I applied my self to make these trials, to examine how well we could by this new way compare the saltness of the waters of several seas, and those also of salt-springs; and likewise whether, and (if any thing near) how far we might by this method determine the proportion of the more simple liquors, that may be mingled in compounded ones, as in the mixture of water and wine, vinegar and water, &c. but being not provided with instruments fit for such nice trials, and a mischance having impaired the glasses lately mentioned before the last trials were quite ended, and having soon after broken one of them, I laid aside those thoughts.

EXPERIMENT XIII.

About the heights, to which water and mercury may be raised, proportionably to their specifick gravities, by the spring of the air.

IN prosecution of the parallel formerly begun, betwixt the effects of the weight of the atmosphere, and the spring of included air, we thought fit, after the foregoing, to make the following experiment.

WE took a strong glass-bottle, capable to hold above a pint of water, and having in the bottom of it lodged a convenient quantity of mercury, we poured on it a greater quantity of water, (because this liquor was to be impelled up many times higher than the other,) and having provided two slender glass-pipes, each open at both ends, we so placed and fastened them, by means of the cement, wherewith we choaked the upper part of the neck of the bottle, that the shorter of the pipes had its lower orifice immersed beneath the surface of the quick-silver, and the longer pipe reached not quite so low as that surface, and so was immersed but in the water, by which contrivance we avoided the necessity of having two distinct vessels for our two stagnant liquors, which would have been inconvenient in regard of the slenderness of the upper part of our receiver. This done, we conveyed the bottle into a fitly shaped receiver, (formerly described at the first experiment,) and having begun to pump out the air, we took notice to what heights the quick-silver and water were impelled up in their respective tubes, on which we had before made marks from inch to inch with hard wax, (that they might not be removed by wet or rubbing,) and we observed, that when the quick-silver was impelled up to two inches, the water was raised to about eight and twenty; and when the quick-silver was about one inch high, the water was about fourteen. I say, about, partly because some allowances must be made for the sinking of the superficies of the stagnant quick-silver, and the greater subsidence of that of the stagnant water, by reason of the liquors impelled into the two pipes; partly, because that the breadth of the mark of wax was considerable, when the quick-silver was but about an inch high, and so made it difficult to discern the exact height of the metal, when the water was fallen down to fourteen inches: especially in regard, that the quick-silver never ascending so high as the neck of the bottle, (which the water left far beneath it,) the thickness of the receiver, and that of so strong a bottle, made it difficult to discern so clearly the station of the quick-silver as I could have wished.

EXPERIMENT XIV.

About the heights answerable to their respective gravities, to which mercury and water will subside, upon the withdrawing of the spring of the air.

FOR the further illustration of the doctrine proposed in the last, and some of the foregoing experiments, about the raising and sustentation of liquors in pipes by the pressure of the air; I thought it not unfit to make the following trial, though it were easy to foresee in this peculiar experiment a peculiar difficulty.

WE caused then to be conveyed into a fitly shaped receiver two pipes of glass very uneven in length, but each of them sealed at one end: the shorter tube was filled with mercury, and inverted into a small glass jarr, wherein a sufficient quantity of that liquor had been before lodged: the longer pipe was filled with common water, and inverted into a larger glass, wherein likewise a fit proportion of the same liquor had been put.

THEN the receiver being closely cemented on to the engine, the air was pumped out for a pretty while before the mercury began to

subside;

ſubſide; but when it was ſo far withdrawn, that its preſſure was no longer able to keep up a mercurial cylinder of that height, that liquid metal began to ſink; the water in the other tube, though this were three times as long, ſtill retaining its full height. But when the quick-ſilver was fallen ſo low, as to be but between three and four inches above the ſurface of the ſtagnant quick-ſilver, the water alſo began to ſubſide, but ſooner than according to the laws of meer ſtaticks it ought to have done, becauſe many aerial particles emerging from the body of the water to the upper part of the glaſs, did by their ſpring concur with the gravity of the water to depreſs this liquor. And ſo when the quick-ſilver was three inches above the ſtagnant mercury, the water in the other pipe was fallen divers inches beneath 42, and ſeveral inches beneath 28, when the mercury had ſubſided an inch lower. But this being no more than was to be expected, after we had cauſed the pumping to be a while continued, to free the water the better from the latitant air, we let in the external air; and having thereby impelled up again both the liquors into their pipes, and removed the receiver, we took out thoſe pipes, and inverting each of them again to let out the air, (for even that, which held the quick-ſilver had got a ſmall bubble, though inconſiderable in compariſon of the air that had got up out of the water,) we filled each of them with a little of the reſtagnant liquor belonging to it; and inverting each tube once more into its proper liquor, we repeated the experiment, and found it, as it ſeemed, to require more pumping than before to make the liquors begin to ſubſide; ſo that when the mercury was fallen to three inches, or two or one, the water ſubſided ſo near to the heights of 42, 28, or 14 inches, that we ſaw no ſufficient cauſe to hinder us from ſuppoſing, that the little differences, that appeared between the ſeveral heights of the quick-ſilver, and fourteen times as great heights of the water (which fell ſomewhat lower than its proportion in gravity required) proceeded from ſome aerial corpuſcles yet remaining, in ſpite of all we had done, in the water, and by their ſpring, though but faint, when once they had emerged to the upper part of the glaſs, furthering a little the depreſſion of it: not now to mention leſſer circumſtances, particularly, that the ſurface of the ſtagnant water did not inconſiderably riſe by the acceſſion of the water lately in the pipe; whereby the cylinder of water, raiſed above that ſurface, became by ſo much the ſhorter. However your lordſhip may, if you think fit, cauſe the experiment to be reiterated, which I could not ſo well do, by reaſon of a miſchance that befel the receiver.

EXPERIMENT XV.

About the greateſt height, to which water can be raiſed by attraction or ſucking pumps.

SINCE the making and the writing of the foregoing experiments, having met with an opportunity to borrow a place ſomewhat convenient to make a trial, to what height water may be raiſed by pumping; I thought not fit to neglect it. For though both by the conſideration of our hypotheſis, to whoſe truth ſo many phænomena bear witneſs; and though particularly by the conſequences deduceable from the three laſt recited experiments, I were kept from doubting what the event would be; yet I thought it worth while to make the trial.

I know what is ſaid to have been the complaint of ſome pump-makers. But I confeſs the phænomenon, it was grounded on, ſeemed not to me to be certainly enough delivered by a writer or two, that mention what they complained of; and their obſervation ſeems not to have been made determinately or carefully enough for a matter of this moment. Since that, which they complain of, ſeems to have been in general, that they could not by pumping raiſe water to what height they pleaſe, as the common opinion of philoſophers about nature's *fuga vacui* made them expect they might. And it may well have happened, that as they endeavoured only to raiſe it to the height their occaſions required, ſo all that their diſappointment manifeſted, was, that they could not raiſe it to that particular height: which did not determine, whether, if the pump had been a foot or a yard ſhorter, the water would then have been elevated to the upper part of it or no: but that which I chiefly conſider is, that theſe being but tradeſmen, that did not work according to the dictates of, or with deſign to ſatisfy a philoſophical curioſity, we may juſtly ſuſpect, that their pumps were not ſufficiently ſtanch, nor the operation critically enough performed and taken notice of.

WHEREFORE, partly becauſe a trial of ſuch moment ſeemed not to have yet been duly made by any; and partly becauſe the varying weight of the atmoſphere was not (that appears) known, nor (conſequently) taken into conſideration by the ingenious Monſieur *Paſchal* in his famous experiment, which yet is but analogous to this; and partly, becauſe ſome very late, as well as learned writers, have not acquieſced in his experiment, but do adhere to the old doctrine of the ſchools, which would have water raiſeable in pumps to any height, *ob fugam vacui*, (as they ſpeak,) I thought fit to make the beſt ſhift I could to make the trial, of which I now proceed to give your lordſhip an account.

THE place I borrowed for this purpoſe was a flat roof about 30 foot high from the ground, and with rails along the edges of it. The tube we made uſe of ſhould have been of glaſs, if we could have procured one long and ſtrong enough. But that being exceeding difficult, eſpecially for me, who was not near a glaſs-houſe, we were fain to cauſe a tin-man to make ſeveral pipes of above an inch bore, (for of a great length it was alledged they could not be made ſlenderer,) and as long as he could, of tin or latten, as they call thin plates of iron tinned over; and theſe being very carefully ſodered together made up one pipe, of about one or two and thirty foot long, which being tied to a pole, we tried with

with water whether it was ſtanch, and by the effluxions of that liquor finding where the leaks were, we cauſed them to be ſtopped with ſoder; and then for greater ſecurity, the whole pipe, eſpecially at the commiſſures, was diligently caſed over with our cloſe black cement, upon which plaiſter of Paris was ſtrewed to keep it from ſticking to their hands or cloaths, that ſhould manage the pipe. At the upper part of which was very carefully faſtened with the like cement a ſtrong pipe of glaſs, of between 2 and 3 foot in length, that we might ſee what ſhould happen at the top of the water. And to the upper part of this pipe was (with cement, and by the means of a ſhort elbow of tin) very cloſely faſtened another pipe of the ſame metal, conſiſting of two pieces, making a right angle with one another, whereof the upper part was parallel to the horizon, and the other, which was parallel to the glaſs-pipe, reached down to the engine, which was placed on the flat roof, and was to be with good cement ſollicitouſly faſtened to the lower end of this deſcending part of the pipe, whoſe horizontal leg was ſupported by a piece of wood, nailed to the abovementioned rails; as the tube alſo was kept from overmuch ſhaking by a board, faſtened to the ſame rails, and having a deep notch cut in it, for the tube to be inſerted into.

See Plate V. Fig. 1. THIS apparatus being made, and the whole tube with its pole erected along the wall, and faſtened with ſtrings and other helps, and the deſcending pipe being carefully cemented on to the engine, there was placed under the bottom of the long tube a convenient veſſel, whereinto ſo much water was poured, as reached a great way above the orifice of the pipe, and one was appointed to ſtand by to pour in more as need ſhould require, that the veſſel might be ſtill kept competently full.

AFTER all this, the pump was ſet on work; but when the water had been raiſed to a great height, and conſequently had a great preſſure againſt the ſides of the tube, a ſmall leak or two was either diſcovered or made, which without moving the tube we cauſed to be well ſtopped by one, that was ſent up a ladder to apply ſtore of cement where it was requiſite.

WHEREFORE, at length we were able, after a pretty number of exſuctions, to raiſe the water to the middle of the glaſs-pipe abovementioned, but not without great ſtore of bubbles, made by the air formerly concealed in the pores of the water, and now emerging; which for a pretty while kept a kind of foam upon the ſurface of it, (freſh ones continually ſucceeding thoſe that broke.) And finding the engine and tube as ſtanch as could be well expected, I thought it a fit ſeaſon to try what was the utmoſt height, to which water could by ſuction be elevated; and therefore, though the pump ſeemed to have been plied enough already, yet for further ſatisfaction, when the water was within few inches of the top of the glaſs, I cauſed 20 exſuctions more to be nimbly made, to be ſure that the water ſhould be raiſed as high as by our pump it could be poſſibly. And having taken notice where the ſurface reſted, and cauſed a piece of cement to be ſtuck near it, (for we could not then come to reach it exactly,) and deſcending to the ground where the ſtagnant water ſtood, we cauſed a ſtring to be let down, with a weight hanging at the end of it, which we applied to a mark, that had been purpoſely made at that part of the metalline tube, which the ſuperficies of the ſtagnant water had reſted at, when the water was elevated to its full height: and the other end of the ſtring being, by him that let it down, applied to that part of the glaſs, as near as he could gueſs, where the upper part of the water reached, the weight was pulled up; and the length of the ſtring, and conſequently the height of the cylinder of water was meaſured, which amounted to 33 foot, and about 6 inches. Which done, I returned to my lodging, which was not far off, to look upon the baroſcope, to be informed of the preſent weight of the atmoſphere, which I found to be but moderate, the quick-ſilver ſtanding at 29 inches, and between 2 and 3 eights of an inch. This being taken notice of, it was not difficult to compare the ſucceſs of the experiment with our hypotheſis. For if we ſuppoſe the moſt received proportion in bulk between cylinders of quick-ſilver and of water of the ſame weight, namely that of 1 to 14, the height of the water ought to have been 34 foot and about 2 inches, which is about 8 inches greater than we found it. But then your lordſhip may be pleaſed to remember, that I formerly noted, before ever I made this experiment, that I did not allow the proportion betwixt mercury and water (at leaſt ſuch water as I made my trials with) to be altogether ſo great; and though in ordinary experiments, we may with very little inconvenience make uſe of that proportion to avoid fractions, yet in ſo tall a cylinder of water as ours was, the difference is too conſiderable to be neglected. If therefore, inſtead of making an inch of quick-ſilver equivalent to 14 inches of water, we abate but a quarter of an inch, which is but a 56 part of the height of the water, this abatement being repeated 29 times and a quarter, will amount to 7 inches, and above a quarter; which added to the former height of the water, namely 33 foot and 6 inches, will make up 34 foot and above an inch; ſo that the difference between the height of the mercury ſuſtained by the weight of the atmoſphere in the baroſcope, and that of the water raiſed and ſuſtained by the preſſure of the ſame atmoſphere in the long tube, did not appear to differ more than an inch or two from the proportion they ought to have had, according to the difference of their ſpecifick gravities. And though in our experiment the difference had been greater, provided it exceeded not 8 or 10 inches, it would not have been ſtrange; partly, becauſe of the difficulty of meaſuring all things ſo exactly in ſuch an experiment; partly, becauſe as waters are not all of the ſame weight, ſo a little diſparity of it in ſo long a cylinder may be conſiderable; and partly, and perhaps chiefly, becauſe the air flying out of the bubbles, that roſe out of ſo great a quantity of water, and breaking at the top of it, and ſo

near

near that of the tube, might by its ſpring, though but very weak, aſſiſting the weight of ſo much water, ſomewhat (though not much) hinder the utmoſt elevation of that liquor. But our experiment did not make it needful for me to inſiſt on theſe conſiderations; and the inconſiderable difference, that was betwixt the height of the water we found, and that which might have been wiſhed, did rather countenance, than at all diſfavour the thing to be made out by our experiment, ſince by no pumping we could raiſe the water quite ſo high (though I confeſs it wanted but very little) as the weight of the atmoſphere was able to keep up a cylinder of mercury proportionable to it in height, and equivalent in weight: and yet I preſume, your lordſhip will eaſily grant, that there was at leaſt as much care uſed in this experiment, to keep the things imployed about it tight, as has been wont to be uſed by tradeſmen in their pumps, where it is not ſo eaſy either to prevent a little inſinuation of the air, or to diſcern it.

IT is not that I am ſure, that even all our care would have kept the water for any long time at its full height; but that the air was ſufficiently exhauſted for our purpoſe, when we determined the height of the water, I was induced to conclude by theſe circumſtances.

I. As well the conſtruction of the engine, as the many formerly related experiments, that have been ſucceſsfully tried with it, ſhew, that it is not like it ſhould be inferior in cloſeneſs to the great water-pumps, made by ordinary tradeſmen: and particularly the XIth experiment foregoing manifeſts, that by this pump quick-ſilver was raiſed to as great a height, as the atmoſphere is able to ſupport in the Torricellian experiment.

II. THE ſtanchneſs of the pipe appeared by the diminution (as to number) of bubbles, that appeared at the top of the water, and by their ſize too; for when there was a leak, (though but ſo very ſmall, that the water could not get out at it in the tube) it might uſually be taken notice of by the attentive ear of him, that ſtood to watch upon the ladder, erected by the ſide of the tube; and the air, that got in, did eaſily diſcover itſelf to the eye by large bubbles, manifeſtly differing from thoſe, that came from the aerial particles belonging to the water; and if the leak were not ſo very ſmall, the air that got in would ſuddenly lift up the water above it, and perhaps fill with it the deſcending pipe.

III. THOUGH there had been ſome imperceptible leak, yet that would not have hindred the ſucceſs of the experiment for the main. For in leaks, that have been but ſmall, though manifeſt enough, we have often, by cauſing the pump to be plied leſs nimbly than it now was, been able to proſecute our trials; becauſe the pump carried off ſtill more air than could get in at a leak that was no greater.

IV. AND that little or no intruding air was left in the upper part of our tube, was evident by thoſe marks, whereby it was eaſy for them, that are well acquainted with the pump, to eſtimate what air is left in the veſſel it ſhould exhauſt; and particularly towards the end of our operation I obſerved, that when the ſucker was depreſſed, there came out of the water, that covered the pump, ſo very few bubbles, that they might be imputed to the air afforded by the bubbles, ſpringing from the water in the tube; whereas if any adventitious air had got into that cylinder of water, it would have appeared in the water that covered the pump.

V. LASTLY, it were very ſtrange, that if the water was but caſually hindred by ſome leak from aſcending any higher, it ſhould be ſo eaſy to raiſe it to the very number of feet that our hypotheſis requires, and yet we ſhould be unable by obſtinate pumping to raiſe it one foot higher.

N. B. 1. As ſoon as we had made our experiment, and thereby found, that what was requiſite to it was in order; I ſent to give notice of it to Dr. *Wallis* and Dr. *Wren*, as perſons, whoſe curioſity makes them as well delighted with ſuch trials, as their deep knowledge makes them moſt competent judges of them. But before they could be found, and come, it being grown ſomewhat late and windy, I, that was not very well, and had tired myſelf with going up and down, could not ſtay with them ſo long as I intended, but leaving the reſt of the repeated experiments to be ſhewn them by *I. M.* (who had been very induſtrious in fitting and erecting the tube) they and their learned friend (whom they brought with them) Dr. *Millington*, told me a while after, that they alſo had found the greateſt height, to which they could raiſe the water, to be 33 foot and an half.

2. WHEN the water began firſt to appear in the glaſs, the bubbles would be, as I had foretold, exceeding numerous, ſo as to make a froth of near a foot high, if the water were newly brought, and had never been raiſed in the tube before. But if the pumping were long continued, the number and height (or at leaſt one of the two) of the aggregate of bubbles, would (as there remained fewer and fewer aerial particles in the water) be leſſer and leſſer; but their emerging did never, that I remember, wholly ceaſe.

3. AT the beginning alſo there would appear great vibrations of the water in the upper part of the tube; the riſing and the falling amounting ſometimes to a foot, or near half a yard: but theſe grew leſſer and leſſer, as thoſe of the quick-ſilver in the Torricellian experiment uſe to do.

4. ONE may uſe an ordinary pail to hold the ſtagnant water; but we rather employed a veſſel of earth, made for another purpoſe, ſomewhat ſlender, and of a cylindrical ſhape, becauſe in a narrow veſſel it is more eaſy to gueſs by the riſing and falling of the liquor, how the pump is plied, and to perceive even ſmaller leaks.

5. I muſt not forget to take notice, that though the newly named gentlemen came to me (when they had ſeen the experiment tried) within leſs than an hour after the time I had looked upon the baroſcope, and obſerved the quick-ſilver to ſtand ſomewhat beneath 29 inches, and three eighths; yet when preſently upon

upon their return I consulted the same instrument again, the mercury appeared to be sensibly risen, being somewhat (though but very little) above nine and twenty inches, and three eighths; and five or six hours after (at bed-time) I found it to be yet more considerably risen. Which may keep your lordship from wondering at what I intimated a little above, touching Monsieur *Paschal's* experiment, as well as touching the disappointment of the pump-makers endeavours. For it is not only possible, that (as I have elsewhere noted) water may be raised in the same pump, though we suppose it still equally stanch, higher at one time than at another: but it was contingent, that, in Monsieur *Paschal's* noble attempt to imitate the Torricellian experiment with water instead of quicksilver, the proportion betwixt the heights of those two liquors in their respective tubes answered so well to their specifick gravities. For, the varying weight of the atmosphere being not then, that appears, known, or consequently taken into consideration; if Monsieur *Paschal* having tried the Torricellian experiment, when the air was for instance very heavy, had tried his own experiment, when the atmosphere had been as light as I have often enough observed it to be, he might have found his cylinder of water to have been half a yard or two foot shorter than the formerly measured height of the quick-silver would have required.

I have now no more to add about this fifteenth experiment, but that it may serve for a sufficient confirmation of what I note in another treatise, against those hydraulical and pneumatical writers, who pretend to teach ways of making water pass by inflected pipes, and by the help of suction, from one side of a mountain to the other, be the mountain never so high. For, if the water be to ascend as it were spontaneously above 35 or 36 foot, a sucking pump will not ordinarily, at least here in *England*, be able to raise it.

AND now I speak of mountains, it will not be altogether impertinent to add, that if it had not been for unseasonable weather, I had thought fit to make the foregoing eleventh experiment (of elevating mercury by suction) to be tried at the top of an hill, not far from the place I then was at. For by what has been already delivered, it appears, that we might have estimated the height, to which the water may be there elevated by suction, without repeating the experiment with a thirty five foot tube, (which we could not hope for conveniency to do) by the utmost height, to which our engine could have raised mercury: and it may be of some use to be able from experiments to make some estimate (for it can scarce be an accurate one) how much it may be expected, that pumps shall (*cæteris paribus*) lose of their power of elevating water by suction, by being employed at the top of an hill, instead of being so at the bottom, or on a plain. Remembering always what I lately intimated, that even in the same place liquors will be brought to ascend by suction to a greater or less height at one time than another, according to the varying gravity of the atmosphere.

EXPERIMENT XVI.

About the bending of a springy body in the exhausted receiver.

THE cause of the motion of restitution in bodies, and consequently of that, which makes some of them springy, which far the greater part of them are not, has been ingeniously attempted by some modern corpuscularians, and especially Cartesians. But since divers learned and judicious men do still look upon the cause of elasticity, as a thing, that needs to be yet farther enquired into; and because I am not myself so well satisfied as to blame their curiosity, I held it not unfit to examine by the help of our engine their conjecture, who imagine, that the air may have a great stroke in the making of bodies springy; and this I the rather did, because I had * elsewhere shewn, that there is no need to assert, that in all bodies, that have it, the elastical power flows immediately from the form, but that in divers of them it depends upon the mechanical structure of the body.

TO make some trial therefore, whether the air have any great interest in the motion of restitution, we took a piece of whalebone of a convenient bigness and length; and having fastened one end of it in a hole made in a thick and heavy trencher, to be placed on the plate of the engine, we tied to the other end a weight, whereby the whalebone was moderately bent, the weight reaching down so near to a body placed in a level position under it, that if the spring were but a little weakened, the weight must either lean upon, or at least touch the horizontal plain: or if on the other side the spring should grow sensibly stronger, it might be easily perceived by the distance of the weight, which was so near the plain, that a little increase of it must be visible.

THIS done, we conveyed these things into the receiver, and ordered those that pumped to shake it as little as they could, that the weight might not knock against the body that lay under it, or so shake it, as to hinder us from discerning, whether or no it were depressed by the bare withdrawing of the air.

AND when the air had been well pumped out, I watched attentively, whether any notable change in the distance of the weight from the almost contiguous plain would be produced upon its being let in again: for the weight was then at rest, and the returning air flowing in much more speedily than it could before be drawn out, I thought this the likeliest time to discover, whether the absence of the air had sensibly altered the spring of the whalebone. But though the experiment were made more than once, I could satisfy myself only in this, that the depression or elevation of the weight, that was due to the true and meer change of the spring, was not very considerable, since I did not think my self sure, that I perceived any at all:

* In notes about the history of elasticity.

all: for though it be true, that ſometimes, when the receiver was well exhauſted, the weight ſeemed to be a little depreſſed, yet that I thought was very little, if any thing more than what might be aſcribed to the abſence of the air, not conſidered as a body, that had any thing to do directly with the ſpring, but as a body, that had ſome (though but a little) weight; upon which account it made the medium, wherein the experiment was tried, contribute to ſupport the weight, that bent the ſpring; which weight, when the air was abſent, muſt (being now in a lighter medium) have its gravitation increaſed by as much weight, as a quantity of the exhauſted air, equal to it in bulk, could amount to. But this experiment being tried only with whalebone, and in a receiver not very great, may deſerve to be further tried in taller glaſſes, with ſprings of other kinds, and by the motions of a watch, and other more artificial contrivances.

EXPERIMENT XVII.

About the making of mercurial, and other gages, whereby to eſtimate how the receiver is exhauſted.

BECAUSE the air being inviſible, it is not always eaſy to know, whether it be ſufficiently pumped out of the receiver that was to be exhauſted; we thought it would be very convenient to have ſome inſtrument within the receiver, that might ſerve for a gage, or ſtandard, whereby to judge whether or no it were ſufficiently exhauſted.

TO this purpoſe divers expedients were thought on, and ſome of them put in practice; which, though not equally commodious, may yet all of them be uſefully imployed, one on this occaſion, and another on that.

THE firſt (if I miſremember not) that I propoſed, was a bladder, (which may be greater or leſs, according to the ſize of the veſſel it is to ſerve for) to be very ſtrongly tied at the neck, after having had only ſo much air left in the folds of it, as may ſerve to blow up the bladder to its full dimenſions, when the receiver is very well exhauſted, and not before. But though your lordſhip will hereafter find, that I yet make uſe of ſmall bladders on certain occaſions, in which they are peculiarly convenient, yet in many caſes they do, when the glaſſes are well exhauſted, take up too much room in them, and hinder the objects, included in the receiver, from being obſerved from all the ſides of it.

ANOTHER ſort of gage was made with quick-ſilver, poured into a very ſhort pipe, which was afterwards inverted into a little glaſs of ſtagnant quick-ſilver, according to the manner of the Torricellian experiment. For this pipe being but a very few inches long, the mercury in it would not begin to deſcend, till a very great proportion of air was pumped out of the receiver; becauſe till then the ſpring of the remaining air would be ſtrong enough to be able to keep up ſo ſhort a cylinder of mercury. And this kind of gage is no bad one. But becauſe, to omit ſome other little inconveniences, it cannot eaſily be ſuſpended, (which in divers experiments 'tis fit the gage ſhould be) and the mercury in it is apt to be too much ſhaken by the motion of the engine, there was another kind of gage by ſome ingenious man (whoever he were) ſubſtituted in its place, conſiſting of a kind of ſiphon, whoſe ſhorter leg hath belonging to it a large bubble of glaſs, moſt commonly made uſe of at an illuſtrious meeting of virtuoſi; where your lordſhip having ſeen it, I ſhall not need to deſcribe it more particularly.

BUT none of the gages I had formerly uſed, nor even this laſt, having the conveniences, that ſome of my experiments require; I was fain to deviſe another, which is that I moſt make uſe of, as having advantages, ſome or other of which each of the gages already mentioned wants; for even that with ſpirit of wine, not to mention leſſer diſadvantages, hath a bubble too great to let it be uſeful in veſſels ſo ſlender, as for ſome purpoſes I divers times employ; and this ſhort cylinder of ſo light a liquor as ſpirit of wine makes the ſubſidence of the liquor be indeed a good ſign, that the receiver is well exhauſted, but gives us not an account what quantity of air may be in the receiver, 'till it be arrived at that great meaſure of rarefaction; and the ſame liquor, being upon a very ſmall leak (ſuch as would not be prejudicial to many experiments) impelled up to the top of the gage, we cannot afterwards by this inſtrument take any meaſure of the air, that gets in at the leak. But now there are divers experiments, where I deſire to ſee the phænomena that will happen, not only (or perhaps not at all) upon the uttermoſt exhauſtion of the air, but when the preſſure of it is withdrawn to ſuch or ſuch a meaſure, and alſo when the air is gradually readmitted.

TO make the gage we are ſpeaking of, take a very ſlender and cylindrical pipe of glaſs, of See Plate III. Fig. 4.
ſix, eight, ten, or more inches in length, and not ſo big as a gooſe-quill (but ſuch as we employ for the ſtems of ſealed weather-glaſſes;) and having at the flame of a lamp melted it, but not too near the middle, to make of it by bending it a ſiphon, whoſe two legs are to be not only parallel to one another, but as little diſtant any where from one another as conveniently may be. In one (which is uſually the longer) of theſe legs, there is to be left at the top, either half an inch, or a whole inch, or more or leſs than either (according to the length of the gage, or the ſcope of the experimenter) of air in its natural ſtate, neither rarified, nor condenſed; the reſt of the longer leg, and as great a part of the ſhorter as ſhall be thought fit, being to be filled with quick-ſilver. This done, there may be marks placed at the outſide of the longer, or ſealed, leg, whereby to meaſure the expanſion of the air included in the ſame leg; and theſe marks may be either little glaſs knubs, about the bigneſs of pins heads, faſtened by the help of a lamp at certain diſtances to the longer leg of the ſiphon, or elſe the diviſions of an inch made on a liſt of paper, and paſted on either to the ſiphon it ſelf, or to the ſlender frame, which on ſome occaſions we faſten the gage to.

THIS instrument being conveyed into a receiver (which for expedition sake we chuse as small as will serve the turn) the air is to be very diligently pumped out, and then notice is to be taken, to what part of the gage the mercury is depressed, that we may know, when we shall afterwards see the mercury driven so far, that the receiver, the gage is placed in, is well exhausted. And if it be much desired to know more accurately (for one may arrive pretty near the truth by guess) what stations of the mercury in the gage are answerable to the degrees of the rarefaction of the air in the receiver; that may be compassed either by calculation (which is not so easy, and supposes some hypotheses) or, though not without some trouble, by letting in the water as often as is necessary, into a receiver, whose intire capacity is first measured, and in which there may be marks made to shew, when the water to be let in shall fill a fourth part, or half, or three quarters, &c. of the cavity. For if (for instance) when the quick-silver in the gage is depressed to such a mark, you let in the water, and that liquor appears to fill a fourth part of the receiver, you may conclude, that about a fourth part of the air was pump'd out, or that a fourth part of the spring, that the whole included air had, was lost by the exhaustion, when the quick-silver in the gage was at the mark above mentioned. And if the admitted water do considerably either fall short of, or exceed the quantity you expected, you may the next time let in the water either after the mercury has a little passed the former mark, or a little before it is arrived at it. And when once you have this way obtained one pretty long and accurate gage, you will not need to take so much pains to make others, since you may divide them by the help of that one; for this being placed with any other in a small receiver, when the mercury in the standard-gage (if I may so call it) is depressed to any of the determinate divisions obtained by observation, you may thence conclude, how much the air in the receiver is rarified, and consequently by taking notice of the place, where the mercury rests in the other gage, you may determine what degree of exhaustion in a receiver is denoted by that station of the mercury in this gage.

PERHAPS I need not tell your lordship, that the ground of this contrivance was, that whereas in divers other gages, when the pump came to be obstinately plied, the expansion of the included air would be so great, that it would either drive out the liquor, especially if it were light, or in part make an escape through it; I judged, that in such an instrument, as that newly described, those inconveniencies would be avoided, because that the more the air should come to be dilated, the greater weight of quick-silver it would in the shorter leg have to raise, which would sufficiently hinder it from making that heavy liquor run over; and the same ponderousness of the liquor, together with the slenderness of the pipe, would likewise hinder the included air from getting through in bubbles.

N.B. 1. FOR most experiments, where exact measures are not required, it will not be so necessary to mark the gage at any other station of the quick-silver than that, which it is brought to by the exhaustion of the receiver; for by that alone we may know, when the air is well pumped out of the receiver, wherein the gage is included: and when one is a little used to some particular gage, one may by the subsidence of the mercury guess at the degree of the air's rarefaction, so near as may serve the turn in such experiments. But when this instrument is to be used about nice trials, where it may be thought requisite to have it divided according to one of the ways formerly proposed, it will on divers occasions be more secure (in case the maker of the gage has skill to do it) to put to the divisions rather by little knubs of glass, than by paper; because this will on such occasions be in danger either to be rubbed off, or wetted. And if glass-marks be used, it will be convenient, that every fifth, or tenth, or such ordinal number as shall be judged fit, be made of glass of a differing colour, for distinction sake, and the more easy reckoning. We sometimes for a need apply, instead of these glass-knubs, little marks of hard sealing wax, which will not be injured by moisture, as those papers will, that are pasted on: but these of wax, though in many cases useful, are not comparable to the other in all; since if they be very small, they are easily rubbed off, and if large, they make not the division exact enough, and often hide the true place of the quick-silver.

2. I shall here about the mercurial gages add only this hint, that what I proposed to myself in that contrivance was not only to estimate the air pumped out of the receiver, or that remaining in it; but also, by the help of this instrument (as elsewhere by another experiment) to measure (somewhat near) the strength of the spring of rarefied air, according to its several degrees of rarefaction; and by this observation, in concurrence with other things, I hoped we might (according to what I have elsewhere insinuated) be assisted to estimate, by the cylinder of mercury raised in the open leg, the expansion of the air included in the sealed leg: but of these things I designed in this place to give but an intimation.

3. THAT leg of the gage, that includes the air, may be sealed up either at the beginning, before the pipe be bent into a siphon, or (which is much better) after the following manner. Before you bend the pipe, draw out the end of it, which you mean to seal, to a short and very slender thread; then having made the pipe a siphon, pour into the leg, which is to remain open, as much quick-silver as you shall judge convenient, which will rise to an equal height in the other leg; out of which by gently inclining the siphon, you may pour out the superfluous mercury, (if there be any,) and when you see, that there is an inch, or half an inch (or what part you designed to leave for air) unfilled with mercury, next to the end that is to be closed; and that the rest of that leg, and as much (as you think fit)

fit) of the other is full of quick-silver, you may, by keeping the siphon in the same posture, and warily applying the slender apex abovementioned to the upper part of the flame of a lamp, blown horizontal, easily seal up that apex without cracking, or prejudicing the open leg, or considerably injuring the air hole, that was to be sealed up in the other. And this sealing of one leg must (as it is evident) keep the mercury suspended in it, though it be higher by divers inches than that in the open leg, till the withdrawing of the external air enable the included, by expanding itself, to depress the mercury in the sealed leg, and raise it in the open.

4. How the length of these mercurial gages is to be varied, according to the bigness and shape of the slender receivers they are to be employed in; and how they may easily be made either to stand upright at the bottom of the receiver, or be kept hanging in the middle, or near the top of it (as occasion may require;) and how the open end may be made to secure the mercury, in cases where that is needful, belongs not so properly to this treatise, as to the second part of the Continuation; where, if ever I trouble your lordship with it, the usefulness of this sort of gages, and the circumstances, that may advantage them, will best appear.

5. There being some experiments, wherein it is not desired, that the receiver should be near exhausted, but rather that the degrees of the air's rarefaction, which ought not to be very great, should be well measured; we may in such cases make use of gages shaped like those hitherto described, but made as long as the receiver will well admit, and furnished instead of quick-silver either with spirit of wine coloured with cocheneal, or else with the tincture of red rose-leaves, drawn only with common water, made sharp by a little either of the oil, or spirit of vitriol, or of common salt. For the lightness of these liquors in comparison of quick-silver will allow the expansions of the air included in the gage to be very manifest, and notable enough, though not half, or perhaps a quarter of the air be pumped out of the receiver.

6. You may also in such cases as these, where the receiver is large enough, and is not to be quite exhausted, make use of a mercurial gage, differing from those above described only in this, that the shorter leg need not to be above an inch, or half an inch long, before it expand itself into a bubble of about half an inch, or an inch in diameter, and having at the upper part a very short and slender unsealed pipe, at which the air may get in and out: by which contrivance you may have this convenience, that you need not include so much air, as otherwise would be requisite, at the top of the longer leg, because the mercury in the shorter cannot, by reason of the breadth of the bubble, whereinto the expansion of the air drives it, be considerably raised: upon which account it becomes more easy to estimate by the eye the degrees of the included air's rarefaction, which may be done almost as easily, as if there were water instead of mercury, provided it be remembered, that quick-silver, by reason of its ponderousness, does far more assist the dilatation of the air, than so much water would do.

EXPERIMENT XVIII.

About an easy way to make the pressure of the air sensible to the touch of those that doubt of it.

Though several of our experiments sufficiently manifest to the skilful, that the pressure of the air is very considerable; yet because some of them require peculiar glasses, and other instruments, which are not always at hand, and because there are many that think it surer to estimate the force of pressure by what they immediately feel, than by any other way; I was invited for the sake of such to employ an easy experiment, which usually proved convincing, because it operated on that sense, whereon they chiefly relied.

I caused then to be made a hollow (but strong) piece of brass, not above two or three inches high, (that it might be in a trice exhausted,) and open at both ends, whose orifices were circular and parallel, but not equal, (the instrument being made tapering, so that it might be represented by an excavated *conus truncatus*, or a gigg, with the lower part cut transversly off.) This piece of brass being cemented on, as if it were a small receiver to the engine, the person, that would not believe the pressure of the air to be near so considerable as was represented, was bidden to lay the palm of his hand upon the upper orifice; and being ordered to lean a little upon it, that so the lower part of his hand might prove a close cover to the orifice, one exsuction of the air was made by the help of the pump: and then upon the withdrawing of the greatest part of the pressure of the internal air, that before counterballanced that of the external, the hand being left alone to support the weight of the ambient air, would be pressed inwards so forceably, that though the stronger sort of men were able (though not without much ado) to take off their hands, yet the weaker sort of triers could not do it, especially if by a second suck the little receiver were better exhausted, but were fain to stay for the return of the air into the receiver to assist them.

This experiment being designed rather to convince than to punish those that were to make it, we took care, not only that the brass should be so thick, and the orifices so smooth, that no sharpness nor roughness of the metal should offend the hand; but also that the narrower orifice (which was the oftnest made use of) should be but about an inch and a quarter in diameter. But if any were desirous of a more sensible conviction, it was very easy to give it him by making the larger orifice the uppermost, which was the reason why the instrument was, as we formerly noted, made tapering. But yet this larger orifice ought not to exceed two inches, or two inches and a half in wideness, least the great weight of the air endanger

danger the breaking or considerably hurting the hand of the experimenter. Which caution I am put in mind of giving, by remembering that I once much endangered my own hand, through the mistake of him that managed the pump, who unawares to me set it on work, when, for another purpose, I had laid my hand upon the orifice of an instrument of too great a diameter.

The famous experiment of Torricellius, *mentioned in the 17th of our already published trials, is of that nobleness and importance, that though divers learned men have (but upon very differing principles) discoursed of it in print, which gives me the less mind to insist long upon it here, yet I shall not scruple to subjoin some notes concerning trials that I made, (though for want of opportunity I could not repeat them according to my custom,) which I had not met with in others, and which may serve to confirm the hypothesis made use of in this Continuation, and the treatise it belongs to.*

EXPERIMENT XIX.

About the subsidence of mercury in the tube of the Torricellian experiment to the level of the stagnant mercury.

A BAROSCOPE being included in a receiver, made of a long bolt head with the lower part of the ball cut circularly off, upon the first exsuction of the air, the quick-silver that before stood at 29 inches, (the atmosphere appearing then by a constant baroscope very light,) would fall so low as to rest at 9 or 10 inches, (for once I measured the subsidence beneath its former elevation,) and in about three sucks more it would be brought quite down to the level of the stagnant quick-silver, and somewhat below, (as it is the property of quick-silver, quite contrary to water, to rise less in a slender pipe than in a wide.) The air being let into the receiver, the quick-silver would be impelled up slowlier or faster, as we pleased, to the former height of 29 inches, or thereabouts.

N.B. 1. THAT if the air were suffered to go hastily out of the receiver, the mercury would, by virtue of the accelerated motion acquired in its descent, at the very first suck descend, till it reached within an inch or two of the stagnant mercury, though it would presently after a few risings and fallings settle at the height of 9 or 10 inches, till the next suck brought it down lower.

2. IF when the mercury was reimpelled up to its due height, those that managed the pump did, instead of rarefying the air, a little compress it, the quick-silver would by the compressed air be easily made to rise an inch or more above the former standard of 29 inches. Which circumstance I mention, not as a new thing, but to confirm (what some think strange) a passage printed, in *New Experiments*, Exper. XVII. where I mention, that if the air in the receiver, instead of being rarefyed in the engine, were a little compressed by it; the pressure of the included air, being somewhat increased by having its spring thus bent, would sustain the mercury in the Torricellian tube at a greater than the wonted height.

AND to confirm another passage in the same page, where I observed, that if the pressure of the air upon the stagnant mercury be not so great as it is wont to be, the mercury will begin to subside in a (filled and inverted) tube, which wants of the usual height; we took a glass cane, (sealed at one end,) much shorter than the due length, and having filled it with mercury, and inverted it into a glass full of stagnant mercury, we placed all in the former receiver; where the mercurial cylinder, for want of the requisite height, remained totally suspended, but upon the first or second suck it would subside, and in two or three sucks more it would fall to the level of the stagnant mercury, or a little below it. Upon the letting in of the air it would be impelled to the very top of the tube, bating an aerial bubble, which seemed to come from the mercury itself, and was so little, as not to be at all discernable, save to a very attentive eye.

THIS experiment I should not think fit here to relate, since I formerly acquainted your lordship with the subsidence of the mercury upon the withdrawing of the air from the receiver; were it not, that, in the mention of that trial, I remember I confessed to you, that I could not so free the great receiver I then used from air, but that the little, that remained or leaked in, made me unable to bring the mercury in the tube totally to subside, or fall much nearer than within an inch of the surface of the stagnant mercury, with which in our present trials that in the tube was brought to a level. Experiment XVII.

EXPERIMENT XX.

Shewing that in tubes open at both ends, when no fuga vacui *can be pretended, the weight of water will raise quick-silver no higher in slender than in large pipes.*

BECAUSE I find it, even by learned and very late writers, urged as a clear and cogent argument against those, that ascribe the phænomena of the Torricellian experiment to the weight of the external air; that it is impossible, that the air, though it were granted to be a heavy body, could sustain the quick-silver at the same height in tubes of very differing bigness, since the same air cannot equally counterpoise mercurial cylinders of such unequal weights: and because this objection is wont very much to puzzle those, that are not well acquainted with the hydrostaticks, I presume your lordship will allow me, till I can shew you some hydrostatical papers, by which the objection may appear to be but ill grounded upon the true theorems of that art, to annex the transcripts of a couple of experiments, (that I once made to remove this, supposedly insuperable, difficulty,) just as I find them registered in my note-books.

The

The FIRST TRYAL, Sept 2, 1662.

WE took a very large glass-tube, hermetically sealed at one end, and about two foot and a half in length. Into this we poured quick-silver to the height of three or four fingers. Then we took a couple of cylindrical pipes of very unequal sizes, the wider being as big again as the slenderer, and open at both ends. The lower ends of these two pipes we thrust into the quick-silver, and fastened them near their upper ends to the tube with strings, that they might not be lifted up, nor moved out of their posture, in which the convex surface of the mercury in both the pipes seemed to lie almost in a level, the tube also itself being placed upright in a frame. This done, by the help of a funnel we poured in water by degrees at the top of the tube, and observed, that as the water gravitated more and more upon the stagnant mercury, so the included mercury rose equally in both the pipes, until the tube being almost filled with water, the mercury appeared to be impelled up to, and sustained at as great a height in the big tube, as in the lesser, being in either raised about two inches above the surface of the stagnant quick-silver.

N. B. 1. HAVING caused about half the water (having no conveniency to withdraw any more) in the tube to be sucked out at the top, we observed the quick-silver in both the tubes to subside uniformly, and to re-ascend alike upon the re-affusion of the water.

2. WE endeavoured to try the experiment (for their sake, who have not the conveniency to have such tubes purposely made) in a wooden vessel, into which, when it was filled with water, we let down a flat glass furnished with stagnant mercury, whereinto, the ends of the two pipes were immersed. But the opacousness of the cylinder (which reduced us to see only from the top the reflection of the stagnant mercury,) and other impediments, disabled us to perceive the motions and stations of the mercury in the pipes, though we once made use of a candle the better to discern them.

The SECOND TRYAL.

WE took a very wide tube of glass, of about a foot long, and into it poured a convenient quantity of quick-silver. We took also two pipes of about equal length, and of that disparity in bigness, that we newly mentioned, (those pipes lately described, being indeed cut off from these we are now to speak of,) and these being filled with quick-silver, after the manner of the Torricellian experiment, were by a certain contrivance let down into the tube, and unstopped under the surface of the stagnant mercury, and then the quick-silver in the pipes falling down to its wonted station, and resting there, we poured into the tube about a foot height (by guess) of water, whereupon the quick-silver, as it before stood, as it were, in a level in both the pipes, so it was, for ought appeared to us, equally impelled up beyond its wonted station, and sustained there, both in the slender and in the bigger pipe, and upon the withdrawing of some of the water, it began to subside alike, as to sense, in them both, falling no lower in the bigger than in the slenderer. And water being a second time poured down into the tube, the mercury did in both pipes rise uniformly as before. By which, and the former experiment, it sufficiently appeared, that a gravitating liquor, as air or water, may impel, or keep up mercury to the same height in tubes, that are of very differing capacities; and that liquors ballance each other according to their altitude, and not barely according to their weight. For in this last experiment, the additional cylinder of one inch of mercury, was manifestly raised and kept up, by the water incumbent on the stagnant mercury, the other cause, whatever it were, of the mercury's suspension, being able to sustain but a cylinder shorter by an inch. And the same parcel of water did counterpoise in the differing pipes two mercurial cylinders, which though but of the same altitude, (namely about an inch) were of very unequal weight.

EXPERIMENT XXI.

Of the heights, at which pure mercury, and mercury amalgamed with tin, will stand in barometers.

CONSIDERING with myself, that if the sustentation of the quick-silver in the Torricellian experiment at a certain height depends upon the æquilibrium, which a liquor of that specifick gravity does at such a height attain to with the external air; if that peculiar and determinate gravity of the quick-silver be altered, the height of it, requisite to an æquilibrium with the atmosphere, must be altered too: considering this, I say, I thought it might somewhat confirm the hypothesis hitherto made use of, if a phænomenon so agreeable to it were actually exhibited. This I supposed performable two differing ways, namely by mixing, or as chymists speak, amalgamating mercury either with gold, to make it a mixture more heavy, or with some other metal, that might make it more light than mercury alone is. But the former of those two ways I forbore to prosecute, being where I then was unfurnished with a sufficient quantity of refined gold, for that which is coined is generally allayed with silver, or copper, or both; and therefore amalgamating mercury with a convenient proportion of pure tin, or, as the tradesmen call it, block-tin, that the mixture might not be too thick to be readily poured out into a glass-tube, and to subside in it, we filled with this amalgam a cylindrical pipe, sealed at one end, and of a fit length, and then inverted it into a little glass furnished with the like mixture. Of which tryal the event was, that the amalgam did not fall down to 29, nor even to 30 inches, but stopped at 31 above the surface of the stagnant mixture.

N. B. 1. THAT though one may expect, that the event of the experiment would be the more considerable, the greater the quantity is, that is mingled of the light metal, yet care must be taken, that the amalgam be not made too thick, lest part of it stick here and there (as we did to our trouble find it apt to do) to the inside of the pipe, by which means some aerial

corpuscles

corpuscles will meet with such convenient receptacles, as to make it very difficult, if not almost impossible, to free the tube quite from air.

2. It may perhaps be worth while to try, whether by comparing the height of the amalgam, to what it ought to be upon the score of the specifick gravities of the mercury, and the tin, mingled in a known proportion in the amalgam, any discovery may be made, whether those two metals do penetrate one another after such a manner (for there is no strict penetration of dimensions among bodies) as copper and tin have, as I elsewhere note, been by some chemists observed to do, when being melted down together, they make up a more close and specifically ponderous body, than their respective weights seemed to require.

3. That by comparing this 21st experiment with the 18th of those formerly published, it may appear, that the height of the liquor, suspended in the Torricellian experiment, depends so much upon its æquilibrium with the outward air, that it may be varied by a change of gravity in either of the two bodies that counterballance each other, whether the change be of weight in the atmosphere, or of specifick gravity in the suspended liquor.

ADVERTISEMENT.

I Should here acquaint your lordship with what I have since tried, in reference to the 18th of the printed experiments, where I mention, that I observed, by long keeping the same instrument, with which I once made the Torricellian experiment, in the same place, that the height of the suspended mercury would vary according as the weight of the atmosphere happened to change. But though about the barometer (as others have by their imitation, allowed me to call the instrument hitherto mentioned, put into a frame) I made in the year 1660 several observations, that would not perhaps be impertinent in this place, yet having long since left them with a friend, who lives far off, and not having them now in my power, I must beg your lordship's permission, to reserve them for a part of the appendix, which I doubt I shall be engaged to add to this epistle. And in the mean time, I shall not forbear to present your lordship those other papers, that I have by me, relating to the barometer; some of which will, I presume, sufficiently confirm my lately mentioned conjecture, about the cause of the variation observed in the height of the suspended mercury.

EXPERIMENT XXII.

Wherein is proposed a way of making barometers, that may be transported even to distant countries.

THINKING it a desireable thing (as I have elsewhere intimated) to be able to compare together, by the help of barometers, the weight of the atmosphere at the same time, not only in differing parts of the same country, as of *England*, but in differing regions of the world; I could not but foresee, that it would be very difficult to accomplish my desire without altering the form of the barometers I had hitherto made use of. For as these be unfit to be transported far, because that stagnant mercury would be so apt to spill; so the procuring them to be made in the places, where they are to be used, though it be no bad expedient, and such as I have divers times made use of, is liable to this inconvenience; that, besides that few will take the pains, and have the skill, requisite to make baroscopes well, though they be sufficiently furnished with glasses and mercury for that purpose; besides this, I say, except men be more than ordinarily diligent and skilful, (and perhaps though they be) it will be very difficult to be sure, that the baroscope newly made in a remote country is as good (and but as good) as that, which a man makes use of in this; in regard that at the making of the former, they are supposed to have no other baroscope to compare it with; and to be sure, they have not the same, with which it is to be compared here.

Being by these considerations invited to attempt the making of portable or travelling baroscopes (if I may so call them) I thought it requisite to endeavour these three things: the first, to make the vessel, that should contain both the sustained and the stagnant mercury, all of one piece of glass, of a like bigness: the next, to place this vessel, when filled, in such a frame, as may be easy to be transported, and yet in a reasonable measure defend the glass from external violence, no part of it standing quite out of the frame, as in all other baroscopes: and the third, so to order the vessel, that it may not be subject to be easily broken by the violent motion of the mercury contained in it.

The first of these will not seem practicable to those, that imagine (without any warrant from the hydrostaticks,) that it is as well necessary as usual, that the stagnant mercury should have a vessel much wider than the tube, wherein the mercurial cylinder is sustained; but to us the difficulty seemed much less to make the glass-part of our tube of one piece, and of a convenient shape, than afterwards to fill it.

But to do both, we took a glass cylinder, sealed at one end, and of a convenient length (as about four or five foot) and caused it by the flame of a lamp to be so bent, that, to those, that did not take notice it was sealed at one end, it seemed to be a siphon of very unequal legs, the one being three or four times longer than the other; by virtue of which figure the shorter leg may serve instead of the distinct vessel usually employed to contain the stagnant mercury. To fill this, which is not easy, one may proceed after this manner. Take a small funnel of glass, with a long and slender shank, so that it may reach three or four inches, or further, into the shorter leg of our barometrical siphon (if I may so call it;) and by this funnel pour into this shorter leg as much mercury as may reach about two or three inches in both legs; then stopping the orifice with your finger, and slowly inclining the tube, the mercury in the longer leg will gently fall to the sealed

sealed end; and the air, that was there before, will pass by it, and so make it room. The mercury in the shorter leg (which leg ought to be held uppermost) will by the same inclination of the tube fall towards the orifice; but, being by the finger that stops that, kept from falling out, if you do slowly re-erect the glass, and then make it stoop again as much as before, the mercury will pass out of the shorter leg into the longer, and join with that which was there before; and if all the mercury do not so pass, the orifice is to be stopped again with your finger, and the tube inclined as formerly. This done, the tube is to be erected, and by the help of the funnel more mercury is to be poured in, and the foregoing process of stopping the orifice, inclining the tube, &c. is to be repeated, till all the mercury pour'd into the shorter leg be brought to join with that in the longer; and then the open leg is to be furnished with fresh mercury, observing this, that the nearer the longer leg comes to the being filled, the less you must raise it from time to time, when you pour mercury into the shorter; as also, that when you see the longer leg quite full of mercury (though there be but little in the shorter) you need not pour in any more, if the longer do much exceed a yard; because upon the restoring of the tube to an erected posture, there will subside from the taller leg into the other a pretty quantity of mercury, by reason of the space at the sealed end, which will be deserted by the mercury that was there. But because it is difficult by this way, as well as by that practiced already, to fill a tube with mercury without leaving any visible bubbles; to free it from such (if any happen to be) you must once more stop the orifice with your finger, and incline, and re-erect the tube divers times, till you have thereby brought most of the smaller bubbles into one greater; (which you may if you please increase, by letting in a little air:) for by making this great bubble pass leisurely two or three times from one end of the tube to the other, it will in its passage as it were lick up all the small bubbles, and unite them to itself; which may afterwards by one inclination more of the tube be made to pass into the shorter leg, and thence into the free air.

BUT there is another sort of funnels, which if one have the skill and conveniency to make (as *I. M.* easily doth) one may very expeditiously fill the bended tubes of our portable barometers. For if you make the slender part of the funnel not streight but bended, in the form of an obtuse angle, and of such a length, that the part, which is to go into the shorter leg of our siphon, may reach to the flexure (of the siphon;) then you may, by so holding the tube, that the sealed end be somewhat lower than the other, and by pouring in mercury at the obtuse end of the angular funnel, easily make it run over the flexure into the longer leg of the siphon; provided you do now and then, as occasion requires, erect a little and shake the tube, to help the mercury to get by the air, and expel it.

BY such ways as these we have found by experience, that it is possible (though not easy) to do in such a bended glass, as our purpose requires; what, besides a very late learned writer, the diligent *Mersennus* himself, admonishes his reader, that it is not a practicable thing to do in the ordinary glasses of the Torricellian experiment, viz. to free the mercury of a streight tube from air and bubbles, so as to be able by inclining the glass to make the liquor ascend to the very top.

THE first of our three above-mentioned scopes being thus attained, it was not difficult to compass the second, by the help of a solid piece of wood, which is to be somewhat longer than the tube, and a good deal broader in the lower part than in the upper, that it may receive the shorter leg of the siphon. In such a piece of wood, which was about an inch thick, we caused to be made a gutter or channel, of such a depth and shape, that our siphon might be placed in it so deep, that a flat piece of wood (like a plained lath) might be laid upon it, without at all pressing upon, or so much as touching the glass; so that this piece of wood may serve for a cover to defend the glass, to be put on when the instrument is to be transported, and taken off again, when it is to be hung up to make observations with; the channel-piece of wood serving both for a part of a case, and for an entire frame; which may for some uses be a little more commodious, if the cover be joined (as it may easily be) to the rest of the frame, by two or three little hinges and a hasp, by whose help the case may be readily opened and shut at pleasure.

THE third thing we proposed to our selves is nothing near so easy as the second; nor have we yet had opportunity to try, whether the way we made use of will hold, if the barometer be transported into very remote parts, though by smaller removes we found cause to hope that it will succeed in greater.

THE grand difficulty to be obviated was this; that though it were easy to hinder the spilling of the mercury, by stopping the orifice of the shorter leg of our siphon, yet that would not serve the turn; for the upper part of the tube being destitute of air, if the mercury be by the motion of the instrument put to vibrate, it will be apt (for want of meeting with any air in the upper part of the tube to check its motions) to hit so violently against the top of the glass as to beat it out, or to crack some of the neighbouring parts.

TO obviate this great inconvenience our way is, to incline the tube, till the mercury be impelled to the very top of it, and yet there will remain a competent quantity in the shorter leg of the glass, if that be not at first made too short. This done, the remaining part of the shorter leg is to be quite filled up either with water or mercury, and the orifice of it is to be very carefully and firmly stopped, for which purpose we use our strong black cement: for by this means the mercury in the longer leg, having no room to play, cannot strike with violence as before, against the top of the glass. But though by many times successively shaking the baroscope we did not perceive, that it was very like to be prejudiced by the

the ſhakes it muſt neceſſarily endure in tranſportation to remote places, if due care be had of it by the way, yet till further trial have been made I ſhall not pretend to be certain of the event. But thus much of conveniency we have already found in this contrivance, that we ſent it ſome miles off, to the top of a hill, and had it brought home ſafe again, the phænomena at the top and bottom of the hill being anſwerable to what we might have expected, if we had employed another baroſcope.

See the whole baroſcope Plate V. Fig. 2.

WHEN the inſtrument is to be ſent away, the height of the mercurial cylinder (to be meaſured from the ſurface of the ſtagnant mercury in the ſhorter leg) being taken for that place, day, and hour, and compared (if it may be) with that of another good baroſcope, which is to continue in that place; as much of the gutter as is unfilled by the glaſs may be well ſtuffed with cotton, or ſome ſuch thing, to keep the glaſs the more firm in its poſture; and that the tube be not ſhaken or preſſed againſt the wood, ſome of the ſame matter may be put between the reſt of the frame and the cover, which ought to be well bound together. And when the inſtrument is arrived at the remote place, where it is to be employed, (for if it be to be ſent but a little way, it may be carried ſafely without uſing any adventitious liquor) the water, that is added, may be taken off again, by ſoaking it up with pieces of ſpunge, linnen, &c. but if inſtead of water you put in mercury, as it ought to have been put in by weight, ſo it is to be taken out, till you have juſt the weight that was put in: and it is not difficult to take out the mercury by degrees, by the help of a ſmall glaſs-pipe, ſince you may either ſuck up little by little as much as remains of the additional mercury, when by erecting the barometer, and warily unſtopping the orifice of the lower leg, as much mercury as will of of itſelf flow out is effluxed; or elſe you may take out the ſuperfluous mercury, by thruſting the lower end of the pipe into that liquor, and when it has taken in enough, ſtopping the upper end cloſe with your finger, to keep it from falling back again when you remove the pipe.

N.B. IF it ſhould happen in a long voyage, that by the numerous ſhakings of the inſtrument, there ſhould from the additional water or mercury in the ſhorter leg get up into the longer any little aerial bubble, which ſeems the only, but I hope not likely, danger in this contrivance, he that is to uſe the inſtrument, at the end of the voyage may, if he be ſkilful, free the mercury from it by the ſame way, that we lately preſcribed to free it from air, when the inſtrument was firſt filled.

I preſume I need not tell your lordſhip, that the chief uſe of his travelling baroſcope is, that he that uſes it in a remote part, keepin ga diary of the heights of the mercury, by comparing theſe heights with thoſe, at which the mercury ſtood at the ſame times in the barometer that was not removed, the agreement or difference of the weight of the atmoſphere in diſtant places may be obſerved. To which this may be added, the conveniency, which the ſtructure of theſe inſtruments gives them to be ſecurely let down into wells or mines, and to be drawn up to the top of towers, and ſteeples, and other elevated places: not here to conſider, whether by a convenient addition, theſe, as well as ſome other barometers, may not be made to diſcover even very minute alterations of the atmoſphere's preſſure.

WHETHER this travelling baroſcope, being furniſhed at its upper end with a very good ball and ſocket, and at the lower end with a great weight (which way of keeping things ſteady in a ſhip has been happily uſed by the Royal Society on another occaſion;) whether, I ſay, our inſtrument may by this contrivance, or ſome other that might be ſuggeſted to the ſame purpoſe, be made any thing ſerviceable at ſea, notwithſtanding the differing motions of the ſhip, I have had no opportunity to try: but whether it may or may not be uſeful in ſpite of the rolling of the ſhip, it may at leaſt be made uſe of in flat calms (which divers times happen in long voyages, eſpecially to the *Eaſt Indies*, and to *Africk*) and then the inſtrument, which at other times may lie by without being at all cumberſome, may be made uſe of, as long as the calm laſts, to acquaint the obſerver with the weight of the atmoſphere in the climate where he is, and that upon the ſea: which may give ſome welcome information to the curioſity of ſpeculative naturaliſts, and perhaps prove either more directly, or in its conſequences, of ſome uſe to navigators themſelves, as by enabling them by its ſudden changes to foretel the end of the calm. Beſides that, having one of theſe inſtruments ready at hand, whereever they ſet foot on ſhore, though it be but upon a ſmall iſland, or a rock, they can preſently and eaſily take notice of the gravity of the atmoſphere in that place; which whether or no, if compared with other obſervations, it may in time prove not altogether uſeleſs to the gueſſing whereabouts they are, and the foreſeeing ſome approaching changes of weather, I leave to future experience, if it ſhall be thought worth the making, to determine.

BESIDES the ordinary baroſcope, and this travelling one, I have employed two or three other inſtruments of quite differing kinds, to diſcover the varying gravities of the atmoſphere; but though they have hitherto ſucceeded well, for the main, yet being willing to make further obſervations about them, I reſerve one of them for another opportunity, and think fit to leave the other in a tract it belongs to.

A POSTSCRIPT ADVERTISEMENT.

SINCE the writing of the foregoing and the following experiments about the travelling baroſcope, having had occaſion to make one at a place about fifty miles diſtant from that where I was when I writ them, I took notice, that the mercury in the travelling baroſcope was not by $\frac{1}{4}$ of an inch ſo high as that in another baroſcope made the ordinary way; and yet it was not eaſy to perceive, that the former had been leſs carefully filled than the latter. So that I yet know not well to what cauſe

cause to impute the difference, unless it should perhaps depend upon this circumstance; that the pipe, whereof the travelling baroscope was made, was very slender, and much more so, than the tube of the other; and I have already elsewhere observed, that mercury, contrary to what happens in water, is less apt to rise in very slender pipes. And though I remember, that, at the place where I writ the experiment, to which this postscript belongs, in the tube, I then employed to make the travelling baroscope, the mercury ascended as high as in a noted one made the common way; yet not being in the other place furnished with a tube, long and big enough, I think myself obliged, till I can clear the doubt by further trial, to give your lordship this advertisement, lest either the cause already suspected, or some other unheeded thing, may in some cases make these travelling baroscopes somewhat differing from others. But though they should prove to be so, yet it would not follow, that they cannot be made serviceable: for keeping a pretty while that instrument, which suggested the scruple to me, just by the other, with which I had compared it, and carefully taking notice of the respective heights, at which the mercury rested in both; I observed, that when it rose or fell in the other barometer, it did also rise and fall in the portable one; and when it rested at its first station in the former, it did so in the latter; and though there seemed to be an inequality in the quantity of the ascent, and subsidence of the mercury in the two instruments, yet that seemed to be accountable for, by some circumstances, especially the very unequal breadth of the vessel, that contained the stagnant mercury in the other barometer, and that shorter leg, which answered to that vessel in the travelling barometer. But till the formerly proposed scruple be by further observations removed, the safest way will be to make the barometer to be sent to remote places, as like as may be (in bigness, and length of the tube) to another portable one kept at home; that so, when they are once adjusted, the collations may be made betwixt two instruments of the same kind, whereof that, which is kept at home, may also, if it be thought fit, be compared, when the observations are made, with a baroscope, made the ordinary way.

EXPERIMENT XXIII.

Confirming, that mercury in a barometer will be kept suspended higher at the top, than at the bottom of a hill. On which occasion something is noted about the height of mountains, especially the pic of Tenariff.

TO give your lordship some instance (till I can present you with a noble one) of the use of our travelling barometer, I shall now add; that when I writ the foregoing experiment, chancing to be within two or three miles of a hill, which, though not high, was the least low in that country, I thought our instrument might be safely, and not altogether uselesly, carried on horse-back to the top of it, which was too remote from the bottom to be conveniently reached by me on foot in the midst of winter. This trial therefore I resolved to make, because, though I formerly told you of a considerable one, that had been made in *France* by some eminent virtuoso of that country; yet I was willing, not only to have a proof, how safely our baroscope might be transported, but to confirm to your lordship upon our own observation, made in another region, so considerable an argument, as these kind of experiments afford to our hypothesis.

AND though when I came to try the experiment, I happened to have an indisposition, that forbid me to do it all myself; yet having carefully marked on the edge of the frame the height, to which the suspended quick-silver reached, and compared it with a good baroscope made the ordinary way, I committed our instrument to a couple of servants, that I had often employed about pneumatical and mercurial experiments, giving them particular instructions what to do. And the instrument being such as might be safely carried on horse-back, I had in two or three hours an account brought me back, the sum of which was; that they found the suspended mercury fall a little as they ascended the hill, at whose top they gave the liquor leave to settle, and carefully took notice, by a mark, of the place it rested at; which was, as I afterwards found, ¼ of an inch, or somewhat better beneath the mark I had made; and this notwithstanding the hill was not high, and the air and wind seemed to them to be much colder at the top of it, than beneath. But though, as they descended more and more, they observed the mercury to rise again higher and higher, (as being pressed against by a taller column of the atmosphere,) and though consequently the experiment agreed very well with our hypothesis, and may serve for a confirmation of it; yet by reason of the small height of the mountain, the decrement of the height of the mercurial cylinder was not not so considerable, but that I should perhaps have omitted the mention of this trial, if it did not shew, that our travelling baroscopes may be fit to be employed about such experiments. And therefore, when I can recover some of my scattered papers, I shall by way of appendix subjoin to this some other observations, that I procured to be made by ingenious men, who had the opportunity of living near higher mountains.

SOME further trials I have recommended to be hereafter made by some other inquisitive persons; and to make them the more instructive, I could wish, that others would do what I should have done, if opportunity had befriended me. For I designed to make the experiment at the bottom, the top, and the intermediate part of the whole, at three differing constitutions of air, viz. when it should appear by a good ordinary baroscope, that the atmosphere was very heavy, when it should be found to be very light, and when it should have a moderate degree of gravity: and I hoped, that if sagacious experiments should make these diversified observations on distant and unequal hills, good hints may result from the

the collations, that may be made of the varying decrements of the mercurial cylinder's height, according to the differing gravities of the atmosphere at several times, and the differing heights of the hills and stations where the observations should be made.

I also endeavoured to get a baroscope carried down to the bottoms of deep mines; partly, to try whether the atmospherical pillar being longer there than at the top, the mercury in the tube would not be impelled up higher; and partly, in order to other discoveries. But some impediments in the structure of those mines made it not very practicable to employ barometers there; which yet makes me not despair of success in some other mines, where the shafts or pits are sunk more perpendicularly.

PERHAPS I told your lordship already by word of mouth, that I have been sollicitously endeavouring to get the Torricellian experiment tried upon the pic of *Teneriff*; but hitherto I have had no account of the success of my endeavours; for which I am the more concerned, because of the eminent (if not matchless) height of that mountain, of which you may receive some satisfaction, by what I am going to subjoin about it.

An Appendix about the height of Mountains.

FORASMUCH as on the one hand, not only *Kepler*, but divers other modern writers of note, do endeavour to straiten the atmosphere, and make it lower by half than the least height, to which, according to our estimation, it should reach; and to countenance their opinion, will not allow the clouds to be often above a mile high, nor even the highest mountains to exceed two miles. And forasmuch as, on the other side, other learned men seem to make the clouds and the mountains of a stupendous height; we, who take a middle way of estimating the height of the one and the other, hold it not unfit to subjoin on this occasion some uncommon observations, in favour of our opinion, that we have obtained from inquisitive travellers.

BUT first I will subjoin a passage I have some where met with in *Ricciolus*'s *Almigestum novum*, where he, if I well remember, relates, that the rector *Metensis*, as he calls him, of the Jesuits college affirmed to him some years since, that he had measured the height of many clouds, without having found any of them higher than 5000 paces: which argues, that he met with some so high, though indeed the height of clouds must needs be very various, according the gravity or lightness, density or thinness, rest or agitation of the air, and the condition of the vapours and exhalations they consist of. And if either that be true, which we have formerly had occasion to mention concerning *Maignan*'s observation; or if it be true, that sublunary comets (for I speak not of celestial ones) are generated of exhalations of the terrestrial globe, we may well conjecture, that the atmosphere, (especially if its height be not uniform,) and even clouds, especially those that have most fumes, and fewest vapours, may reach much higher than *Cardan*, *Kepler*, and others have defined.

BUT of the height of clouds, which we have sometimes attempted to take geometrically, we may have elsewhere occasion to speak again; and therefore I shall now proceed to what I have to say concerning the height of mountains. Which being an enquiry curious and difficult enough in itself, and of some importance in the disquisition about the height of the atmosphere, it being evident, that that must reach at least as high as the tops of mountains, upon whose tops men can live; I hope it will not be unacceptable to your lordship, if having a while since, as I was intimating, had the opportunity to discourse with some credible persons, that have been upon the top of exceeding high mountains, particularly of the pic of *Teneriff*, and especially with one gentleman, who was a few days before brought to satisfy the curiosity of our inquisitive and discerning monarch, by giving him an account of his journey, I acquaint you with those of the particulars, which I learned from thence, that are the most pertinent to our present purpose. First then, whereas divers late mathematicians will not allow above two miles or half a german league, and some of them not half so much, to the height of the highest mountain; the mountain we speak of, in the island of *Teneriff*, one of the Canaries or Fortunate Islands, is so high, that though perhaps I think those travellers I have taken notice of speak with the most when they write, that the top of this mountain is be seen at sea, four degrees off, i. e. at least threescore german leagues; yet having asked the ingenious gentleman lately mentioned, Mr. *Sydenham*, from what distance the top of the sugar-loaf, or highest part of the hill, so called from its figure, could be seen at sea, according to the common opinion of seamen; he answered, that that distance was wont to be reckoned 60 sea-leagues, of three miles to a league; adding, that he himself had seen it about 40 leagues off, and yet it appeared exceeding high, and like a bluish pyramid, manifestly a great deal higher than the clouds. And what he related to me about the distance was afterwards confirmed by the answers I received from observing men of differing nations, who had sailed that way; and particularly by a noble virtuoso, skilled in the mathematicks, who was then admiral of a brave English fleet. And the abovementioned gentleman Mr. *S.* also told me, that sometimes men could from thence see the island of *Madera*, though distant from it 70 leagues; and that the great Canary, though 18 leagues off, seemed to be very near them that were on the top of the sugar-loaf, as if they might leap down upon it. Thus far Mr. *Sydenham*, by whose relation it appears, that this pic must be far higher than *Kepler* and others allow mountains to be; for else it could not be seen at sea from so great a distance. And the learnd *Ricciolus*, supposing it to be, (as some navigators report it to be,) discoverable at sea four degrees off, calculates its height measured by a perpendicular line, and allowing too for refraction, to amount to ten miles,

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which altitude alſo the accurate *Snellius* aſſigns it. But I fear this learned man may have been ſomewhat miſ-informed by the navigators he relies on, or elſe that the way of allowing for refractions is not yet reduced to a ſufficient certainty. For I do not find by thoſe, who have purpoſely gone to the top of it, that the mountain is ſo high as his calculation makes it. And whereas the ſame eminent writer reſolutely pronounces, that the height of mount *Caucaſus*, deduction being made for refraction, is 51 Bolognian miles, which are conſiderably greater than the Roman miles; I doubt not here likewiſe, though I queſtion not his ſupputations, if you grant him the grounds of them, he makes this mountain far higher than indeed it is. For the paſſage of *Ariſtotle*, on which he founds his opinion, is obſcure enough; and *Ariſtotle* himſelf does ſometimes take up reports upon hearſay, without over-ſtrictly examining their truth or probability; whereas all the navigators and travellers I have hitherto met with, (and your lordſhip knows, that I have, upon a publick account, the opportunity of meeting often with ſuch men,) do almoſt unanimouſly agree, that the pic of *Teneriff* is the higheſt mountain hitherto known in the world; and yet that is ſo far from being 15 leagues high, as ſome eminent, and even late writers would perſwade us, that it is ſcarce a ſeventh part ſo high as *Ricciolus* computes mount *Caucaſus* to be. For having aſked Mr. *Sydenham*, and others, what was the eſtimate made by the moſt knowing perſons of the iſland of the height of the hill, he told me, that his guides accounted it to be one and twenty miles high from the town called *L'oretava*, ſeated on the lower part of the hill, from which town to the ſea there is three miles of way always deſcending. But in regard that the way, which amounted to 21 miles in length, is, as other ways, whereby ſteep places are wont to be aſcended, made to wind and turn for the conveniency of travellers; I can ſcarce deduct leſs than two thirds for the crookedneſs of the way: and accordingly having aſked him, whether the perpendicular height of it had been accurately taken by any with mathematical inſtruments, he anſwered, that he could ſay nothing to that upon his own knowledge, but that a ſeaman with great confidence affirmed himſelf to have accurately enough meaſured it by obſervations made in a ſhip, and to have found the perpendicular height of the hill to be about ſeven miles. Which eſtimate agrees well enough with the calculations of *Ricciolus* and *Snellius*, if we leſſen the diſtance, from which the top of the hill is to be diſcovered, from 60 German leagues of four miles to a league, to the like number of common leagues at three miles to a league.

AND becauſe eminent writers have ſo confidently delivered prodigious things touching the height of this mountain, I will here, to confirm the eſtimate already made, add theſe particulars, which I took from the gentleman's own mouth, and which were afterwards confirmed to me by another, that went with him; and partly alſo by a third, who went up to the top at another time of the year, viz. that they begun their journey from *L'oretava* on the 18th of *Auguſt*, about 10 of the clock at night, and travelled till five in the afternoon on the monday following, reſting two hours by the way, and travelling about 10 miles of their way upon mules, which afterwards they were forced to leave, and betake themſelves to their feet. Reſting upon monday till midnight, they reſumed their journeying, and travelled until about nine the next morning, at which time they arrived at the top of the Sugar-loaf, or higheſt pile of the mountain; ſo that they travelled in all but 26 hours, in which, conſidering the ſteepneſs and ruggedneſs of the ways, and that they were forced to go above half way on foot, to which they were unaccuſtomed, it is likely enough, that the length of the way did not much, if at all, exceed the computation of the guides.

WE have ſince endeavoured, but without yet knowing what will be the ſucceſs, to have the height of this mountain carefully taken by ſkilful men. In the interim I ſhall not deny, but that if what *Ariſtotle* and other authors report of mount *Caucaſus* be true, there may be far higher mountains than the pic of *Teneriff*; eſpecially ſince there is one conſideration, which perhaps you will not think deſpicable, that I find not taken notice of by thoſe, that have written of the height of mountains; viz. that of two mountains, that meaſured by geometrical inſtruments may appear to be of the ſame height, there may yet be a great inequality; becauſe the meaſurer meaſures only from ſome plain piece of ground at the bottom of the hill to the top, whereas it may be, that the country, wherein one of thoſe mountains ſtands, may be exceedingly much higher than that, wherein the other is placed: which difference of heights in the ſeveral countries he, that is to meaſure only the height of one of the mountains, is not wont to take any notice of; and conſequently though in reſpect of the plains, adjacent to the feet of the mountains, their altitudes may be equal, yet in reſpect of the level, or ſuperficies of the terraqueous globe, conſidered as having no mountains at all but thoſe two, the height of the one may far exceed that of the other; and ſo the pic of *Teneriff* being looked upon from the level of the ſea, may be much leſs high than ſome other hills, but may appear much higher than ſome other hills, which yet protuberating above the level part of ſome country, which is itſelf generally exceeding high, may have its top more remote from the centre of the earth, than that of the pic, and would appear higher than it, if as well the one as the other were looked upon from the ſame ſuperficies of the ſea.

The like conſideration I ſince found to have been had, before me, by the learned *Ricciolus*.

BUT to return to the height of the atmoſphere; in order to the making an eſtimate of what we have conſidered as to the height of mountains, I ſhall add, that though by what has been already ſaid touching the height of the pic and other hills, it appears, that the atmoſphere reaches far higher than many learned men would hitherto allow; yet we are not to think, that the atmoſphere may not reach almost incomparably higher than the tops of mountains. Nor do I suffer my self to be concluded by what many commentators of *Aristotle* and other writers are wont to teach, touching the distinct narrow extent they allow to that sphere, within whose limits they would have the steams of the terrestrial globe to produce meteors. How far the height of mountains may make the air at the tops of them inconvenient for respiration, shall be (God permitting) considered, when I come to acquaint your lordship with my loose trials about respiration.

EXPERIMENT XXIV.

Shewing that the pressure of the atmosphere may be exercised enough to keep up the mercury in the Torricellian experiment, though the air press upon it at a very small orifice.

BY a very slight variation of the foregoing 22d experiment we may both confirm one of the most important, and the least likely truths of the hydrostaticks, and remove an objection, which, for want of the knowledge of this truth, is wont to be urged against our hypothesis even by learned men. For divers of these, when they see the same phænomena happen in the Torricellian experiment, whether it be made in the open air, or in a chamber, are forward to object, that if it were, as we say it is, the weight of the air, incumbent on the stagnant mercury, which keeps that suspended in the tube from falling down, the mercury would not be sustained at any thing near the same height in the open air, where the pillar, that is supposed to lean upon the stagnant mercury, may reach up to the top of the atmosphere, as in a close room, where they imagine, that no more air can press upon it, than what reaches directly up to the roof or cieling. And when to this it is answered, that though if a room were indeed exactly closed, the sustentation of the mercury ought to be ascribed to some other cause than the weight of the imprisoned air, which other cause I have elsewhere shewn to be its spring; yet in ordinary rooms there is still a communication between the internal and external air, either by the chimney, or, if the room have none, by some crevice in the window, or by some chink between the wall and the door, or at least by the key-hole. And when to this it is objected, that the orifice of the key-hole is much narrower than the superficies of the stagnant mercury, and consequently, though the atmosphere were not reduced to press obliquely on the mercury, yet, entering at so small an orifice, it could not press sufficiently upon it; when, I say, in answer to this objection I have alledged that hydrostatical theorem, that the pressure, in such cases as ours, is to be estimated by the heights of the liquors and not the breadths, the assertion has been thought unlikely and precarious.

TO confirm therefore this hydrostatical truth, one may take the bended tube, mentioned in the 22d experiment; and inclining it till the greatest part of the mercury pass from the shorter leg into the longer, the upper end of this shorter leg may by the flame of a lamp be drawn out so slender, that the orifice of it shall not be above an eighth or tenth part (not to say a much less) as big as it was before. For this being done, and the tube erected again, if the tall cylinder of mercury be of the usual or former height, as we have found it, it will appear congruous to our hypothesis, that the weight of the external air may exercise as much pression upon the stagnant mercury through a little hole, as when all the upper superficies of that mercury was directly exposed to it.

AND if one have not the conveniency to draw out the shorter leg as is prescribed, one may nevertheless make the trial, by carefully stopping up the orifice with a cork and cement, leaving only, or afterwards making, a very small hole for the air to pass in and out. If I had not wanted a fit instrument, I would have tried to exemplify the truth of what has been delivered, by adding to the glasses we employed to make the fifth experiment such a cover, as might be cemented on to the edge of the glass, having only a very small hole in the midst, at which the atmosphere would be reduced to exercise its pressure; and the like cover I would have made use of in the tenth experiment, about the breaking of glass-plates in the unexhausted receiver, by the bare spring of the air.

EXPERIMENT XXV.

Shewing that an oblique pressure of the atmosphere may suffice to keep up the mercury at the wonted height in the Torricellian experiment, and that the spring of a little included air may do the same.

BY adding a couple of little circumstances to the trials lately proposed, we may confirm two considerable articles of our hypothesis. For, 1. if, instead of drawing the shorter leg of our barometrical siphon, if I may so call it, directly upwards, or parallel to the longer leg, as in the foregoing experiment, you make the slender part bend off so, as that, if it were continued, it would make a right angle with the longer leg of the siphon, or else an acute angle, tending downwards; this being done, I say, if when the tube is erected the mercury rest at its wonted station, it will appear, that the pressure of the atmosphere may be exercised upon it as well obliquely, when the pipe that conveys it is either horizontal, or opens downwards. See Plate V. Fig. 3.

AND, 2. if, instead of bending this slender pipe, one seal it up hermetically, the continuance of the mercurial cylinder at the same height will shew, that the spring of a very little air, shut up with the pressure of the atmosphere upon it, (though no more than what the air here below is ordinarily exposed to by the weight of the incumbent air) is able to support as tall a cylinder of mercury as the weight of the whole atmosphere, i. e. of as much of it, as can come to exercise its pressure against the mercury. See Plate V. Fig. 4.

N. B. IF when the ſhorter leg of the baroſcope is ſealed up, you move the inſtrument up and down, the mercury will vibrate, by reaſon of the ſomewhat yielding ſpring of the impriſoned air; but becauſe of the reſiſtance of the ſpring, the motion will be diverſified after an odd and pretty manner: which may be eaſily perceived by the impreſſion it makes upon the hand, but not ſo eaſily deſcribed. And becauſe that, when the ſhorter leg is drawn out ſlender enough, after the inſtrument is furniſhed with quick-ſilver, it is eaſy to ſeal it up with the flame of a candle, without the help of any inſtrument at all, I ſhall here take notice to your lordſhip, (which I could not reaſonably do before,) that it may on ſome occaſions be convenient to ſeal up the barometer, before it be tranſported, and, in ſome caſes, to incline the tube beforehand, till the quick-ſilver have quite fill'd the longer leg: by this means the vibrations of the quick-ſilver will be leſs than otherwiſe they would be, and it will be no trouble at all, when the inſtrument is brought to the deſigned place, to break off the ſlender apex of the ſhorter leg, and ſo expoſe again the mercury to the preſſure of the atmoſphere.

As about the former experiments, ſo about theſe two this advertiſement may be given; viz. That the ſame trials, for the main, may be made without confining one's ſelf to the propoſed ways of making them.

1. FOR the firſt of theſe new trials may be made by cementing very carefully on to the orifice of the ſhorter leg (which need not be altered) a ſhort pipe of glaſs, whoſe upper end may be drawn out very ſlender, and bent either horizontally or downwards; which is far eaſier to be done, than to draw out the ſhorter leg, when the glaſs is furniſhed with mercury.

2. AND as for the ſecond trial, that may be well enough made, by carefully ſtopping the unaltered orifice of the ſhorter leg with a good cork, and our cloſe cement, or with the latter only; and when you would afterwards uſe this inſtrument as a baroſcope, you need but heat a pin or ſlender wire red hot, and ſo burn a hole through the ſtoppel.

AND this expedient, which I could not conveniently advertiſe your lordſhip of ſooner, may be of uſe, when a travelling baroſcope is to be often removed; becauſe having once ſtopped the whole orifice well, it is far more eaſy to ſtop and open a pin-hole accurately, than to cloſe and unſtop the whole orifice of the tube.

NOTE, I endeavoured to confirm more than one of the foregoing particulars by this one experiment. Having cauſed a portable barometer to be made with the ſhorter leg of a ſomewhat more than ordinary length, I afterwards cauſed the upper part of this leg to be drawn out very ſlender, as in this 25th experiment; and laſtly, I cauſed the ſame ſhorter leg to be either about or ſomewhat above the middle bended downwards, ſo that the ſmall orifice of the ſlender apex pointed towards the ground. This done, I was to have meaſured the height of the ſuſpended mercury, but not having a fit ruler at hand, I then deferred, and afterwards forgot to do it; but I remember, that neither I, nor ſome others verſed in ſuch experiments, to whom I ſhewed it, took any notice, that the mercury was leſs high than in ordinary barometers; whence it was concluded, that the atmoſphere could exerciſe his preſſure not only at a very ſmall orifice, which in our experiment did little, if at all, exceed a pin-hole, but when the air muſt at this little orifice preſs upwards to be able to preſs upon the ſurface of the ſtagnant mercury.

EXPERIMENT XXVI.

About the making of a baroſcope (but of little practical uſe) that ſerves but at certain times.

TO ſhew ſome ingenious men by a medium, that has not hitherto (that I know of) been made uſe of, that the not ſubſiding of quick-ſilver in an inverted tube, that is a little ſhorter than thirty inches, or thereabouts, does not proceed from ſuch a *fuga vacui* as the ſchools aſcribe to nature, but from the gravity of the external air, I deviſed the following experiment.

HAVING made choice of a time, when it appeared by a good baroſcope, which I had frequently conſulted for that purpoſe, that the atmoſphere was conſiderably heavy, I cauſed a glaſs-pipe, hermetically ſealed at one end, and in length about two foot and a half, to be filled with quick-ſilver, ſave a very little, wherein ſome drops of water were put, that we might the better diſcern the bubbles, if any ſhould be left after the inverſion of the tube into an open glaſs with ſtagnant mercury in it. Having by this means, though not without difficulty, freed the tube from bubbles, we ſo ordered the matter, that the quick-ſilver and the little water, that was about it, filled the tube exactly, without leaving any interval, that we could diſcern at the top, and yet the mercurial cylinder was but very little higher than that of our baroſcope was at that time.

THIS done, the newly filled pipe was left erected in a quiet place, where the liquors retained their former height for divers days. But though an ordinary ſchool-philoſopher would confidently have attributed this ſuſtentation of ſo heavy a body to nature's fear of admitting a vacuum; yet it ſeems, that either ſhe is not always equally ſubject to that fear, or ſome other cauſe of the phænomenon muſt be aſſigned; for when (a pretty while after) I had obſerved by the baroſcope, that the atmoſphere was grown much lighter than before, repairing to my ſhort tube, I found, that according to my expectation the quick-ſilver was not inconſiderably ſubſided, and had left a cavity at the top, which afterwards grew leſſer, according as the atmoſphere grew heavier.

N.B. 1. THE tube employed about this experiment may be brought to the requiſite ſhortneſs, either by wearing off a little of the glaſs at the orifice of it, or by increaſing the height of the ſtagnant mercury, into which it hath been inverted.

2. WHEN the quick-ſilver in our ſhort tube was much ſubſided, there appeared in the water

ter that swam upon it a little bubble, about the bigness of a small pin's-head; but, considering how careful we had been to free the tube from bubbles before we set it to rest, it may very well be, that this so small a bubble was not produced, till after the subsiding of the quicksilver, whereupon the aerial particles in the water became less compressed than before; not to mention, that the bubble (such as it was) appeared very much greater than it would have done, if the pressure of the atmosphere had not been kept from it by the weight of the subjacent pillar of mercury.

EXPERIMENT XXVII.

About the ascension of liquors in very slender pipes in an exhausted receiver.

WHAT I related to your lordship, in the 35th of the published experiments, about the seemingly spontaneous ascension of water in slender pipes, has occasioned the making of many trials by the curious, whereby that experiment has been not a little diversifyed. But because among those I have yet heard of, none have been made in our engine, it may not be amiss to add the following trial, which may be of use in the examen of one or two of the chief conjectures, that have hitherto been proposed about the cause of that odd phænomenon.

WE tinged some spirit of wine with cocheneal, which being put into the receiver, and the air withdrawn, did exceedingly bubble for a pretty while. Then little hollow pipes of different sizes being put into it, the red liquor ascended higher in the slenderer than the others; but upon the withdrawing of the air there scarce appeared any sensible difference in the heights of the liquor, nor yet upon the letting it in again.

AFTERWARDS two such pipes of differing sizes, being fastened together (at a distance) with cement, were let down into the same spirit of wine, when the receiver was well exhausted, notwithstanding which the liquor ascended in them, for ought we could plainly see, after the ordinary manner; only when the air was let in again, there seemed to be some little (and but very little) rising, at least, in one of the pipes. In this trial, this phænomenon was noted; that though there appeared no bubbles at all in the vesseled spirit of wine, (notwithstanding that we continued to pump,) yet there did for a pretty while arise bubbles in that part of the liquor, that was got into the slender pipes; which I guessed to proceed from the sustentation (in part) of the spirit of wine, made by the inside of the pipe, whereto it adhered.

EXPERIMENT XXVIII.

About the great and seemingly spontaneous ascension of water in a pipe filled with a compact body, whose particles are thought incapable of imbibing it.

UPON occasion of the (seemingly) spontaneous ascension of water in slender pipes of glass, I considered, that it would be easy by another way to make it rise to a far greater height than hitherto had been done; for since we had found by observation, that, *cæteris paribus*, the slenderer the little pipes were that we employed, the higher the liquor would rise in them; and since the hydrostaticks had taught us, that oftentimes, even in very crooked pipes, water would be made to ascend by the same ways (of raising it) to the same perpendicular height (or thereabouts) as in straight ones; I thought, that I might well substitute a powder, consisting of solid corpuscles heaped upon one another, and included in a glass-cane, instead of the little pipes I had hitherto used. For I considered the little intervals, that would necessarily be left between these differingly shaped, and confusedly placed corpuscles, would allow passage to the water, as did the cavities of the little pipes, and yet would in many places be straighter than the slenderest pipes I had used. And though beaten glass, or fine sand, &c. might have been employed about this experiment, yet I judged it far more convenient to make use of some metalline calx, because the operation of the fire, making a more exquisite comminution of solid bodies than our pestles are wont to do, is fit to supply us with exceeding minute granes, that intercept proportionable cavities between them.

UPON this consideration therefore (besides others to be hereafter hinted) I took a straight pipe of glass, open at both ends, and of a moderate wideness, (for it need not be very slender,) and having tied a linen-rag to one end of it, that the water might have free passage in, and the powder not be able to fall out, we carefully, and as exactly, as we could, filled the cavity with minium, (which is lead calcined, without addition, to redness;) and then having erected the tube, so that the bottom of it rested upon that of a somewhat shallow, and open mouthed glass, containing water enough to swim an inch or two above the bottom of the tube; into whose cavity it did, as I expected, insinuate itself by degrees, as appeared by a little change of colour in that part of the minium which it reached, till (the open glass being from time to time supplied with fresh liquor) it attained to the height of about 30 inches. And then, our Society expressing a curiosity to see it, and have it placed among better things, I was hindered from making any further observations with that particular glass.

This was (if I forget not) about the latter end of the year 1662.

WHEREFORE taking afterwards another tube, and some minium carefully prepared, I prosecuted the experiment, so as to make the water rise in the pipe about 40 inches above the surface of the stagnant water. I guessed it had risen higher, but, by reason that at the upper part of the minium the difference of colour was so small, as not to be easily distinguishable with certainty, I forbore to allow a greater height to the ascension of the water: nor could I, where I then was, much promote the experiment, for want of such accommodations as I desired; but about the experiment,

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as I tried it, I shall take notice of the following particulars.

I tried some other powders besides red lead, (as beaten glass, pieces of fine spunge, putty, &c.) but did not find any of them do so well; which success was yet perhaps but accidental, and therefore the trial may be repeated, especially with putty, because that being a metalline calx as well as minium, consists of very small grains, and by reason of its great whiteness, receives a greater change of colour by wetting, than minium does; in which, especially if it be very fine, the discoloration, that water makes towards the upper part of the tube, is sometimes not so easy to be clearly discerned.

2. I did indeed endeavour to remedy this inconvenience, by using, instead of meer water, tincted liquors, as ink, tincture of saffron, &c. but they seemed not to rise near so high as water alone, as if the dissolved ingredients did by degrees choak the pores of the minium.

3. TO have the grains of our powder more minute, and the smaller intervals between them, I chose not only to use the finest sort of minium I could procure, but also to sift it through a very fine searce, and to put it but by little and little into the tube, that by ramming it from time to time, it might be made to lie the closer; which expedients succeeded not ill.

4. IT seemed by a trial or two (for I am not sure the observation will always hold,) that if the tube were very slender, (as about the bigness of a swan's quill,) the experiment succeed ed not well.

5. IT may be worth while to observe, in what times the water ascends to such and such heights; for at the beginning, it will ascend much faster than afterwards, and sometimes it will continue rising 24 or 30 hours, and sometimes perhaps much longer.

6. ONE of the scopes I proposed to my self in this experiment was to discover a mistake in the explication, that some learned modern writers have given us of the cause of filtration; for, whereas they teach, that the parts of filtre, that touch the water, being swelled by the ingress of it to their pores, are thereby made to lift up the water, till it touch the superior parts of the filtre, that are almost contiguous to them; by which means, these being also wetted, and swelled, raise the water to the other neighbouring parts of the filter, till it have reached to the top of it, whence its own gravity will make it descend. But in our case, we have a filter made of solid metalline corpuscles, where it will be very hard to shew, that any such intumescence is produced, as the recited explication requires.

7. WATER ascends so few inches even in very slender pipes, as to seem much to favour their judgment, who disallow the conjecture lately entertained by some ingenious men, (particularly Mr. *H.*) about the raising of the sap in trees, after the like manner, that water is raised in slender pipes. But without fully delivering yet my thoughts of that speculation, I may take notice, that in the last tryal above recited, I made water to ascend near, if not above, 3 foot $\frac{1}{2}$; and if, by so slight an expedient, water may be made to rise as high as is necessary for the nutrition of some thousand of plants, (for such a number there is, that exceed not 3 foot $\frac{1}{2}$ in height) one may without absurdity ask, Why it is not possible, that nature, or rather the most wise author of it, may have made such contrivances in plants, as to make liquors ascend in them, to the tops of the tallest trees; especially since, besides divers things that we may already suspect, (as heat, and something equivalent to well placed valves) many others, that perhaps are not yet dreamt of, may probably concur to the effect.

8. As I formerly made, by bending the slender pipes we have been talking of, short siphons, through which the water runs, without being at first assisted by suction, so I thought fit to try, whether I could not in larger pipes, by the help of minium, make much longer siphons. But though when the orifices were turned upwards, fine minium were rammed into both the legs, and the orifices were both of them closed; yet when they came to be again turned downwards, the weight of the minium would somewhere or other (and, for the most part, at, or near, the flexure) make some such chink, or discontinuation, as to hinder the farther progress of the water. Which impediment, though I judged it superable enough, (especially by making, at the flexure, a little pipe or socket, by which both legs might be closely filled) yet for want of accommodations and leisure, it was left unsurmounted. Upon which account also, I did not satisfy my self about the success of some former tryals, as of the ascension of water into pieces of wood of differing sorts, the operation of the vicissitudes of the sun's beams, and the absence of them, upon liquors ascending in tubes filled with minium, &c.

9. WHETHER the pressure of the outward air be the cause of the ascension of liquors in our tubes, furnished with minium, is a problem, in order to whose solution, I could acquaint your lordship with a contrivance, wherewith to make some trials in our engine. But since it can scarce be well described without many words, unless you express a particular curiosity to know it, I shall not trouble you with it: and the rather, because the best way I know of examining this difficulty belongs to the second part of this Continuation, where mention is made of an attempt about it, which did not, I confess, displease me.

EXPERIMENT XXIX.

Of the seemingly spontaneous ascension of salts along the sides of glasses, with a conjecture at the cause of it.

TO the same cause, or the like, with that of the ascension of water, in slender pipes, may be probably referred an odd phænomenon, which, though I remember not to have been mentioned by any chymical or other writer, I have not unfrequently observed, as as well by chance, as in trials purposely made to satisfy my self, and others, about the truth of it.

THE phænomenon, in short, was this. That having, in wide mouthed glasses, (which should not be very deep) exposed to the air a strong solution of common sea-salt, or of vitriol, which reached not, by some inches, to the top of the glass; and having suffered much of the aqueous part to exhale away very slowly, the coagulated salt would, at length, appear to have lined the inside of the glass, and to have ascended much higher, not only than the place where the surface of the remaining water then rested at, but than the place, to which the liquor reached, when it was first poured in. And if the experiment were continued long enough, I sometimes observed this ascension of the salt to amount to some inches, and that the salt did not only line the inside of the glass, but, getting over the brim of it, cover'd the outside of it with a saline crust: which made them, that saw how little liquor remain'd in the glass, admire how it could possibly get thither.

AND though I have mentioned but the solution of vitriol, and sea-salt, because they are much easier than others to be procured, and yet the experiment succeeds better in them than in some other far less parable salts; yet they are not the only ones, by whose solutions the recited phænomenon may be exhibited.

As for the cause of this odd effect, though I shall not propose any thing about it with confidence, till I have further inquired into it, and especially till I have tried, whether the phænomenon may be produced in an exhausted receiver; yet by what I have hitherto observed, I am inclined to conjecture, that it may be referred to such a cause as that of the ascension of liquors in pipes, after some such manner as this.

FIRST, I observed, that in water, and aqueous liquors, that part of the surface, which is next the sides of the glass, is (whatever the reason of it be) sensibly more elevated than the rest of the superficies; and, if very little clippings of straw, or other such minute, and light bodies, floating upon the water, chance to approach near enough to the sides of the glass, they will be apt (which one would not expect) to run up, as it were, this ascent of water, and rest against the sides of the glass.

NEXT we may take notice, with the salt-boilers, and chymists, that sea-salt is usually wont to coagulate at the top of the water, in small and oblong corpuscles, so that as to these, it is easy to conceive to them, that have considered the first observation, how numbers of them, may fasten themselves round about to the inside of the glass. And besides sea-salt, I have found by trial divers others, if their solutions be slowly enough evaporated, that will, whilst yet there remains a good proportion of liquor, afford saline concretions at the top of the water. And the fastning of saline particles to the sides of the glass may perhaps be promoted by the coldness, that may be communicated to the corpuscles contiguous to the glass, by reason of the coldness, which the glass may be suspected to have, upon the score of its density, in comparison of water. But to proceed: I consider, that by the evaporation of the aqueous parts of the solution, the surface of the remaining liquor must necessarily subside, and those saline particles, that were contiguous to the inside of the glass and the more elevated part of the water, having no longer enough of liquor to keep them dissolved, will be apt to remain sticking to the sides of the glass, and upon the least farther evaporation of the water, will be a little higher than the greater part of the superficies of that liquor; by which means it will come to pass, that, by reason of the little inequalities, that will be on the internal surface of the adhering corpuscles of the salt, and perhaps also on the internal superficies of the glass, there will be intercepted between the salt and the glass little cavities, into which the water contiguous to the bottom will ascend or be impelled, upon such an account as that, whereon it is raised in slender pipes. And when the liquor is thus got to the top of the salt, and comes to be exposed to the air, the saline part may, by the evaporation of the aqueous, be brought to coagulate there, and consequently to increase the height of the saline film, if I may so call it; which, by the like means, may be at length brought to reach to the very top of the glass, whence it may easily be brought over to the outside of the vessel, where the natural weight of the solution will facilitate its progress downwards; and the skin of salt, together with the contiguous surface of the glass, may, at length, constitute a kind of siphon.

TO this explication it agrees well, that I have usually observed the saline film hitherto mentioned to be with great ease separable from the glass in large fleaks; which argues, that they did not stick close to one another, except in some few places, but had a thin cavity intercepted between them, through which the water might ascend.

NOR is it repugnant to this explication, that in case the water ascended, it should, as it seems, dissolve the salt. For the liquor being already upon the point of concretion, is so glutted with salt, that it can dissolve no more. Whence we may also render a reason, why, when the saline film chances to reach to the outside of the glass, the liquor divers times does not run down to the bottom, but is coagulated by the way. And I have also had a suspicion, (though I could not seasonably take notice of it before now,) that when the concretion is once begun, the film may be raised and propagated, not only by the motion of the liquor between the inside of it and the glass, but by the same liquor's insinuating it self on the outside of the film into the small chinks and crevises, intercepted between the saline corpuscles, as ink (especially if somewhat thin) rises into the slit, and along the sides of the nib of a pen, though nothing but its very point be dipt in the surface of the liquor. And by this means the impregnated solution may, as it were, climb

up

up to the top of the saline concretion, and by coagulating there add to its height.

SOME other circumstances I have noted of our phænomenon, that agree with the proposed explication; but perhaps it would not be worth while to spend more time about it. Not to examine here, whether what has been related, so as to make it probable, that ascending water may carry up wherewithal to heighten and increase the pipes, or vessels, through which it rises, may contribute any thing more than was suggested in the former 28th experiment, towards the explication of the rising and diffusing of the sap in trees.

EXPERIMENT XXX.

About an attempt to measure the gravity of cylinders of the atmosphere, so as that it may be expressed by known and common weights.

WHILST I was making the former experiments, it was more than once my wish, that by knowing the just weight of a cylinder of quick-silver of a determinate diameter, and of 29 or 30 inches high, which is near the height, that the air does usually counterballance, I might the better estimate the weight of a cylinder of the atmosphere of that diameter, and consequently make the better guesses, how near the effects of the spring of the air, as well as of its weight, produced by the help of our engine, approached to the utmost of what might have been expected, in case all the instruments employed had been perfect, and all concurrent circumstances had been favourable. And upon this account, I several times regretted my want of a long instrument of steel, or hardned iron, wherewith I many years since made an observation, that was more carefully registered than preserved, of the weight of a mercurial cylinder of a determinate height, as well as diameter; which weight I did not think so safe to determine by the help of glass tubes, because it is very difficult to have them uniformly cylindrical, and to know that they are so, in regard that they are formed but by blowing and drawing out; and, besides the inequality, that may happen to the cavity upon other accounts, it is very difficult to make the sides of the glass equally thick, and to examine whether they be so, or no.

BUT, at length, lighting upon (what I had too often wanted in the foregoing experiments) a dextrous artificer, that chanced to come for a while to the place where I then was, I endeavoured to repair my loss, as well as he could help me to do it, by causing him to turn very carefully a cylindrical piece of brass, of an inch in diameter, and 3 inches in length, and open (that it might be the better wrought) at both ends, to one of which was exactly fitted a flat bottom of the same metal, fastned very close to it with little screws on the outside; this being judged a better way, than if it had been turned all of a piece.

THIS instrument being diligently counterpoised in a trusty pair of scales, was carefully filled with mercury, which (for greater caution) we took out of a new parcel, that we had not yet employed about other experiments, and finding it to weigh 17 ounces, 1 dram, 45 grains, Troy weight, (or 137 drams 45 grains) multiplying that by 10, there will come for the weight of a mercurial cylinder, of one inch in diameter, and 30 inches in height, (and so high I have divers times seen the mercury to be in a good barometer) about 14.2 lb. (i. e. 14 lb. 2 ounces, and above three drams, Troy weight;) and almost 11.8 lb. Avoirdupoize weight (i. e. 11 lb. 12 ounces, and above 6 drams) which is a greater weight, than, without such a trial, one would easily imagine, that so short a cylinder of mercury, and much less that a cylinder of so light a body as air, being neither of them above an inch diameter, could amount to.

NOTE first, to examine at the same time the weight of the mercury, and its proportion to water, we did, before the mercury was poured into the brass-vessel, fill it with water, (after which, we wiped it dry before the mercury was put into it;) and this liquor weighing 10 drams, and 15 grains, the proportion between the mercury and the water appeared to be that of $13\frac{18}{41}$ to 1: which, though it seem somewhat of the least, yet your Lordship may remember, that I formerly told you, I had several times found the received proportion of 14 to 1, between mercury and water, to be somewhat too great; and besides that, in a vessel, whose orifice was no less than an inch in diameter, it is exceeding difficult to be sure, when it is precisely full, either of water, or mercury; because the former has a superficies considerably concave, and the the other one, that is notably convex; and though we used some little artifices (which would be troublesome here to mention) to estimate the protuberance of the one liquor, and the deficience of the other, as near the truth as could be, yet I am not sure, but there may have been a few mercurial corpuscles more than there should have been, and that consequently some small abatement may have been made, of the weight newly attributed to the whole mercurial cylinder of 30 inches.

2. I had thoughts of making use of the barrel of a gun, of a convenient length, to find the weight of a mercurial cylinder of 2 foot and $\frac{1}{2}$; but I preferred the instrument already made use of (especially not being where I could have one bored after a peculiar way) not only because I could not meet with one, whose diameter was just an inch, and consequently as convenient for calculations, and because that the barrels of guns are often bored a little tapering; but because a skilful artificer confessed to me, that they scarce ever bore such barrels, but with a four-square bit (as they call it,) which leaves the cavity too angular, or too imperfectly round; whereas if an hexahedrical bit be imployed, it will, as he affirmed, make the cavity almost as cylindrical, as can be reasonably desired. I say nothing here of making use, for our purpose, of a trunk, as they call a hollow cylinder of wood, because I elsewhere shew, that wood (at least, such as the trunks to shoot pellets with are wont to be

made of) is not of a texture close enough for such an use.

3. Because in cylinders of mercury, 30 inches is a height, which the atmosphere is seldom heavy enough to be able to counterpoise; and because 29 inches is somewhat nearer the middle between the greatest and the least heights, at which I have observed the mercury at differing times to stand in good barometers. Your lordship may, if you please, abate a thirtieth part of the weight assigned above to a mercurial cylinder of 30 inches, (though I take 29 and $\frac{1}{4}$, or thereabouts, to be somewhat a more usual height of the mercury, than precisely nine and twenty.)

4. The weight of a mercurial cylinder in an æquilibrium with the atmosphere, and of one inch in diameter, being thus settled, we may, by the help of the doctrine of proportions, and a few propositions, especially the 14th of the 11th book of *Euclid*'s Elements, easily enough calculate the weight of a cylinder of mercury of another diameter, and consequently the force of the pressure of an atmospherical pillar of the same diameter. For since according to the forenamed 14th proposition of the 11th, cylinders of equal heights are to one another as their bases; and since by the second proposition of the same 11th element, circles (such as are the bases of cylinders) are to one another, as the squares of their diameters; and since lastly, we suppose, that mercury being a homogeneous body, at least as to sense, the mercurial cylinders will have the same proportion to each other in weight, that they have in bulk; since, I say, these things are so, if, for instance, we desire to know, what will be the weight of a cylinder of 30 inches high, whose diameter is two inches, the rule will be this,

As the square of the diameter of the standard cylinder, (as I call that, whose weight is already known) is to the square of the diameter of the cylinder proposed; so will the bulk of the former cylinder be to that of the latter, and the weight of that to the weight of this.

According to which rule, the square of one inch (which is the diameter of the standard cylinder) being but one, (whereby your lordship may perceive, how much the measure I pitched on facilitates computations,) and the square of two (which is the diameter of the proposed cylinder) being four, the bulk, or solid contents of this latter cylinder, and consequently its weight, will be four times as great as those of the standard cylinder; and so, since the lesser has been already supposed to weigh 11.8 lb. avoirdupoise, the mercurial cylinder of two inches in diameter, will weigh 47.2 lb. of the same weight.

EXPERIMENT XXXI.

About the attractive virtue of the loadstone in an exhausted receiver.

SOME learned modern philosophers, that have attempted to explicate the cause and manner of magnetical attraction or coition, give such an account of it, as supposes, that the air between the two magnetical bodies, being driven away by their effluviums from between them, presses them on the parts opposite to those, where the contact is to be made; and upon some such score (for I must not now stay to deliver their theories circumstantially) the air is supposed to contribute very much to the attraction and sustentation of the iron by the loadstone: wherefore, partly to examine this opinion, and partly for some other purposes (not necessary now to be mentioned) we thought fit to make the following experiment.

We took a small, but vigorous loadstone, capped and fitted with a loose plate of steel, so shaped, that when it was sustained by the loadstone, we could hang at a little crook, that came out of the midst of it, and pointed downward, a scale, wherein to put what weights we should think fit. Into this scale we put sometimes more and sometimes less weight; and then by shaking of the loadstone as much as we guessed it would be shaken by the motion of the engine, we found the greatest weight, that we presumed it would be able to support, in spite of the agitation it would be exposed to, which proved to be, besides the iron-plate and the scale, six ounces Troy weight, to which if we added half an ounce more, the whole weight appeared too easy to be shaken off. This done, we hung the loadstone, with all the weight it sustained, at a button of glass, which we had procured to be fastened on to the top of the inside of a receiver, when it was first blown; and though, in about 12 exsuctions, we usually emptied such receivers as much as was requisite for most experiments; yet this time, to exhaust it the more accurately, we continued pumping, till we had exceeded twice that number of exsuctions; at the end of which time, shaking the engine somewhat rudely, without thereby shaking off the weight that hung at the loadstone, the iron seemed to be very near as firmly sustained by it, as before the air began to be pumped out. I said very near, rather than altogether, because that the withdrawing of the air, though it be not supposed to weaken at all the power of the loadstone precisely considered, yet it must lessen its power to sustain the steel; because this in so thin a medium must weigh heavier, than in the air, by the weight of as much air, as is equal in bulk to the appended body.

Some other magnetical trials (and also some electrical ones) I remember I attempted to make by the help of our engine; but not having the notes I took of them now at hand, I shall suspend the mentioning them, till I can give your lordship a more punctual account of them.

EXPERIMENT XXXII.

Shewing, that when the pressure of the external air is taken off, it is very easy to draw up the sucker of a syringe, though the hole, at which the air of water should succeed, be stopped.

HAVING taken notice, that some learned opposers of the modern doctrine about the weight of the atmosphere think themselves more

more than ordinarily befriended by the difficulty we find in drawing up the embolus, or sucker of a syringe, when the hole, at which the air or water should succeed, is stopped, and by the violence, with which, as soon as it is let go, it is, as they imagine, drawn back. And supposing the reason of this confidence of theirs to be, that men have not yet been able in these phænomena (as in some others) to prove the interest of the atmosphere's gravity by direct or confessedly analogous experiments; I presumed it will not be unwelcome to your lordship, if I here fortify the speculations, that have been, or may be proposed to explicate these things according to the hypothesis of the weight of the air, by what we tried to that purpose, among others, when we were making use of a syringe in our engine.

The First Trial.

We took a syringe of brass, (that metal being closer and stronger than pewter, of which such instruments are usually made,) being in length (in the barrel) about six inches, and in diameter about one inch ⅛; and having, by putting a thin bladder about the sucker, and by pouring a little oil into the cavity of the cylinder (or barrel,) brought the instrument to be stanch enough, and yet the sucker to move to and fro without much difficulty, we thrust this to the bottom (or basis) of the barrel to exclude the air; and having unscrewed, and laid aside the slender pipe of the syringe (which in this and some other trials, was like to prove not only needless, but inconvenient) we carefully stopped the orifice, to which the pipe in these instruments is wont to be screwed, and then drawing up the sucker, we let it go, to judge by the violence, with which it would be driven back again, whether the syringe were light enough for our purpose; and finding it to be so, we fastened to the barrel a ponderous piece of iron to keep it down, and then fastening to the handle of the rammer (or axle-tree of the sucker) one end of a string, whose other end was tied to the often-mentioned turning key, we conveyed this syringe, and the weight belonging unto it, into a receiver; and having pumped out the air, we then began to turn the key, thereby to shorten the string that tied the handle of the syringe to it; and, as we foretold, that the pressure of the air, lately included in the receiver, being withdrawn, we should no more find the wonted resistance in drawing up the sucker from the bottom of the cylinder, so we found upon trial, that we could very easily pull it up without finding any sensible resistance.

See Plate VI. Fig. 1.

However, having thought fit to repeat the experiment, (which we did with the like success,) lest it might be objected, that this want of resistance might proceed, as partly from our employing the turning key to raise the sucker, so principally from some unperceived leak, at which the air may be supposed to have got into the cavity of the cylinder; I thought fit not only to examine by trial, after the receiver was removed from off the pump, whether the syringe were not stanch, (upon which I found, that I could not, without some straining, draw up the sucker even a little way, and that it would be violently beaten back again,) but also in one of these experiments to make this variation; that when the receiver being exhausted, we had drawn up the sucker almost to the top of the barrel, by such a string as was purposely chosen somewhat weak, we kept the parts of the syringe in that posture, till we had open a passage to the outward air, upon whose ingress the sucker was (as we intended it should be) so forceably depressed, that it broke the string, by which it was tied to the turning key, and was violently driven back to the lower part of the barrel, and that notwithstanding these two disadvantageous circumstances; one, that the string was not so weak, but that one, whom I employed to try it before it was fastened to the syringe, made it sustain a lump of iron, that weighed between four and five pounds; and the other, that yet this string was broken long before all the air, that flowed in to fill the receiver, had got in: so that the pressure of all the admitted air would doubtless have broken a much stronger string, if we had employed such a one to resist the depression of the sucker, which will yet be more evident by a phænomenon of our syringe, that I shall presently have occasion to relate.

The Second Trial.

Containing a variation of the foregoing.

We took the syringe employed in the foregoing experiments, and having found by trial, that it was, though not perfectly, tight, nor altogether so much so as before, yet enough so for our present purpose, (since, when the orifice of the vent in the basis was stopped, if the sucker were more forceably drawn up a little way, and then let go, it would hastily return, or rather violently be impelled back towards the bottom of the barrel) we made it serve us as well as we could for the following experiment. Of this syringe we did very carefully with a cork and our cement close the vent; and then having tied to the barrel of the syringe a weight, that happened to be at hand (and to amount to two pound, and as many ounces) we suspended the rammer of the syringe by a string in a large receiver; and then causing the pump to be applied, we made 11 or 12 exsuctions of the air, without any appearance of change in the syringe. But because I had judged the above-mentioned weight sufficient, and supposed, that the little air still remaining in the receiver had yet too strong a pressure to be surmounted by it, I caused the pumping to be continued, and within two or three exsuctions more I perceived the cylinder to begin to be drawn down, though but very slowly, by the weight hanging at it (assisted by its own gravity:) and likewise tried (after having purposely stopped a-while the working of the pump) that just upon a fresh suck the descent would be manifestly accelerated. And when we had suffered the barrel and weight to slide down as far as we thought fit, we let in the external air, which, as was to be expected, raised them both again much faster than they had subsided.

N.B. THERE would not have needed any thing near so great a weight to depress the barrel of the syringe, but that it is difficult in such an instrument to make the sucker fill it accurately enough, without making it somewhat uneasy to be moved to and fro. Upon which account it was necessary, that a weight should be added, not only to surmount the pressure of the air remaining in the receiver, (which was not, nor needed to be diligently exhausted in this experiment) but to overcome that resistance, which we just now noted the inequalities of the inside of the cylinder, and those of the sucker to give to the motion of the one in or over the other. And yet for all this it is not easy, though it be not impossible, to make one of these syringes very tight, especially when the nose is well stopped, and the sucker drawn up; there being often some little air, that strains in between the sucker and the barrel, and some that will be harboured between the sucker, though thrust home, and the bottom of the barrel, besides what may lurk between the same sucker and the cork that stops the orifice of the vent. Nor were we confident, that our syringe did not at length let some aerial particles insinuate themselves into the cavity, which the depression of the barrel had made betwixt the bases of that barrel and the sucker: and in such cases we ought not to wonder, if upon the return of the air the barrel and weight be not impelled up altogether to the same height they rested at, when they were first suspended in the receiver.

2. IT agreed very well with our doctrine, that as the cylinder and weight began not to fall, till a great quantity of air had been pumped out of the receiver, so they did not begin to move upwards presently upon the freedom, that was allowed the air to return into the receiver. For till it had continued a pretty while flowing in, there was not enough of it entered to restore by its pressure the cylinder and the annexed weight to their former situation.

3. WHAT has been delivered about our experiment may be confirmed by this variation, which we made of it; that having substituted a far heavier weight instead of that lately mentioned, the depression of the barrel of the syringe succeeded two or three times one after another much sooner than formerly, viz. about the sixth, or, at most, the seventh exsuction.

EXPERIMENT XXXIII.

About the opening of a syringe, whose pipe was stopped in the exhausted receiver, and by the help of it making the pressure of the air lift up a considerable weight.

THOUGH the trial I am about to relate had not all the success I desired, yet perhaps it will not be impertinent to make mention of it, because there is not any sort of experiments, that is wont so much to persuade the generality of spectators of the great force of the pressure of the air, as those, wherein they plainly see heavy and solid bodies made to ascend, (upon the operation of the air on them) without seeing any other thing lift them up.

WE took the often mentioned syringe, and having closed up the hole at the bottom with good cement, we tied to the barrel a hollow piece of iron, that served us for a scale, into which we put divers weights one after another, trying from time to time, whether, when the sucker was forceably drawn up, and held steadily in its highest station, the weight tied to the barrel (which was held down, whilst the sucker was drawn up, and afterwards let go) would be considerably raised. And when we perceived, that the addition of half a pound, or a pound more, would make the weight too great to be so raised, we forbore to put in that increase of weight; and having tied the handle of the rammer to the turning-key, we conveyed the syringe, together with its clog, into a receiver, out of which a convenient quantity of air being pumped, we were thereby enabled easily to draw up the sucker without the cylinder; after which having let in the air, the by-standers concluded, that the weight was raised a little, which yet I would not have allowed, if we had not been able, by inclining the engine and the receiver, to make the syringe and weight a little to swing. But to make the effect more evident, I caused a two pound weight to be taken out, and then the receiver being somewhat exhausted, and the air re-admitted, the clog, when all the air was come in, was swiftly raised, and as it were snatched up from the middle to the upper part of the suspended rammer. See Plate VI. Fig. 2.

IT is no easy matter to measure, with any certainty and exectness by a syringe, the weight of an atmospherical pillar equal to it in diameter, especially if there be any imperfection in the syringe, either because the sucker does not go close enough, in which case it can scarce be stanch, or because by its pressure against the inside of the barrel, which often happens if it be too close, it hinders the sucker and barrel from sliding without resistance by one another, and consequently there is an undue resistance made to the endeavour of the atmosphere, to raise the barrel and weight. And therefore, though our syringe being, upon the account of some ill accident, less in order than it was in some of the foregoing experiments, I must not conclude, that a cylinder of the atmosphere of the same wideness with it is equipollent to no greater a weight, than that which was taken up in our trial, yet we may safely conclude, that so slender a pillar of the atmosphere is able to raise by a syringe at least such a weight, as in our experiment it actually lifted up, which amounted to above sixteen pounds (avoirdupoise weight,) for it exceeded fifteen pounds and three quarters, besides the weight of the syringe's barrel itself.

EXPERIMENT XXXIV.

Shewing, that the cause of the ascension of liquors in syringes is to be derived from the pressure of the air.

I SHALL not here trouble your lordship with what I have elsewhere proposed about the explicating of suction: but as by the lately recited

recited experiments (I mean the 31, 32, and 33) it has appeared, that it is to the preſſure of the external air that we ſhould aſcribe the difficulty of drawing up the ſucker of a ſyringe, when the pipe or the vent is ſtopped; ſo I ſhall now endeavour to ſhew, that the aſcenſion of liquors, which follow the ſucker when it is drawn up, the pipe being open, depends alſo upon the preſſure of the air, incumbent on that liquor.

IF I had been furniſhed with very tall receivers, and ſuch other glaſſes, as I could have wiſhed, I had tried the following experiments with water, as well as quick-ſilver; but for want of thoſe accommodations, I was reduced to make my experiment with the latter only of thoſe liquors, which yet will, I hope, ſufficiently make out what was intended.

The FIRST TRYAL.

See Plate VII.Fig.2. which tho' made primarily for the 39 experiment, may facilitate the conceiving of this.

We took a ſmall receiver, ſhaped almoſt like a pear, cut off horizontally at both ends, (being the ſame capped glaſs, that is elſewhere mentioned in the accounts of other experiments:) we alſo took the ſyringe formerly deſcribed, and, having faſtened on to it, with good cement, inſtead of its own braſs-pipe, a ſmall glaſs-pipe, of about half a foot in length, we put this ſyringe in at the narrower end of the receiver; to whoſe orifice was afterwards carefully cemented on the braſs-cap with the turning-key, whereto was tied by a ſtring the handle of the rammer. Then having conveniently placed upon the engine a very ſhort thick glaſs, ſhaped like a ſugar-loaf, (which was made uſe of, for want of a better) with a ſufficient quantity of quick-ſilver in it; we ſo placed the receiver over it, that the lower end of the pipe of the ſyringe reached almoſt to the bottom of this glaſs, and conſequently, was immerſed a pretty way beneath the ſurface of the quick-ſilver. We had alſo poured a little water in the upper part of the ſyringe, that no air might get in between the ſucker and the cylinder, notwithſtanding that, by ſome accident or other, the ſyringe was become ſomewhat leſs tight than before. And laſt of all, we cemented the receiver to the engine, after the uſual manner.

THAT which now remained, being to try the experiment it ſelf, in order to which all this had been done, the air was pumped out of the receiver, (and conſequently out of the little glaſs that held the mercury) and then the ſucker being warily drawn up, we could not ſee the quick-ſilver aſcend to follow it, though a little water, which, it ſeems, the outward air had thruſt in between the ſucker and the cylinder, was either raiſed, or ſtopped in the glaſs-pipe of the ſyringe, (whereof, yet much the greateſt part remained unfilled;) of which the reaſon, according to our hypotheſis, was manifeſt, namely, that the air being pumped out of the receiver, the little that remained had not ſtrength enough to preſs up ſo ponderous a liquor as the quick-ſilver into the pipe, (though even that little unexhauſted air might have ſpring enough left to raiſe a little water.) And ſince it appeared by this, that without the preſſure of the air, the quick-ſilver would not be elevated, we thought it ſeaſonable to ſhew, that, by the preſſure of the air, it would. Whereupon, the air being let ſlowly into the receiver, the mercury was quickly impelled up, at leaſt, to the top of the glaſs-pipe, (though, by reaſon of ſome unperceived leak, it was not long ſuſtained there.)

AND, for further ſatisfaction, when the experiment was to be tried over again, we ordered it to be ſo made, that might plainly be obſerved, that though, when, the receiver not being yet exhauſted, the ſucker was drawn up but one inch, the mercury would be raiſed to the upper part of the glaſs-pipe of the ſyringe, yet after the exhauſting of the receiver, though the ſucker was drawn up twice as high, there appeared no aſcenſion of the mercury in the pipe, whoſe lower part only was darkened by the little glaſs, which contained that fluid metal.

BEFORE I diſmiſs this experiment, I muſt, to make good a promiſe I made your lordſhip, acquaint you with a phænomenon, which does not a little confirm our doctrine, according to which, it was eaſy both to foreſee, and to explain it; the phænomenon was, that if, when the air was diligently pumped out of the receiver, the ſucker were endeavoured to be pulled up, it could not be ſo, without much difficulty and reſiſtance, ſuch as was formerly found when the vent of the ſyringe was ſtopped, of which in our hypotheſis the reaſon may be clearly this; that there being no common air in the receiver, to aſſiſt by its preſſure (whether immediate, or mediate) the raiſing of the ſucker, this could not be raiſed but by a force great enough to ſurmont the weight of the external air, or atmoſpherical pillar that leaned upon it. So that as the other phænomena of our experiments manifeſt, that the raiſing of liquors by a ſyringe, which is commonly aſcribed to attraction, depends upon the preſſure of the air; ſo by this phænomenon it appears, that the difficulty of opening a ſyringe, whoſe pipe is ſtopped, need not be attributed to ſuch a *fuga vacui* as vulgar philoſophers refer it to; ſince, in our caſe, the ſame difficulty was found, though the pipe were open, and the liquor it was immerſed in might have had free acceſs to the place deſerted by the ſucker.

The SECOND TRIAL.

Being a proſecution of the former attempt.

To vary as well as confirm the foregoing experiment, we cauſed the ſyringe to be tied faſt to a competently ponderous body, that might keep the cylinder unmov'd, when the ſucker ſhould be drawn up. We alſo cemented on to the vent or ſcrew at the bottom of the ſyringe, a pipe of glaſs of about two inches in length, (which ſhould have been longer, but that then there would not have been room in the receiver, for pulling up of the ſucker) and having placed the heavy body, whereto the ſyringe was tied, upon a pedeſtal of a convenient height, that the glaſs-pipe might be all ſeen beneath it, and a very low vial almoſt filled with quick-ſilver might be ſo placed under-

underneath the pipe, that the stagnant mercury reached a good way above the immersed orifice of the said pipe. These things being thus provided, and the handle of the syringe's rammer being tied with a string to the turning-key, that belonged to the brass-cover of the receiver, this vessel was cemented on to the engine, and by it exhausted after the usual manner.

WHEN this was done, we looked upon the syringe's glass-pipe abovementioned, and being able to see through it, (whereby we were certain, that it was not yet full of quick-silver) we did, by the string, draw up the sucker to a good height, but could not perceive the pipe to be filled with any succeeding mercury. Wherefore warily letting in some air, we quickly saw the mercury impelled to the very top of the pipe; and we concluded, from the quantity of quick-silver that was raised, that a pretty deal was also driven into the cavity of the cylinder.

N. B. I had once before seen the mercury ascend into the pipe, upon the letting in of the air into the emptied receiver; but it seeming somewhat difficult to me to determine, whether the sucker had been raised, because there was no mark to guide my estimate by, I thought it might be suspected, that in case the sucker had not been raised, the ascension of the quick-silver might have proceeded from hence, that the air contained in the glass-pipe, breaking out through the stagnant mercury upon the exhausting of the receiver, the quick-silver might upon the return of the air into the receiver, be pressed up into the place deserted by the air, that broke out of the pipe. Wherefore we caused a string to be tied about the rammer, as near as we could to the top of the cylinder, by which means, when the receiver was the next time exhausted, we perceived, that by drawing up the sucker, we had raised it about two inches, if not more, and yet we could not discern any mercury to follow it, (the glass-pipe still continuing transparent) until we had let some air return into the receiver.

THIS experiment, joined with those we have formerly related to have been tried with our syringe, may teach us, that if a syringe were made use of above the atmosphere, neither the stopping of the pipe would hinder the easy drawing up of the sucker, nor the drawing up of the sucker, though the pipe were not stopped; would raise by suction the liquor, which the pipe was immersed in.

POSTSCRIPT.

SINCE the last recited experiment was made, and written, finding some of our instruments to be in better order than they were when that trial was made, we thought fit to endeavour by that which follows, to repair an omission or two, that formerly we could not well avoid.

HAVING then caused such a glass-pipe, as has been lately mentioned, to be well cemented on to the syringe, (whose sucker did now move more easily, and yet fill the barrel more exactly, than before) I ordered (being to be absent for a while myself) that the pipe should be filled with spirit of wine tincted with cocheneal, that the liquor and its motions might be the better discerned, and that the pipe being filled, that air might be excluded, which would else be harboured in the pipe, which caution was omitted in the foregoing experiment. And this the person, to whom I committed it, affirmed to have been carefully done, though when he inverted the pipe thus filled into the rest of the red liquor, that was put into a vial, he could not possibly do it so well, but that a bubble of air got into the pipe, and took up some (though but a little) room there. By that time I was called upon, to see the event of the trial, and could come to look upon it, the receiver was almost quite exhausted: wherefore after I had made the pumping be continued a little longer, and perceived, that the tincted spirit was fallen down out of the pipe, and that which lay in the vial seemed almost to boil at the top, by reason of the emersion of numerous bubbles, I caused the sucker to be, by the help of the turning-key, drawn up by our estimate, about two inches and a half, notwithstanding which, we could not perceive the spirit of wine to rise in the pipe, though the pumping were before left off. For which reason, I ordered the air to be let in very leisurely, upon which we could plainly see, that the red spirit was quickly driven up to the top of the pipe; and that it was so likewise into the cavity of the barrel, appeared, when the receiver was removed, by the small quantity of liquor, that remained in the vial, and the plenty of it, which came out of the syringe.

N. B. THAT if I had not wanted dexterous artificers, to work according to a contrivance I had designed, I had attempted to imitate, by the help of the bare spring of the air, such experiments, as in the lately recited trials were made to succeed, by the help of the pressure exercised by the air upon the account of its weight.

EXPERIMENT XXXV.

Shewing, that upon the pressure of the air depends the sticking of cupping glasses to the fleshy parts they are applied to.

IT is sufficiently known, that if the air within a cupping glass be rarefied by the flame of tow, flax, or the like, burned for a little while in it, and the glass be presently clapped upon some fleshy part of a man's body, there will quickly ensue a painful and visible swelling of the part covered by the cupping glass.

IT is also known, that this experiment is wont to be urged by the schools, as a clear proof of that abhorrence of a vacuum they ascribe to nature; for, say they, the reason of this phænomenon is plainly, that the internal air of the cupping glass, præternaturally rarified by heat when the instrument is applied, that heat after a while ceasing, the succeeding cold must again necessarily condense the air; and so this contracted air being no longer able to fill the whole space it replenished before, there would ensue a vacuum, if the flesh covered by the cupping glaſs, or adjoining to it, did not ſwell into the cavity of it, to fill the place deſerted by the air.

THOSE moderns, that aſſert the weight of the atmoſphere, do thence ingeniouſly endeavour to deduce the phænomenon. And indeed, if to their hypotheſis about the airs weight the conſideration of its ſpring be added, it will be eaſy enough to explicate the phænomenon, by ſaying, that when the cupping glaſs is firſt ſet on, though much of the air it formerly contained were a little before expelled by the heat, yet the ſame heat, increaſing the preſſure of the remaining air, is the cauſe, that the abſence of the air driven out of the glaſs does not immediately occaſion ſo ſenſible a pain: but, when that adventitious agitation of the included air ceaſes, that air having now, becauſe of the paucity of its corpuſcles, but a weak ſpring, can no longer preſs upon the part covered by the cupping glaſs near ſo ſtrongly, as the outward air does by its weight preſs upon all the neighbouring parts of the fleſh: by which means, according to what we have more than once explicated already, ſome of the yielding fleſh, or other body covered by the ſkin, muſt be forceably thruſt into the cavity of the cupping glaſs, where there is leſs preſſure, than at the outſide of it. And the fibres and membranous parts being thus violently ſtretched, there muſt needs follow a ſenſible pain as well as tumour. Which tumour yet does not fill up the cupping glaſs, not only becauſe of the reſiſtance of the ſkin to be ſo far diſtended, but alſo, if the included air have not been much rarified, becauſe of the ſpring of the impriſoned air, which grows ſo much the ſtronger, by how much the ſwelling fleſh reduces the air into leſs room, as I have ſometimes tried, by applying a cupping-glaſs to quick-ſilver, or even to water, which will riſe in it but to a certain height.

BUT though by this, or ſome ſuch explication, the argument urged by the ſchools in favour of the *fuga vacui* may be ſufficiently enervated; yet it ſuited better with the deſign of this treatiſe, to propoſe ſome new experiment, to illuſtrate our hypotheſis; and though it ſeemed to be far more difficult to do it in reference to cupping glaſſes, than to other ſubjects, yet I pitched upon two different ways of experimenting; whoſe ſucceſs not diſappointing me, I ſhall now give your lordſhip an account of them.

WE took a glaſs of about one inch and a half in diameter, but a good deal longer than an ordinarily ſhaped cupping glaſs of that breadth would have been, that there might be the more room for the flame to burn in it, and rarify the air. We alſo provided a receiver ſhaped almoſt like a pear; this receiver was open at both ends; at the ſharper whereof there was but a ſmall orifice, but at the obtuſe end there roſe up a ſhort neck, whoſe orifice was wide enough to admit with eaſe the newly mentioned cupping glaſs, without touching the ſides of it, and we were not willing it ſhould be much larger, leſt it ſhould not be ſo exactly covered by the palm of the hand that ſhould be laid upon it, and leſt alſo the hand ſhould be broken or hurt by the too great weight of the atmoſphere, when the included air ſhould be withdrawn from under it.

See Plate VI. Fig. 3.

THESE things being thus prepared, and the ſmaller orifice of the receiver being faſtened with cement to the engine, I cauſed the cupping glaſs to be faſtened, with the mouth upwards, to the palm of the hand of a youth, whom your lordſhip may remember to have ſeen with me, whoſe hand ſeemed framed by nature for this experiment, being broad, ſtrong, and very plump. And having pulled the glaſs, to try whether it ſtuck well on, I cauſed him to put it into the receiver, and lay his hand ſo upon the orifice lately mentioned, that it might ſerve for a cover to it, and hinder any air from getting in between them.

THAT, which we pretended was, that the receiver being but ſmall (that it might be quickly exhauſted, and ſo not put the youth to a long pain) upon an exſuction or two made with the pump of the air about the cupping glaſs, the remaining air ſhould have its preſſure ſo far weakened, as not to be able to ſupport the cupping-glaſs; eſpecially ſince if the air without the cupping-glaſs, but yet in the receiver, ſhould be more rarified by the removal of that which had been pumped out, than the air included in the cupping glaſs was by the precedent heat, this laſt mentioned air having a ſtronger ſpring (or tendency to expand itſelf) than the external air of the receiver, the glaſs muſt needs fall down, or rather be thruſt off, though, in caſe there had been no air at all left in the cavity of the cupping glaſs, the air in the receiver would by its preſſure ſuſtain a far greater weight.

THE event of our trial agreed very well with our conjecture. For upon the firſt ſuck the cupping glaſs fell off, the weight of the atmoſphere preſſing ſo hard upon the young man's hand, that, though he be more than ordinary ſtrong, he complained he could very hardly take it off the glaſs it was almoſt thruſt into, and, a while after, that his hand was very ſore. But this laſt inconvenience became not ſo quickly very ſenſible, but that we had time to repeat our experiment, by faſtening the cupping glaſs more ſtrongly than before; ſo that he complained, that it drew in his hand very forceably; and though that part be not wont to be fleſhy, yet the tumour occaſioned by the cupping glaſs was manifeſt enough to the eye: but as before, ſo now, at the very firſt turning of the ſtop-cock, to let out the air of the receiver, the cupping glaſs fell off.

EXPERIMENT XXXVI.

About the making, without heat, a cupping glaſs to lift up a great weight.

THE other experiment I lately told your lordſhip we had made, to illuſtrate our doctrine about the cauſe of the ſticking of applied cupping glaſſes, was tried after the following manner.

WE took the braſs-hoop or ring, mentioned in the fifth and ſixth experiments, and covered it with a bladder, which was wetted to make it it the more limber, and was so tied on to it (which was easy to do) that the bottom of the bladder covered the upper orifice of the hoop, and was stretched, though not strongly, upon it, almost like the membrane, that makes the head of a drum; and the neck of the bladder was tied with a string near the middle of the lower orifice of the hoop, and in this lower part of the bladder we made two or three small holes for the air to pass in and out at. Then having placed at the bottom of the often mentioned capped receiver a thick piece of wood, that had a hole in it, to receive the neck of the bladder, we so placed the covered hoop upon this piece of wood, that the upper part of the bladder lay parallel to the horizon. This done, we suspended, at the turning-key belonging to the cap of our receiver, a blind head, as chemists call it, of glass, which for want of a true cupping glass we were fain to substitute, and which indeed was not very unlike one either for shape or size; and to the upper part of this glass we fastened a large ring of metal, the better to depress it, and make it lean strongly on the bladder.

Plate VI. Fig. 4.

THESE things being thus made ready, and the receiver cemented on to the engine, we did by the help of the turning-key let down the cupping glass (for so we shall hereafter call it) till it came almost to touch the level superficies of the bladder; and when the receiver was as far exhausted as we thought fit, but not near as far as it might have been, we let down the cupping glass a little lower, so that it leaned upon the bladder, and touched it with all the parts of its orifice: so that the cupping glass with the subjacent bladder was become an internal receiver, if I may so call it, whose air was considerably expanded, and consequently weakened as to its spring. All this being done, we warily let the air into the receiver, and thereby the air, that did surround the cupping glass, which we just now called the internal receiver, having now a stronger pressure than the air in the cupping glass could resist; the bladder, on which the cupping glass rested, was, as we looked for, thrust up a pretty way into the cavity of the glass, in which it made a conspicuous tumor; and was made to stick so close to the orifice of it, that one would have thought, that the bladder had been violently drawn in, as the skin is wont to be in the ordinary applications of cupping glasses.

AND because we took notice, that though this glass were not capacious, for it scarce held a pint of water, yet the orifice of it was not very narrow, being in diameter an inch and ¼, we thought fit in repeating the experiment to add something, that seemed odd enough, and was fit to manifest, that cupping glasses may, without heat, by the bare pressure of the external air, be more strongly fastened, than for ought we know they are by the help of flame. Having then reiterated the former experiment with this only variation, that we exhausted the receiver further than before, we took out the cupping glass and the bladder, which together with the included brass-hoop was hanging at it; and then having tied the glass to the hook of a

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good statera, and tied a large scale to the neck of the bladder, we put in by degrees weights into the scale, till we had loaded it enough to force off the bladder from the glass; which happened not till the whole weight, that tended to draw down the bladder, amounted to 35 pound, if not better, of sixteen ounces in the pound. Nor did we doubt, but that the pressure of the atmosphere would in our experiment have kept up a much greater weight, if we had, before we let in the outward air, diligently exhausted the receiver; which we had purposely forborn to do, for fear the too disproportionate pressure of the external air should break the bladder: which puts me in mind of adding, upon the by, that as more weight was put into the scale, the bladder (stretched more and more by the weight on one side, and the air on the other) appeared to swell higher in the cavity of the glass.

Plate VI. Fig. 5.

EXPERIMENT XXXVII.

Shewing, that bellows, whose nose is very well stopped, will open of themselves, when the pressure of the external air is taken off.

IT is wont by the peripateticks and others to be made a great argument for the *fuga vacui*, which they attribute to nature, that if the nose of a pair of bellows be well stopped, one cannot open them by raising the upper board from the lower. But of this another reason may be easily assigned, without determining whether there be a vacuum or no, namely the weight and pressure of the air: for when the nose of a pair of bellows, that are tight enough, is well stopped, no air being able to insinuate itself upon the disjoining of the boards into the cavity made by that disjunction, this cannot be effected, but by such a force, as is almost able,(I say almost, because ordinary bellows cannot be so well shut, but that there will remain some air in them, whose spring will facilitate the opening of them) to raise an atmospherical pillar, whose basis shall be the upper board, which is commonly so large, that a less force may serve to break common bellows, than to raise so great a weight: but if they were made strong enough, and there were applied a sufficient force to lift so great a weight, as the newly mentioned pillar of the atmosphere, the sides might be disjoined, how close and stanch soever the instrument were made.

THUS far one may argue upon the bare principle of the weight of the air, but taking in the spring of it too, I thought one might proceed so much further, that I ventured to foretel divers ingenious men, that if the pressure of the ambient air were taken off, not only it would be easy to open the bellows in spite of there being carefully stopped at the nose, but that they would fly open as it were of their own accord, without the application of any external force at all. And it was partly to justify his prediction, as well as to make a trial, I thought more considerable, that we made the following experiment.

WE caused, then, to be made a pair of bellows, differing from ordinary ones in these particulars.

See Plate VI. Fig. 6.

ticulars. First, that the boards were circular, (and so without handles) and of about six inches in diameter: 2. That there was no clack or valve: 3. That the nose was but an inch long, or less, being to be lengthened if occasion required with a pipe: 4. That the leather, which was not spared, that the instrument might be the more capacious, was not horny or very stiff, but limber. The reason of the first and third diversity was, that the bellows might be capable to be conveyed into our receiver; (for which purpose also, if there had appeared need, the nose might have been made in the uppermost of the two boards:) the reason of the second variation was, that the instrument might be the more staunch: and of the fourth, that the bases of the bellows might, as in organ-bellows, be clapped closer together, and harbour less air in the wrinkles and cavity. So that when the bellows were opened to their full extent, by drawing up the upper basis at a button purposely made in the midst of it, the bellows looked like a cylinder of sixteen or eighteen inches high; upon which resemblance I take the liberty to call both the boards, as geometricians do both the circular parts of a cylinder, bases.

BUT though these were made by an artificer, otherwise dexterous, yet it not being his trade to make bellows, nor any other man's in the town I then was in, he could not make them so tight, but that in spite of our oiling the leather, and choaking the seams with good cement, there was some little and unperceived hole or cranny, whereby some air had passage when the nose was accurately stopped: but this was not so considerable, but that if we drew up the upper basis from the lower, the external air would on all sides press the leather inwards, and so make the shape of the instrument very far from being so cylindrical, as it would be if the nose were left open.

WHEREFORE concluding, that notwithstanding this imperfection the bellows would serve, though not for both the experiments I designed, yet for one of them, we carefully stopped the nose, after we had approached the bases to one another, and conveying them into a large receiver, it quickly appeared, when the pump was set on work, that at every exsuction of the incumbent air, the air harboured in the folds of the leather, and the rest of the little cavity that could not but be left between the bases, made the upper of those bases manifestly rise, though its weight (because of the thickness and solidity of the wood) would soon after depress it again, either by driving out some of the air at some place, where the instrument was not sufficiently tight, or by making it as it were strained through the leather itself; and if the pump were agitated somewhat faster than ordinary, the expansion of the internal air would be greater than could be rendered quite ineffectual by so small a leak, and the upper part of the bellows would be soon raised to a considerable height, as would appear more evidently, if we hastily let in the external air, upon whose ingress the bases would be clapped together, and the upper of them a good way depressed. So that the imperfection of the bellows made the experiment rather more than less concluding; for since there was no external force applied to open them, if notwithstanding that some of the included air could get out of them, yet the spring of the internal air was strong enough to open the bellows, when the ambient air was withdrawn, much more would the effect have been produced, if the bellows had been perfectly stanch.

EXPERIMENT XXXVIII.

About an attempt to examine the motions and sensibility of the Cartesian Materia subtilis, *or the* Æther, *with a pair of bellows, made of a bladder, in the exhausted receiver.*

I WILL not now discuss the controversy betwixt some of the modern atomists, and the Cartesians; the former of whom think, that betwixt the earth and the stars, and betwixt these themselves there are vast tracts of space, that are empty, save where the beams of light do pass through them; and the latter of whom tell us, that the intervals betwixt the stars and planets, among which the earth may perhaps be reckoned, are perfectly filled, but by a matter far subtiler than our air, which some call celestial, and others æther. I shall not, I say, engage in this controversy; but thus much seems evident, that if there be such a celestial matter, it must make up far the greatest part of the universe known to us. For the interstellar part of the world, if I may so stile it, bears so very great a proportion to the globes, and their atmospheres too, if other stars have any, as well as the earth, that it is almost incomparably greater in respect of them, than all our atmosphere is in respect of the clouds, not to make the comparison between the sea and the fishes that swim in it.

WHEREFORE I thought it might very well deserve a heedful enquiry, whether we can by sensible experiments (for I hear what has been attemped by speculative arguments) discover any thing about the existence, or the qualifications of this so vast æther: and I hoped our curiosity might be somewhat assisted by our engine, if I could manage in it such a pair of bellows as I designed. For I proposed to myself to fasten a convenient weight to the upper basis, and clog the lower with another, great enough to keep it horizontal and immoveable; that when by the help of the turning key, frequently above-mentioned, the upper basis should be raised to its full height, the cavity of the bellows might be brought to its full dimensions. This done, I intended to exhaust the receiver, and consequently the thus opened bellows with more than ordinary diligence, that so both the receiver and they might be carefully freed from air. After which I purposed to let go the upper base of the bellows, that being hastily depressed by the incumbent weight, it might speedily enough fall down to the lower basis, and by so much, and so quickly lessening the cavity, might expel thence the matter (if any where) before contained in it, and that (if it could by this way be be done) at the whole of a slender pipe, fastened either near the bottom of the bellows, or in the upper basis; against, or over the orifice, of which pipe there was to be placed at a convenient distance, either a feather, or (if that should prove too light) the sail of a little windmill made of cards, or some other light body, and fit to be put into motion by the impulse of any matter, that should be forced out of the pipe.

BY this means it seemed not improbable, that some such discovery might be made, as would not be altogether useless in our enquiry. For if notwithstanding, the absence of the air, it should appear by the effects, that a stream of other matter, capable to set visible bodies a moving, should issue out at the pipe of the compressed bellows; it would also appear, that there may be a much subtiller body than common air, and as yet unobserved by the vacuists, or (their adversaries) the schools, that may even copiously be found in places deserted by the air; and that it is not safe to conclude from the absence of the air in our receivers, and in the upper part of those tubes, where the Torricellian experiment is made, that there is no other body left but an absolute vacuity, or (as the atomists call it) a *vacuum coacervatum*. But if on the other side, there should appear no motion at all to be produced, so much as in the feather, it seemed, that the vacuists might plausibly argue, that either the cavity of the bellows was absolutely empty, or else that it would be very difficult to prove by any sensible experiment that it was full; and, if by any other way of probation it be demonstrable, that it was replenished with æther, we, that have not yet declared for any party, may by our experiment be taught to have no confident expectations of easily making it sensible by mechanical experiments; and may also be informed, that it is really so subtle and yielding a matter, that does not either easily impel such light bodies as even feathers, or sensibly resist, as does the air itself, the motions of other bodies through it, and is able without resistance to make its passage through the pores of wood and leather, and also of closer bodies, which we find not that the air doth in its natural or wonted state penetrate.

TO illustrate this last clause I shall add, that to make the trial more accurate, I waved the use of other bellows, (especially not having such as I desired,) and caused a pair of small bellows to be made with a bladder, as a body, which some of our former experiments have evinced to be of so close a texture, that air will rather break it than pass through it: and that the bladder might no where lose its entireness by seams, we glued on the two bases, the one to the bottom, and the other to the opposite part of it, so that the neck came out at a hole purposely made for it; in the upper basis, and into the neck it was easy to insert what pipe we thought fit, binding the neck very close to it on the outside. We had likewise thoughts to have another pair of tight bellows made with a very light clack in the lower basis, that by hastily drawing up the other basis, when the receiver and bellows were very carefully exhausted, we might see by the rest, as the lifting up of the clack, whether the subtle matter, that was expelled by the upper basis in its ascent, would, according to the modern doctrine of the circle made by moving bodies, be impelled up or not.

WE also thought of placing the little pipe of the bladder-bellows (if I may so call them) beneath the surface of water exquisitely freed from air, that we might see, whether upon the depression of the bellows by the incumbent weight, when the receiver was carefully exhausted, there would be any thing expelled at the pipe, that would produce bubbles in the liquor, wherein its orifice was immersed.

TO bring now our conjectures to some trial, we put into a capped receiver the bladder accommodated as before is mentioned; and though we could have wished it had been somewhat larger, because it contained but between half a pint and a pint, yet in regard it was fine and limber, and otherwise fit for our turn, we resolved to try how it would do; and to depress the upper basis of these little bellows the more easily and uniformly, we covered the round piece of pastboard, that made the upper basis, with a pewter-plate, (with a hole in it for the neck of the bladder;) which nevertheless upon trial proved not ponderous enough, whereby we were obliged to assist it by laying on it a weight of lead. And to secure the above-mentioned feather, (which had a slender and flexible stem, and was left broad at one end, and fastened by cement at the other, so as to stand with its broad end at a convenient distance just over the orifice of the pipe,) from being blown aside to either hand, we made it to move in a perpendicular slit in a piece of pastboard, that was fastened to one part of the upper basis, as that which the feather was glued to was to another part. These things being thus provided, the pump was set a-work; and as the ambient air was from time to time withdrawn, so the air in the bladder expanded itself so strongly, as to lift up the metalline weight, and yet in part to sally out at the little glass-pipe of our bellows, as appeared by its blowing up the feather, and keeping it suspended till the spring of the air in the bladder was too far weakened to continue to do as it had done. In the mean time we did now and then, by the help of a string fastened to the turning key, and the upper basis of the bellows, let down that basis a little, to observe how upon its sinking the blast against the feather would decrease, as the receiver was further and further exhausted. And when we judged it to be sufficiently freed from air, we then let down the weight, but could not perceive, that by shutting of the bellows, the feather was at all blown up, as it had been wont to be, though the upper basis were more than usually depressed. And yet it seems somewhat odd, that when, for curiosity, in order to a further trial, the weight was drawn up again, as the upper basis was raised from the lower, the sides of the bladder were sensibly (though not very much) pressed, or drawn inwards.

See Plate VI. Fig. 7.

inwards. The bellows being thus opened, we let down the upper basis again, but could not perceive, that any blast was produced; for though the feather, that lay just over and near the orifice of the little glass-pipe, had some motion, yet this seemed plainly to be but a shaking and almost vibrating motion (to the right and left hand,) which it was put into by the upper basis, which the string kept from a smooth and uniform descent; but not to proceed from any blast issuing out of the cavity of the bladder. And for further satisfaction, we caused some air to be let into the receiver, because there was a possibility, that unawares to us the slender pipe might by some accident be choaked: but though upon the return of the air into the receiver, the bases of the bellows were prest closer together, yet it seemed, that, according to our expectation, some little air got through the pipe into the cavity of the bladder: for when we began to withdraw again the air we had let into the receiver, the bladder began to swell again, and upon our letting down the weight, to blow up and keep up the feather, as had been done before the receiver had been so well exhausted. What conjecture the opening and shutting of our little bellows, more than once or twice, without producing any blast sensible by the raising of the feather, gave some of the by-standers, may be easily guessed by the preamble of this experiment; but whilst I was endeavouring to prosecute it for my own farther information, a mischance, that befel the instrument, kept me from giving my self the desired satisfaction.

Experiment XXXIX.

About a further attempt to prosecute the inquiry proposed in the foregoing Experiment.

Considering with my self, that by the help of some contrivances not difficult, a syringe might be made to serve, as far as our present occasion required, instead of a pair of bellows; I thought it would not be improper to try a differing, and, in some regards, a better way to prosecute an attempt, which seemed to me, to deserve our curiosity.

See Plate VII. Fig. 1. I caused then to be made, for the formerly mentioned syringe, instead of its streight pipe, a crooked one; whose shorter leg was parallel to the longer. And this pipe was for greater closeness, after it was screwed on carefully, fastened with cement to the barrel; and because the brass-pipe could scarce be made small enough, we caused a short, and very slender pipe of glass to be put into the orifice of the shorter leg, and diligently fastened to it with close cement. Then we caused the sucker (by the help of oyl, water, and moving it up and down) to be made to go as smoothly as might be, without lessening the stanchness of the syringe. After this, there was fastened to the handle of the rammer a weight, made in the form of a ring, or hoop, which by reason of its figure, might be suspended from the newly mentioned handle of the rammer, and hang loose on the outside of the cylinder, and which both by its figure, and its weight, might evenly and swiftly enough depress the sucker, when that being drawn up the weight should be let go. This syringe thus furnished was fastened to a broad and heavy pedestal, to keep it in its vertical posture, and to hinder it from tottering, notwithstanding the weight that clogged it. And besides all these things, there was taken a feather, which was about two inches long, and of which there was left at the end a piece about the breadth of a man's thumb-nail, (the rest on either side of the slender stalk, if I may so call it, being stript off) to cover the hole of the slender glass-pipe of the syringe; for which purpose the other extreme of it was so fastened with cement to the lower part of the syringe, (or to its pedestal,) that the broad end of the feather was placed (as the other feather was in the foregoing experiment) just over the little orifice of the glass, at such a convenient distance, that when the sucker was a little (though but very little) drawn up and let go again, the weight would depress it fast enough to blow up the broad part of the feather, as high as was permitted by the resistance of the stalk, (and that was a good way) the spring of which would presently restore the whole feather to its former position.

All these things being done, and the handle of the rammer being tied to the turning-key of a capped receiver, the syringe and its pedestal were inclosed in a capacious receiver, (for none but such an one could contain them, and give scope for the rammer's motions) and the pump being set on work, we did, after some quantity of air was drawn out, raise the sucker a little by the help of the turning-key, and then turning the same key the contrary way, we suffered the weight to depress the sucker, that we might see at what rate the feather would be blown up; and finding, that it was impelled forceably enough, we caused the pumping to be so continued, that a pretty many pauses were made, during each of which we raised and depressed the sucker as before, and had the opportunity to observe, that as the receiver was more and more exhausted of the air, so the feather was less and less briskly driven up, till at length, when the receiver was well emptied, the usual elevations and depressions of the sucker would not blow it up at all that I could perceive, though they were far more frequently repeated than ever before; nor was I content to look heedfully my self, but I made one, whom I had often employed about pneumatical experiments, to watch attentively, whilst I drew up, and let down the sucker, but he affirmed, that he could not discern the least beginning of ascension in the feather. And indeed to both of us it seemed, that the little and inconsiderable motion, that was sometimes (not always) to be discerned in the feather, proceeded not from any thing, that issued out of the pipe, but from some little shake, which it was difficult not to give the syringe and pedestal, by the raising and depressing of the sucker.

And that, which made our phænomenon the more considerable, was, that the weight, that carried down the sucker being still the same, and the motions of the turning-key being easy to be made equal at several times, there seemed no reason to suspect, that contingencies did much (if at all) favour the success; but there happened a thing, which did manifestly enough disfavour it. For I remember, that before the syringe was put into the receiver, when we were trying how the weight would depress it, and it was thought, that though the weight were conveniently shaped, yet it was a little of the least, I would not alter it, but foretold, that when the air in the cavity of the syringe (that now resisted the quickness of its descent, because so much air could not easily and nimbly get out at so small a pipe) should be exhausted with the other air of the receiver, the elevated sucker would fall down more easily, which he, that was employed to manage the syringe whilst I watched the feather, affirmed himself afterwards to observe very evidently. So that when the receiver was exhausted, if there had been in the cavity of the syringe a matter as fit as air to make a wind of, the blast ought to have been greater, because the celerity, that the sucker was depressed with, was so.

AFTER we had long enough tried in vain to raise the feather, I ordered some air to be let into the receiver; and though when the admitted air was but very little, the motions of the sucker had scarce, if at all, any sensible operation upon the feather, yet when the quantity of air began to be somewhat considerable, the feather began to be a little moved upwards, and so by letting in air not all at once, but more and more from time to time, and by moving the sucker up and down in the intervals of those times of admission, we had the opportunity to observe, that as the receiver had more air in it, the feather would be more briskly blown up.

BUT not content with a single trial of an experiment of this consequence, we caused the receiver to be again exhausted, and prosecuted the trial with the like success as before, only this one circumstance, that we added for confirmation, may be fit to be here taken notice of. Having, after the receiver was exhausted, drawn up and let fall the sucker divers times ineffectually; though hitherto we had not usually raised it any higher at a time, than we could by one turn of the hand, both because we could not so conveniently raise it higher by the hand alone, and because we thought it unnecessary, since that height sufficed to make the air briskly toss up the feather; yet *ex abundanti* we now took an instrument, that was pretty long, and fit so to take hold on the turning-key, that we could easily raise the sucker between two and three inches, by our estimate, at a time, and nimbly depress it again; and for all this, which would much have increased the blast, if there had been a matter fit for it in the cavity of the syringe; we could not sensibly blow up the feather, till we had let a little air into the receiver.

TO be able to make an estimate of the quantity of air pumped out, or let in, when the feather was strongly or faintly, or not at all raised by the fall of the sucker; we took off the receiver, and conveyed a gage into it, but though for a while we made some use of our gage, yet a mischance befalling it before the operation was quite ended, I shall forbear to add any thing concerning that trial, and proceed to say something of another attempt, wherein, though I foresaw and met with such difficulties, as kept me from doing altogether what I desired, yet the success being almost as good as could be expected, I shall venture to acquaint your lordship with the trial, which was this.

INSTEAD of the hitherto employed pipe of brass, there was well fastened, with cement, to the syringe, a pipe of glass, whose figure differed from that of the other in this particular, that the shorter, or remoter, leg of our new pipe, after it had for a while been carried parallel to the other leg, was bent off so, that above an inch and a half of it tended downwards, that the orifice of it might be immersed into water, contained in a small open jar. The design of which contrivance was, that when the receiver should be well exhausted, we might, according to what I told your lordship was at first designed, try whether by the raising and depressing of the sucker any such matter would be driven out at the nose of the pipe, as would produce bubbles in the incumbent water, which air (though highly rarefied, perhaps to some hundreds of times beyond its wonted dimensions) is capable of doing. And I chuse to employ rather water than quick-silver, because though by using the latter, I might hope to be less troubled with bubbles, yet the ponderousness and opacity of it seemed to outweigh that convenience. See Plate VII. Fig. 3.

I need not tell your lordship, that in other respects this experiment was made like the former; so that I shall mention only its peculiarities, which were, that as the air was pumped out of the receiver, that in the glass pipe made its way through the water in bubbles; and a little air having once by a small leak got in, and forced some of the water out of the jar into the pipe, when the receiver was again well emptied, both that water, and even the little quantity of stagnant water, that was contained in the immersed part of the pipe, produced so many bubbles of several sizes, as quite disturbed our observations. Wherefore we let alone the receiver, exhausted as it was, for six or seven hours, to give the water time to be freed from air; and then causing what air might have stolen in to be again pumped out, till we had perceived by the gage, that the receiver was well exhausted, we caused the sucker of the syringe; to be raised and depressed divers times; and though even then a bubble would now and then make our observations troublesome, and less certain, yet it seemed to us, that when we were not thus confounded, we sometimes observed, that the elevation and fall of the sucker, though reiterated, did not drive out at the pipe any thing, that made any discernable bubbles in the incumbent water; for though there would appear now and then some small bubbles on the surface of the water, yet

I could not perceive, that the matter that made them iſſued out at the pipe; and ſome of them manifeſtly proceeded from aerial particles, till then lurking in the water, as I concluded from the place and time of their riſing. But this non-eruption of bubbles at the noſe of the pipe was not that, which gave me the moſt of ſatisfaction. For at length both I and another had the opportunity to obſerve the water in the immerſed part of the pipe, which was very ſlender, to be about an inch higher than the reſt of the ſtagnant water, and to continue at that height or place in the pipe, though the ſucker were divers times together raiſed and depreſſed, by gueſs, between two and three inches at a time. Which ſeemed to argue, either that there was a vacuum in the cavity of the ſyringe, or elſe, that, if it were full of æther, that body was ſo ſubtle, that the impulſe it received from the falling ſucker would not make it diſplace a very little thread (perhaps not exceeding a grain in weight) of water, that was in the ſlender pipe, though it appeared by the bubbles, that ſometimes diſcloſed themſelves in the water, after the receiver had been exhauſted, that far more water would be diſplaced and carried up by a ſmall bubble, conſiſting of ſuch rarified air, that according to my eſtimate, the aerial particles of it did not, before the pump was begun to be ſet on work, take up in the water a five-hundredth part of the quantity of a pins-head.

BUT whilſt we were conſidering what to do further in our trial, a little air, that ſtrained in at ſome ſmall undiſcoverable leak, drove the water into the emptied part of the pipe, and put an end for that time to our trial, which had been too toilſome to invite us then to reiterate it.

I had indeed thoughts of proſecuting the enquiry, by dropping from the top of the exhauſted receiver light bodies conveniently ſhaped, to be turned round, or otherwiſe put out of their ſimpleſt motion of deſcent, if they met with any reſiſtance in their fall; and by making ſuch bodies move horizontally and otherwiſe in the receiver, as would probably diſcover, whether they were aſſiſted by the medium. And other contrivances and ways I had in my thoughts, whereby to proſecute our enquiry; but wanting time for other experiments, I could not ſpare ſo much as was neceſſary to exhauſt large receivers ſo diligently, as ſuch nice trials would exact; and therefore I reſolved to deſiſt, till I had more leiſure than I then had, or have ſince been maſter of.

IN the interim, thus much we ſeem to have already diſcovered by our paſt trials, that if when our veſſels are very diligently freed from air, they are full of æther, that æther is ſuch a body, as will not be made ſenſibly to move a light feather by ſuch an impulſe as would make the air manifeſtly move it, not only whilſt it is no thinner than common air, but when it is very highly rarefied, (which, if I miſtake not, it was in our experiment ſo much, as to be brought to take up above an hundred times more room than before).

And one thing more we gained by the trial made with water, namely, a clear confirmation of what I have delivered in the 34th experiment, about the cauſe of the ſuction that is made by ſyringes; for your Lordſhip may remember, that at the cloſe of the experiment we have all this while been reciting, I obſerved, that when the external air was ſo very well withdrawn, the pulling up of the ſucker would not make the ſtagnant water, that the pipe of the ſyringe was immerſed in, to aſcend one inch, or ſo much as the tenth part of it.

EXPERIMENT XL.

About the falling, in the exhauſted receiver, of a light body, fitted to have its motion viſibly varied by a ſmall reſiſtance of the air.

PARTLY to try, whether in the ſpace deſerted by the air, drawn out of our receivers, there would be any thing more fit to reſiſt the motion of other light bodies through it, than in the former experiment we found it to impel them into motion; and partly for another purpoſe to be mentioned by and by, we made the following trials.

WE took a receiver, which, though leſs tall than we would have had, was the longeſt we could procure: and that we might be able, not ſo properly to let down, as to let fall a body in it, we ſo faſtened a ſmall pair of tobacco-tongs to the inſide of the receiver's braſs-cover, that by moving the turning-key, we might by a ſtring tied to one part of them open the tongs, which elſe their own ſpring would keep ſhut. This being done, the next thing was to provide a body, which would not fall down like a ſtone, or another dead weight through the air, but would in the manner of its deſcent ſhew, that its motion was ſomewhat reſiſted by the air. Wherefore that we might have a body, that would be turned about horizontally, as it were, in its fall, we thought fit to join croſs-wiſe four broad and light feathers (each about an inch long) at their quills with a little cement, into which we alſo ſtuck perpendicularly a ſmall label of paper, about an 8th of an inch in breadth, and ſomewhat more in height, by which the tongues might take hold of our light inſtrument without touching the cement, which elſe might ſtick to them.

BY the help of this ſmall piece of paper, the little inſtrument, of which it made a part, was ſo taken hold of by the tongs, that it hung as horizontal as ſuch a thing could well be placed: and then the receiver being cemented on to the engine, the pump was diligently plied, till it appeared by a gage, which had been conveyed in, that the receiver had been carefully exhauſted: laſtly, our eyes being attentively fixed upon the connected feathers, the tongs were by the help of the turning-key opened, and the little inſtrument let fall, which, though in the air it had made ſome turns in its deſcent from the ſame height, which it now fell from, yet now it deſcended like a dead weight, without being perceived by any of us to make ſo much as one turn, or a part of it: notwithſtanding

See Plate VII. Fig. 4.

ſtanding which I did, for greater ſecurity, cauſe the receiver to be taken off, and put on again, after the feathers were taken hold of by the tongs; whence being let fall in the receiver unexhauſted, they made ſome turns in their deſcent, as they alſo did being a ſecond time let fall after the ſame manner.

BUT when after this, the feathers being placed as before, we repeated the experiment by carefully pumping out the air, neither I nor any of the by-ſtanders could perceive any thing of turning in the deſcent of the feathers; and yet for further ſecurity we let them fall twice more in the unexhauſted receiver, and found them to turn in falling as before; whereas when we did a third time let them fall in the well exhauſted receiver, they fell after the ſame manner as they had done formerly, when the air, that would by its reſiſtance have turned them round, was removed out of their way.

N.B. 1. THOUGH, as I intimated above, the glaſs, wherein this experiment was made, were nothing near ſo tall as I would have had it, yet it was taller than any of our ordinary receivers, it being in height about 22 inches.

2. ONE, that had had more leiſure and conveniency, might have made a more commodious inſtrument than that we made uſe of: for being accidentally viſited by that ſagacious mathematician, Dr. *Wren*, and ſpeaking to him of this matter, he was pleaſed with great dexterity as well as readineſs to make me a little inſtrument of paper, on which, when it was let fall, the reſiſtance of the air had ſo manifeſt an operation, that I ſhould have made uſe of it in our experiment, had it not been caſually loſt, when the ingenious maker was gone out of theſe parts.

3. THOUGH I have but briefly related our having ſo ordered the matter, that we could conveniently let fall a body in the receiver when very well exhauſted; yet to contrive and put in practice what was neceſſary to perform this, was not ſo very eaſy, and it would be difficult to deſcribe it circumſtantially without very many words; for which reaſon I forbear an account, that would prove too tedious to us both.

4. WHAT has been hitherto related, was done in proſecution of but one of the two deſigns I aimed at in the foregoing contrivance, by which I intended, if I could have procured a receiver tall enough, to try whether bodies (ſome very light, and ſome heavier) being let fall, when the air was very diligently pumped out, would not deſcend ſomewhat faſter than if the receiver were full of air. But though I had provided a pendulum, that vibrated quarters of ſeconds, yet the glaſs being no higher than it was, the deſcent even of our feathers took up ſo little time, that even this pendulum was of no uſe; only it ſeemed to all of us, that were preſent at making the above recited trials, that when the feathers were let fall at ſuch times as the air, that would have turned them round in their deſcent, was removed, they came to the bottom ſenſibly ſooner than at other times. But when we ſhall have opportunity to repeat the experiment in taller glaſſes, and to make ſome variation of it, I hope to be able to give your lordſhip a fuller ſatisfaction about this particular. And in the mean while I ſhall forbear to examine, whether the air might ſomewhat retard the deſcent of the feathers upon ſome other account, or meerly upon that of its being a medium not quite devoid of gravity.

ANNOTATIONS.

1. BUT here I muſt be ſo ſincere as to inform your lordſhip, that this fortieth experiment ſeemed not to prove ſo much as did the foregoing made with the ſyringe: for being ſuſpicious, that, to make the feathered body above-mentioned turn in its fall, there would need a reſiſtance not altogether inconſiderable, I cauſed the experiment to be repeated, when the receiver was by our eſtimate, which was not made at random neither, little or nothing more than half exhauſted, and yet the remaining air was too far rarefied to make the falling body manifeſtly turn.

2. AND yet perchance it would have happened otherwiſe, if the receiver had been tall enough; which though I had not then leiſure and conveniency to make it, yet it will not be amiſs to let your lordſhip know by what means we did, that it might be ſomewhat fit to make the recited experiment and ſome others, bring it to the height it had, which did conſiderably exceed that of the talleſt glaſs we could then procure.

TO lengthen our receiver therefore, we thought fit to try, whether we could not cloſe enough faſten to the bottom of it with very good cement a cylindrical pipe of laton, whoſe upper orifice ſhould have near the ſame breadth with the bottom of the glaſs. And though this contrivance ſeemed liable to a couple of not mean difficulties; the one, that the laton being every where bended, and in ſome places neceſſary to be ſoddered, it would be very hard, as indeed we found it, to avoid ſome ſmall cracks and leaks; and the other, that if the metalline pipe were wide enough, ſo great and heavy a pillar of the atmoſphere would come to bear againſt it, as to preſs it inwards, if not alſo to break it; yet we hoped we ſhould be able to obviate both of theſe inconveniencies. Againſt the firſt of which our remedy was, to coat over very carefully the whole pipe with the ſame cloſe cement, wherewith we faſtened it to the glaſs receiver. And againſt the ſecond, we provided a little frame, conſiſting of divers ſmall iron bars faſtened together; which frame (though it were not too wide to go into the cylinder of laton, yet it) was wide enough to be ſo near it on the inſide, that (though the weight of the atmoſphere ſhould, as we feared, preſs the laton ſo as to make it yield inward, yet) it could make it bend no further than the iron-frame would permit; which was not far enough to ſpoil either the receiver or the experiment. And this not unpleaſant phænomenon would ſomewhat ſurpriſe unaccuſtomed ſpectators, that when after the receiver had been very well exhauſted, the external air was permitted to return, there would be heard during some time, from the metalline part of the receiver, divers sounds brisk enough, which would make an odd cracking noise proceeding from the lattin-plate, which having been forceably, though but slowly, bent inwards by the predominant pressure of the atmosphere, was now assisted by the pressure of the returning air, to regain its former figure. And as I thought not fit to omit this circumstance, because it confirms the practicableness of the remedy proposed against the second inconvenience; so I thought fit to mention this way of enlarging and heightening receivers, because what we have related seems to give grounds of hoping, that this contrivance may be made good use of in divers other trials, and particularly in attempts to make receivers capacious enough to contain larger animals, and perhaps even a boy, or a man. In order to some of which purposes we endeavoured to get an improvement made of our metalline cylinder by additional contrivances; but could not, where we then were, get artificers, that would perform what was directed.

EXPERIMENT XLI.

About the propagation of sounds in the exhausted receiver.

TO make some further observation than is mentioned in the * published experiments, about the production and conveying of sounds in a glass whence the air is drawn out, we employed a contrivance, of which, because we make use of it in divers other experiments, it will be requisite to give your lordship here some short description.

WE caused to be made at the turner's a cylinder of box, or the like close and firm wood, and of a length suitable to that of the receiver it was to be employed in. Out of the lower basis of this cylinder (which might be about
See Plate VIII. Fig. 1.
an inch and a half in diameter) there came a smaller cylinder or axle-tree, not a quarter so thick as the other, and less than an inch long: this was turned very true, that it might move to and fro; or, as the tradesmen call it, ride, very smoothly in a little ferrule or ring of brass, that was by the same turner made for it in the midst of the fixed trencher, (as we call a piece of solid wood, shaped like a mill-stone) being four or five inches, more or less, according to the wideness of the receiver, in breadth, and between one and two in thickness; and in a large and round groove, or gutter, purposely made in the lower part of this trencher, I caused as much lead as would fill it up, to be placed and fastened, that it might keep the trencher from being easily moved out of its place or posture, and in the upper part of this trencher it was intended, that holes should be made at such places as should be thought fit, to place bodies at several distances as occasion should require. The upper basis of the cylinder had also coming out of the midst of it another axle-tree, but wider than than the former, that, into a cavity made in it, it might receive the lower end of the turning-key divers times already mentioned, to which it was to be fastened by a slender peg of brass, thrust through two correspondent holes, the one made in the key, and the other in the newly mentioned socket, if I may so call it, of the axle-tree. Besides all which, there were divers horizontal perforations bored here and there in the pillar itself, to which this axis belonged, which pillar we shall, to avoid ambiguity, call the verticle cylinder. The general use of this contrivance (whose other parts need not to be mentioned before the experiments where they are imployed) is, that the end of the turning-key being put into the socket, and the lower axis of the verticle cylinder into the trencher, by the motion of the key a body fastened at one of the holes to the cylinder may be approached to, or removed from, or made to rub or strike against another body fastened in a convenient posture to the upper part of the trencher.

TO come now to our trial about sounds, we caused a hand-bell (whose handle and clapper were taken away) to be fastened to a strong wire, that, one end of the wire being made fast in the trencher, the other end, which was purposely bent downwards, took hold of the bell. In another hole, made in the circumference of the same trencher, was wedged in (with a wooden peg) a steel-spring, to whose upper part was tied a gad of iron or steel, less than an inch long, but of a pretty thickness. The length of this spring was such, as to make the upper part of the hammer (if I may so call the piece of iron) of the same height with the bell, and the distance of the spring from the bell was such, that when it was forced back the other way, it might at its return make the hammer strike briskly upon the outside of the bell.
See the Figure last referred to.

THE trencher being thus furnished and placed in a capped receiver, (as you know, for brevity sake, we use to call one, that is fitted with one or other of the brass covers, often mentioned already,) the air was diligently pumped out; and then, by the help of the turning-key, the vertical cylinder was made to go round, by which means as often as either of a couple of stiff wires, or small pegs, that were fastened at right angles into holes, made not far from the bottom of the cylinder, passed (under the bell, and) by the lately mentioned spring; they forceably did in their passage bend it from the bell, by which means, as soon as the wire was gone by, and the spring ceased to be pressed, it would fly back with violence, enough to make the hammer give a smart stroak upon the bell. And by this means we could both continue the experiment at discretion, and make the percussions more equally strong, than it would otherwise have been easy to do.

THE event of our trial was; that, when the receiver was well emptied, it sometimes seemed doubtful, especially to some of the by-standers, whether any sound were produced or no; but to me for the most part it seemed, that after much attention I heard a sound, that I could but just hear; and yet, which is odd, me-

* Page the 105, 106.

thought it had somewhat of the nature of shrilness in it, but seemed (which is not strange) to come from a good way off. Whether the often turning of the cylindrical key kept the receiver from being so staunch as else it would have been, upon which score some little air might insinuate itself, I shall not positively determine: but to discover what interest the presence or the absence of the air might have in the loudness or lowness of the sound, I caused the air to be let into the receiver, not all at once, but at several times, with competent intervals between them; by which expedient it was easy to observe, that the vertical cylinder being still made to go round, when a little air was let in, the stroak of the hammer upon the bell (that before could now and then not be heard, and for the most part be but very scarcely heard) began to be easily heard. And when a little more air was let in, the sound grew more and more audible, and so increased, until the receiver was again replenished with air; though even then (that we omit not that phænomenon) the sound was observed to be much less loud, than when the receiver was not interposed between the bell and the ear.

And whereas in the already published physico-mechanical experiments, I acquainted your lordship with what I observed about the sound of an ordinary watch in the exhausted receiver, I shall now add, that that experiment was repeated not long since, with the addition of suspending in the receiver a watch, with a good alarum, which was purposely so set, that it might, before it should begin to ring, give us time to cement on the receiver very carefully, exhaust it very diligently, and settle ourselves in a silent and attentive posture. And to make this experiment in some respect more accurate than the others we made of sounds, we secured ourselves against any leaking at the top, by imploying a receiver, that was made all of one piece of glass, (and consequently had no cover cemented on to it,) being furnished only within (when it was first blown) with a glass-knob or button, to which a string might be tied. And because it might be suspected, that if the watch were suspended by its own silver chain, the tremulous motion of its sounding bell might be propagated by that metalline chain to the upper part of the glass; to obviate this as well as we could, we hung the watch, not by its chain, but a very slender thread, whose upper end was fastened to the newly mentioned glass-button.

These things being done, and the air being carefully pumped out, we silently expected the time, when the alarum should begin to ring, which it was easy to know by the help of our other watches; but not hearing any noise so soon as we expected, it would perhaps have been doubted, whether the watch continued going, if for prevention we had not ordered the matter so, that we could discern it did not stand still. Wherefore I desired an ingenious gentleman to hold his ear just over the button, at which the watch was suspended, and to hold it also very near to the receiver; upon which he told us, that he could perceive, and but just perceive something of sound, that seemed to come from far; though neither we that listened very attentively near other parts of the receiver, nor he, if his ears were no more advantaged in point of position than ours, were satisfied, that we heard the watch at all. Wherefore ordering some air to be let in, we did by the help of attention begin to hear the alarum; whose sound was odd enough, and, by returning the stop cock to keep any more air from getting in, we kept the sound thus low for a pretty while, after which a little more air, that was permitted to enter, made it become more audible; and when the air was yet more freely admitted, the by-standers could plainly hear the noise of the yet continuing alarum at a considerable distance from the receiver.

From what has hitherto been related, we may learn what is to be thought of what is delivered by the learned *Mersennus*, in that book of his Harmonicks, where he makes this to be the first proposition. *Sonus à campanis, vel altis corporibus non solum producitur in illo vacuo (quicquid tandem illud sit,) quod fit in tubis hydrargyro plenis, posteaque depletis, sed etiam idem acumen, quod in aere libero vel clauso penitus observatur & auditur.* For the proof of which assertion, not long after, he speaks thus: *porro variis tubis, quorum extremis lagenæ vitreæ adglutinantur, observari campanas in illo vacuo appensas propriisque malleis percussas idem penitus acumen retinere, quod in aere libero habent: atque soni magnitudinem ei sono, qui fit in aere quem tubus clausus includit, nihil cedere.* But though our experiments sufficiently manifest, that the presence or absence of the common air is of no small importance as to the conveying of sounds, and that the interposition of glass may sensibly weaken them; yet so diligent and faithful a writer as *Mersennus* deserves to be favourably treated: and therefore I shall represent on his behalf, that what he says may well enough have been true, as far as could be gathered from the trials he made. For first, it is no easy matter, especially for those that have not peculiar and very close cements, to keep the air quite out for any considerable time in vessels consisting of divers pieces, such as he appears to have made use of. And next, the bigness of the bell in reference to the capacity of the exhausted glass, and the thickness of the glass, and the manner whereby the bell was fastened to the inside of the glass, and the hammer or clapper was made to strike, may much vary the effect of the trial, for reasons easy to be gathered out of the past discourse, and therefore not needful to be here insisted on. And upon this account we chose to make our experiment, with sounds that should not be strong or loud, and to produce them after such a manner, as that as little shaking as could be might be given by the sounding body to the glass it was included in. The proposal made by the same *Mersennus*, to have those, that have industry enough, try whether a bag-pipe will be made to afford the same sound as in the open air, in such vessels as he used for his bells, though he seems to think it would succeed, is that which your lordship will not, I presume, ſollicit me to make trial of, if you remember what is related in the almoſt immediately foregoing experiments, ſhewing, that we could make nothing come out of the cavity of a pair of bellows, that had force enough to blow away a feather, when that cavity was freed from air; as the bagpipe would be by the ſame operation, that empties the glaſs that contains it, or elſe the ſound would not be made in ſuch a vacuum as the ſcope of the experiment requires.

IF I had had conveniency, I would have made ſome trials by conveying a ſmall ſtringed inſtrument (perhaps ſome ſuch as they commonly call a kit) exactly tuned, into a large receiver, and then upon briſkly ſtriking the ſtring of a bigger inſtrument, (tuned, as they ſpeak, to an uniſon to (or with) that of the ſmaller inſtrument) I ſhould have taken notice, whether the ſound would have been ſo uniformly propagated, notwithſtanding the interpoſition of the glaſs receiver, as ſenſibly to ſhake the included ſtring; in order to the diſcerning of which, a bended piece of ſtraw, or feather, or ſome ſuch light body, was to be horſed upon the ſtring to be ſhaken. I alſo intended, in caſe the ſtring were made to move, to make the like trial after the receiver was diligently exhauſted. And laſtly I deſigned to try, whether two uniſon ſtrings of the ſame inſtruments, or of a couple to be placed in the ſame receiver, would, when the air (which is the uſual medium of ſounds) was well pumped out, yet maintain ſuch a ſympathy, as it is called, that upon the motion of the one, the other would alſo be made to ſtir: which trials may be varied, by employing for the external inſtrument another inſtead of a ſtringed one.

AND becauſe contraries, as is vulgarly noted, ſerve to illuſtrate each other, I thought to ſubjoin, to the trials above related about the propagation of ſounds in a thinner medium than the air, ſome obſervations about the conveyance of them through that thicker medium, water: but having unluckily miſlaid my notes upon that ſubject, I cannot at preſent acquaint your lordſhip with what I intended, but muſt defer the doing it, till I ſhall have recovered them.

EXPERIMENT XLII.

About the breaking of a glaſs-drop in an exhauſted receiver.

YOU know, that among the cauſes, that have been propoſed of the ſtrange flying of a glaſs-drop into a multitude of pieces, when the ſlender ſtem of it comes to be broken off, one of the leaſt improbable was taken from the preſſure of the air: as if that within the porous, (and as it were honey-combed) inſide of the glaſs, being highly rarefied when the drop of melted glaſs fell into the water at its firſt formation, it was forced to continue in that preternatural ſtate of expanſion by the hardneſs and closeneſs of the external caſe of glaſs, that incloſed the pith-like part (if I may ſo call it;) ſo that upon the breaking off a part of this ſolid caſe at the ſtem, the external air gaining acceſs, and finding in the ſpungy part very little reſiſtance from the highly rarefied and conſequently weakened air included there, ruſhes in with ſuch violence, as to ſhiver the glaſs-drop into a multitude of pieces.

I ſhall not now trouble your lordſhip with the mention of what may be alledged to queſtion this hypotheſis, eſpecially if it be compared with that accurate account of the phænomena of ſuch glaſs-drops, which was ſometimes ſince preſented to the ſociety by that great ornament of it, Sir *Robert Moray*. But I ſhall only ſay in this place, that when I conſidered, that if the diſſilition of the glaſs would ſucceed, when the air was pumped out of it, it would be hard to aſcribe that effect to the eruption of the external air; I thought fit to try what would happen, if a glaſs-drop were broken in our exhauſted receiver. And accordingly did, though not without ſome difficulty, ſo order the matter, that the blunter part of the glaſs-drop was faſtened to a ſtable body (conveyed into the receiver,) and the crooked ſtem was tied to one end of a ſtring, whoſe other end was faſtened to the turning-key; by which means, when the air had been diligently pumped out, the ſtem was (by ſhortning the ſtring) broken off, and the glaſs-drop was ſhattered into a thouſand pieces.

This experiment was long after repeated with the like ſucceſs; and having at that time no gage to try, how far the air had been drawn out, we let the external air impell up the water out of the pump into the receiver, and thereby found, that that veſſel had not been negligently exhauſted.

EXPERIMENT XLIII.

About the production of Light in the exhauſted Receiver.

I Preſume, I need not put your lordſhip in mind, that divers attempts were made to try, whether either a flame, or kindled coals would be made to continue for ſometime burning in our receiver. But thoſe trials making it evident, that it would be either impoſſible, or very difficult to produce any durable light, without the preſence of the air, by the burning of bodies; I thought it not amiſs, conſidering the nobleneſs of light, to make trial, whether it might be otherwiſe produced in our exhauſted receiver; ſince whether or no the attempts ſhould prove ſucceſsful, the event would probably be inſtructive. For as it is the property of light, when it is produced, to be diſcoverable by it ſelf; ſo in ſuch a trial as we intended, it would teach ſomething concerning light, to find that the abſence of the air would or would not hinder it from being produced. In proſecution of this deſign, knowing that hard ſugar, being nimbly ſcraped with a knife, will afford a ſparkling light, ſo that now and then one would think that ſparks of fire fly from it; we cauſed a good lump of hard loaf-ſugar to be conveniently and firmly placed in the cavity of our capped receiver, and to the vertical cylinder forementioned we cauſed to be faſtened ſome pieces of a ſteel-ſpring, which being not very thick,

thick, might in their paſſage along the ſugar, grate, or rub forceably againſt it, and then the receiver being diligently exhauſted in the night-time, and in a dark room, the vertical cylinder (whoſe lower axis was inſerted into the often mentioned trencher) was made for a pretty while to move round by the help of the turning-key, managed by a hand ſteady and ſtrong enough. By which means the irons, that came out of the vertical cylinder, making in their paſſage vigorous impreſſions upon the ſugar, that ſtood ſomewhat in their way, there were manifeſtly produced a good number of little flaſhes, and ſometimes too, though not frequently, there ſeemed to be ſtruck off little ſparks of fire.

The contrivance here mentioned may be conceived by conſidering the figure belonging to the 41 experiment.

EXPERIMENT XLIV.

About the production of a kind of halo and colours in the the exhauſted receiver.

WE took a large inverted cucurbite for a receiver, which being ſo well wiped both within and without as to be very clear, allowed me to obſerve, and to make others do ſo too, that when the pump began to be ſet a work, if I cauſed a pretty large candle to be held on the other ſide of the glaſs, upon the turning of the ſtop-cock to let the air out of the receiver into the cylinder, the glaſs would ſeem to be full of fumes, and there would appear about the flame of the candle, ſeen through them, a kind of halo, that at firſt commonly was between blue and green, and after ſome ſucks would be of a reddiſh or orange colour, and both very vivid. The production of this meteor, if I may ſo call it, was, according to my conjecture, made on ſome ſuch ſcore as this. That the cement being ſomewhat ſoft and new, as is convenient for this experiment, abounds with turpentine; and having a little, as well to faſten on the receiver, as for the other purpoſe, applied to it a hot iron, whereby the cement was both ſoftened and heated, it ſeemed rational to expect, that upon the withdrawing of the air in the receiver, the aerial particles in the cement, freed from their former preſſure, would extricate themſelves, and with the looſer ſteams of the turpentine, and perhaps of the bees-wax, would with a kind of exploſion expand themſelves in the receiver, and by their interpoſition between the light and the eye exhibit thoſe delightful colours we had ſeen. To confirm which, I afterwards found, that by watchfully obſerving it, I could plainly enough perceive the colouring ſteams, juſt upon the turning of the ſtopcock, to fly up from the cement towards the top of the glaſs; and if we continued pumping, the receiver would grow clearer, and the colours more dilute, till we had occaſion to put on the receiver, and heat the cement afreſh: of which the reaſon might be, partly that the aerial and volatile particles of the upper part of the cement did in that tract of time ſpend themſelves more and more; and partly, becauſe the agitation they received from the heat communicated by the iron did continually decay. Not to mention, that when the receiver is more exhauſted, the want of air makes it more difficult for ſteams to be ſupported, and, as it were, ſwim up and down in it.

FOR farther confirmation, I cauſed ſome cement to be put into a ſmall crucible, warm enough to melt it; and conveying this into a clear receiver of a convenient ſhape and ſize, I cauſed the pump to be ſet a work; whereupon it appeared manifeſtly enough, that upon the opening of the ſtop-cock to let out the air, the ſteams would copiouſly be thrown about from the crucible into the capacity of the receiver, and would, after having a little played there, fall down again. But in theſe apparitions the vividneſs, and ſometimes the kind of the exhibited colours ſeemed much to depend upon divers circumſtances, ſuch as the degrees of heat, the bigneſs and ſhape of the receiver, the quantity of air that yet remained unpumped out, and the nature of the cement itſelf; which laſt particular I the rather mention, becauſe, though I were hindered from doing it, I had thoughts to try a ſuſpicion I had, that by varying the materials expoſed to this kind of operation, ſome pretty variety might be made in the phænomena of the experiment.

WHETHER or no the apparition or whiteneſs, or light, that we ſometimes happened to take notice of divers years ago, and have mentioned in the already publiſhed part of our Phyſico-mechanical Experiments, may be partly (though not entirely) referred to ſome of the cements I then employed, differing from thoſe I now uſe moſt, and to the unheeded temper of thoſe cements, as to warmth, and degrees of ſoftneſs, is a doubt, that further obſervation may poſſibly enable us to determine.

EXPERIMENT XLV.

About the production of heat by attrition in the exhauſted receiver.

THE opinion, that aſcribes the incaleſcence of ſolid bodies, ſtruck or rubbed hard againſt one another, to the attrition or vehement agitation of the intercepted air, is famous and received enough to ſeem worthy of a particular examination. But I confeſs to your lordſhip, that it was not any thing relating to this opinion, that chiefly induced me to make the experiment I am now about to give an account of; for I thought it might be uſeful to more purpoſes than one, to be able to produce by attrition a ſomewhat durable heat, even in our exhauſted receiver: and therefore, though it were to foreſee, that it would prove no eaſy taſk, yet we thought fit to attempt it ſpight of the difficulties met with at our firſt trials. In what way and with what ſucceſs we afterwards made this attempt, I now proceed to relate.

CROSS the ſtable trencher, formerly mentioned, there was faſtened a pretty ſtrong ſpring of ſteel or iron, ſhaped almoſt like the lath of a croſs-bow; and to the midſt of this ſpring was ſtrongly faſtened on the outſide, a round piece of braſs hollowed almoſt like a concave burning-glaſs, or one of thoſe tools, See Plate IV. Fig. 3.

 wherein

wherein they use to grind eye-glasses for telescopes. To this piece of brass, which was not considerably thick, nor above two inches diameter, was fitted a convex piece of the same metal, almost like a gage for a tool to grind glasses in, which had belonging to it a square handle, whereinto as into a socket was inserted a square piece of wood, proceeding from the basis of a square wooden pillar, which we made use of on this occasion instead of our vertical cylinder. By the help of another piece of wood, coming from the other basis of the same pillar, the turning-key was joined to this pillar, which was made of such a length, that when the turning-key was forceably kept down as low as the brass cover, it was a part of, would permit, the convex piece of metal lately described did depress the concave piece a pretty way, notwithstanding a vigorous resistance of the subjacent spring.

BESIDES these things, a little fine powder of emery was put between the convex and concave pieces of brass, to make them more congruous, and facilitate the motion that was to be made; and there was fastened to the upper part of the turning-key a good wimble, without which we presumed the turning of the key would not produce a sufficient motion; in order to the making of which, it was, after the first trial, judged requisite to have a strong man, that was used to exercise his hands and arms in mechanical labours, upon which account we sent for a certain locksmith, that was a lusty and dexterous fellow.

ALL things, that were thought necessary, being thus in readiness, and a mercurial gage being conveyed into the receiver, we caused the air to be diligently pumped out; and then the smith was ordered to turn the wimble, and to continue to lean a little on it, that he might be sure to keep the turning-key from being at all lifted up by the former mentioned spring.

WHILST this man with much nimbleness and strength was moving the wimble, I watched the gage, to observe, whether the agitation of the stop-cock, and consequently the engine, did not prejudice the experiment; and for greater caution I caused the pump to be almost all the while kept at work, though that seemed not so necessary.

WHEN the turner of the wimble was almost out of breath, we let in for haste the air at the cover of the receiver, by lifting up the turning-key; and nimbly removing the receiver, we felt the pieces of brass, betwixt whom the attrition had been made, and, as we expected, found both of them very sensibly warm.

BUT being willing to confirm the experiment by a second trial, which we hoped might, after the experience taught us by the first, be somewhat better performed, we caused the smith, after he had well refreshed himself with rest and drink, to lay hold of the wimble again, when the gage made it appear, that the receiver was well exhausted, so that by further pumping the quick-silver seemed not to be further depressed. And in this second trial the nimble smith played his part so well, the pump in the mean while not being neglected, that when we did as before hastily let in the air, and take out the bodies, that had been rubbed against one another, they were both of them, especially the uppermost, so hot, that I could not endure to hold my hand on either of them, and they did for a considerable time retain a not inconsiderable degree of warmth.

THE same day I caused to be made at the turners two bodies of wood, for size and shape like those of brass we had just before employed; the upper of these was of hard oak, the other of beach, such a difference between woods, to be heated by mutual attrition, being thought to be an advantageous circumstance. But though the wimble was swiftly turned as before, and that by the same person, nevertheless the wood seemed not to me, (for all the by-standers were not of my opinion) to have manifestly acquired any warmth; and yet that there had been a considerable attrition, appeared by the great polish, which part of the wood had evidently acquired, which made me suspect, that though the wood seemed dry enough, yet it might not really be so, notwithstanding the contrary was affirmed to me. But not being willing to sit down with a single trial, I caused the experiment to be repeated with more obstinacy than before; the effect of which was, that the wood, especially the upper piece of it, was brought to a warmth unquestionably sensible.

EXPERIMENT XLVI.

About the slacking of quick-lime in the exhausted receiver.

THE several scopes I aimed at in making the following trial are not necessary to be here particularly taken notice of. But one of them may be guessed at by the subsequence of this experiment to that immediately foregoing, and the phænomena of it may be mentioned in this epistle, upon the account of their being exhibited by our engine.

WE took in an evaporating glass a convenient quantity of water, and having conveyed it into a receiver, and well drawn out the air, we let down into it by the turning-key a lump of strong lime, about the bigness of a pippin; and observed not, that at the first immersion, nor for some while after, there appeared any considerable number of bubbles; but within about ¼ of an hour, as I guessed it, the lime began (the pump having been and being still plied from time to time) to slack with much violence, and with bubbles wonderfully great, that appeared at each new exsuction, so that the inside of the receiver, though pretty large, was at length lined with lime-water, and a great part of the mixture did from time to time overflow the vessel, that had purposely been but little filled; nor did any thing but our weariness put a period to the bubbling of the mixture, whose heat was sensible, even on the outside of the receiver, and which continued considerably hot in the evaporating glass for ¼ of an hour, as I conjectured, after the receiver was removed.

Note. THAT the lime employed about this experiment, was of a very good and strong kind, made of hard stones, and not such lime, made of chalk, as is commonly used at *London*, which probably would not have been strong enough to have afforded us the same phænomenon.

EXPERIMENT XLVII.

About an attempt made to measure the force of the spring of included air, and examine a conjecture about the difference of its strength in unequally broad mouth'd vessels.

THOUGH several of the foregoing trials have sufficiently manifested, that the spring of the air in its natural or wonted state hath a force very considerable, and indeed much greater than men seem to have hitherto believed; yet I could not hope by any of these experiments to determine by any known weight, how great that force is, so as to conclude, that it is equivalent to such a weight, as so many pounds, ounces, &c. and to no more. Wherefore among the uses I had designed to make of our syringe formerly often mentioned, it was one, to try, if by the help of that instrument, we could determine somewhat near (for no more was to be expected) how much weight a cylinder of uncompressed air included in it, and consequently of the same diameter with the cavity of the barrel, would be able to sustain, or also to lift up.

IN order to this trial, 1. we provided a stable pedestal, or frame, wherein the syringe might be kept firm, and erected. Next, we also provided a weight of lead shaped like our
Exper. V. brass-hoop, or ring, formerly described, that by the advantage of its figure it might be made to hang down by strings from the top of the handle of the rammer, and so press evenly enough on all sides, without making the upper part of the instrument top-heavy. 2. We took care to leave, between the bottom of the syringe, which was firmly closed with strong cement) and that part of it where the sucker was, a convenient quantity of air, to expand itself, and lift up the weight, when the air external to that included air should be pumped out of the receiver. And lastly, the handle of the rammer (from which the annular weight lately spoken of depended) was so fastened to the turning-key of the cover of the receiver, that the weight might not compress the air included in the syringe, but leave it in its natural state or wonted laxity, till the air were withdrawn from the receiver.

BUT notwithstanding all this, when we actually tried the experiment, that happened, which I feared. For though by this method the included air would well enough lift up a weight of seven or eight pound, yet when the rammer came to be clogged with so considerable a weight, as my scope in making the experiment required, the instrument proved not so stanch, but that it was easier for some particles of air to force themselves a passage, and get away between the sucker and the inside of the barrel, than to heave up so great a weight. And yet I have thought fit to relate the experiment thus particularly, because, if an exact syringe can be procured, which I fear will be very difficult, but do not think impossible, this seems to be one of the likeliest and least exceptionable ways I know, of measuring the force of the air's spring.

BUT despairing to get such a syringe, as I desired, in the place where I then was, I bethought my self of another way, by which I hoped to be able, though not to arrive at an exact knowledge of the full force of the air's spring, yet, at least to approach nearer it than I have been able to do by the help of the syringe. For this purpose, considering with my self, that if a convenient quantity of air were included in a fine small bladder, the sides of it would hinder the air from getting away, and the limberness of them would permit the air to accommodate it self and the bladder to the figure of a cylindrical vessel, into which it might be put:

WHEREFORE with much ado I procured to be made by a person exercised in turning a couple of hollow cylinders, whose sides were of a sufficient thickness, that they might resist the pressure of the air to be imprisoned in them, and of such differing breadths, that the first had but one inch in diameter, and the second two; their depths being also unequal, that the one might receive a much larger bladder than the other.

WITH the lesser of these, which was very carefully turned, I made a diligent trial; whose circumstances I cannot now acquaint your lordship with, the paper, wherein they were amply recorded, having been with other notes belonging to this continuation unluckily lost: but the most considerable things in the event were, that it was very difficult to procure a bladder small and fine enough for that little cylinder; and that one, which at length we procured, would not continue stanch for many trials, but would after a while part with a little air in the well exhausted receiver, when it was clogged with the utmost weight it could sustain: but whilst it continued stanch we made one fair trial with it, from whence we concluded, that a cylinder of air of but an inch in diameter, and less than two inches in length, was able to raise visibly, though but a little, a weight of above ten pounds (I speak of avoirdupoize weights, where a pound contains sixteen ounces.) The manner of making this experiment, and the cautions used in judging of it, your lordship may learn by the recital of the subsequent trial; my notes about which were not so unfortunate as those that concerned the former.

INTO a hollow cylinder of wood of four inches in depth, and two in diameter, furnished with a broad and solid bottom or pedestal, to make it stand the firmer, was put a lamb's or sheep's bladder very strongly tied at the neck, on which was put a wooden plug, marked with ink where the edge of the cylinder was contiguous to it: this plug being loaden with weights, See Plate VIII. Fig. 2 and 4.

weights,

weights, amounting to 35 pound, (the uppermost of which weights was fastened to the turning-key, to keep it upright, and to help to raise it at first) the receiver was exhausted, till the mark appeared very manifestly above the brim of the cylinder; and then, though the string were by turning the key quite slackened, yet the mark on the plug continued very visible: and when so much air was let into the receiver, as made the weight depress the plug quite beneath the mark, upon the repumping out of the air, the weight was without the help of any turning-key lifted up, and by degrees all the mark on the plug was raised about $\frac{3}{8}$ above the edge of the cylinder.

WHEREFORE we substituted for a seven pound weight one that was estimated at 14, (for then we had not a ballance strong enough to weigh it with,) and using the same bladder we repeated the experiment, only having a care to support a little the uppermost weight by the turning-key, till the bladder had attained its expansion; and then the weight being gently let go, depressed not the plug so low, but that we could yet see the mark on it, (which yet was all we could do,) though that part of the plug, where the mark was, were manifestly more depressed than the other.

FOR the clearing up of some particulars relating to this tryal, we will subjoin the following notes,

1. THE plug is to be so fitted to the cavity of the cylinder, as easily to slip up and down it, without grating against the sides of it, lest it needlesly increase the resistance of the weight to be raised. And this plug ought to be of a convenient length, as about an inch and a half at least, that it may be the fitter to help to reduce the bladder by compression into a somewhat cylindrical shape, and yet that it may not be thrust in too deep by the incumbent weight; and that the weight might rest more firmly upon it, there was a broad and strong ledge made at the top of it, by which it might lean on every side upon the brim of the hollow cylinder.

2. BEFORE the instrument was conveyed into the receiver, the bladder (which ought to be of a just size, and not full blown, and of a fine and limber contexture) was put into the cylinder, and by divers gradual (but not immoderate) compressions was reduced to conform its self, as much as might be, to the cylindrical shape of the containing vessel. And then the weight being put on, and taken off again, there was a mark (in the form of an horizontally placed arch) made with ink, where the edge of the brim of the hollow cylinder did almost touch the plug. This we thought necessary to do, to avoid a mistake; for we must not judge, that all the weight, that might be raised by our bladder, may pass for the weight sought after by our experiment; since the air in the bladder is by reason of the incumbent weight more compressed than it was before, and consequently its being able to heave up a great weight will not infer, that our common air is able in its natural state (as they call it) to exert so great a strength; that weight being only to be looked on as raised or sustained by the uncompressed air, that is, raised or sustained, when the plug is lifted up to the mark, since till then the spring of the air does but bring it back from its new state of adventitious compression to its natural or wonted laxity.

3. WHEN, after the operation was ended, we took the bladder out of the vessel, it had obtained a form cylindrical enough; and though it could be but two inches in diameter, yet it was so little, as to be but half an inch more long than broad.

THE reason, why I chose to have the two cylinders made of the unequal diameters above-mentioned, was to examine, as far as by this way I could, a conjecture I had, that the force of the spring of differing cylinders of air to lift up solid weights would, at the very first raising of the weights, be in duplicate proportion to the diameters of their cylinders, (those diameters being proportionable to the areas of the plain superficies, against which the air does immediately press,) without very much considering the inequality, that may be between the quantity of the several parcels of air, whose pressures are compared. But it is to be remembered, that I said, at the very first raising of the weights, because presently after that, the quantity of the parcels of air may be very considerable: for, as I have shewn in another treatise, two very unequal quantities of air being made by their expansion to possess two equal spaces the lesser quantity of air must be much more rarefied in proportion than the greater; and consequently, to bring this home to our present argument, though both be lifted up $\frac{1}{4}$ or $\frac{1}{2}$ of an inch, the spring of a very little air must be much more weakened than that of a very considerable quantity, and so it cannot continue to lift up its weight, as the above-mentioned proportion would (if it were not for this advertisement) seem to require.

TAKING then our conjecture in the sense now declared, the success of our trials is agreeable to it, inviting us to conclude, that the air in the bladder, which was but two inches in diameter, was able by its pressure to countervail the weight of 42 pound, which is about four times, the weight, that we lately observed the spring of a cylinder of air of one inch in diameter to be able to lift up. For though, according to what we have formerly said of a duplicate proportion, 42 pound seems to be somewhat more than ought to have been lifted up in the cylinder of two inches bore, when that of one inch lifted up not above 10 pound; yet this disagrees not with the hypothesis, if we consider, that the substance of the bladder straitens the cavity of the smaller cylinder in a greater proportion than that of the bigger.

5. THOUGH we have thus (as far as the instruments we were able to procure would assist us) measured the pressure of included air, yet I must not forbear to advertise your lordship, that considering what I formerly observed to you about the weight of an atmospherical pillar of an inch in diameter, I cannot but think, that if a cylinder, or other convenient instrument, exactly tight, can be procured, the spring

of an aerial cylinder will appear to be greater than we found it by the foregoing tryals; in which I conſider, that, not to mention the reſiſtance of the bladder itſelf, the membraneous ſubſtance, that lined the cylinders (though it were very thin and fine) could not but ſomewhat ſtraiten their cavities, and conſequently ſomewhat (though not much) leſſen the diameters of the included aerial cylinders.

6. To all theſe notes I muſt add this advertiſement, that it may be therefore the more difficult in ſuch trials as ours to aſcertain the force of the air's ſpring, becauſe that air itſelf, when it is concluded, being ſhut up with the preſſure of the atmoſphere upon it, it is probable, that ſince that preſſure, as we have ſhewn, is not at all times the ſame, the ſpring of the included air will accordingly be varied. And if my memory fail me not, when the lately recited experiments were made, our barometer declared the atmoſphere to be ſomewhat light.

FROM what has been hitherto delivered, this may reſult; that it is likely, that the ſpring of an aerial cylinder an inch broad may be able to ſuſtain, if not raiſe, a pretty deal more than ten pound weight; and that the paſt trials, without determining that the air can raiſe no more in them than it did, do at leaſt prove, that it can raiſe up as much weight as we have related, ſince we actually found it to do ſo.

EXPERIMENT XLVIII.

About an eaſy way of making a ſmall quantity of included air raiſe in the exhauſted receiver 50 or 60 pound, or a greater weight.

I WOULD very willingly have further proſecuted the foregoing trials, to ſee how far the lately propoſed conjecture or hypotheſis would hold; but was hindered by the want of receivers tall and capacious enough to contain the weights, that ſuch an attempt required: but remembring, that there were not any experiments made in our engine, that appeared more ſtrange to the generality of ſpectators, and ſerved more to give them a high opinion of the air's ſpring, than thoſe, wherein they ſaw ſolid bodies actually lifted up by it; and remembering, that I had lying by me a braſs veſſel, which had been beſpoken for another experiment, for which the workmen had not made it fit; I thought it not amiſs to employ it about making a trial very eaſy, and yet fit to be ſhewn to ſtrangers, to convince them, that the ſpring of the air is a much more conſiderable thing than they imagined.

See Plate VIII. Fig. 4. WE took then a braſs veſſel made like a cylinder, and having one of his orifices exactly covered with a flat plate very firmly faſtened to it, the other orifice being wide open. The depth of this veſſel was four inches, and the diameter ſhould have been preciſely, but wanted about a quarter of an inch of, four inches. To this hollow cylinder we fitted a wooden plug, like one of thoſe deſcribed in the foregoing experiment, ſave that it was not quite ſo long, and that it was furniſhed with a rim or lip, which was purpoſely made of a conſiderable breadth, that it might afford a ſtable baſis to the weight that ſhould lean upon it. And then taking a middle-ſized and limber bladder, ſtrongly tied at the neck, but not near full blown, we preſſed it by the help of the plug into the cylinder to make it the better accommodate it ſelf to the figure of it. Then taking notice by an inky mark, how much the plug was extant above the orifice of the veſſel, we laid the weights upon the plug, whoſe rim or lip hindered it from being depreſſed too deep into the cavity of the veſſel; and having conveyed them into the receiver, we found, as we expected, that if we had loaded the plug but with a ſingle weight, as to avoid trouble and the danger of breaking the glaſs we uſually thought fit to do, though that were a common half-hundred weight, which you know amounts to 56 pound, it would very quickly be manifeſtly heaved up by the ſpring of the included air. For confirmation of more than which, I ſhall ſubjoin the enſuing trial, as I find it recorded among my looſe notes.

See Plate VIII. Fig. 3.

See Plate VIII. Fig. 5.

THE weight, that was lifted up by the bladder in the cylinder four inches broad, was 75 pound: this weight was lifted up till the wooden plug diſcloſed the mark, that was to ſhew the height, at which the air kept the ſaid plug before it was compreſſed: diſcloſed, it I ſay, viſibly at the fifth exſuction, and at the ſeventh that mark was $\frac{1}{8}$, or rather $\frac{3}{16}$ above the edge of the cylinder. In the gage, where the mercury in the open air was wont to ſtand about $\frac{1}{8}$ above the uppermoſt glaſs-mark, it was depreſſed, till it was $\frac{1}{8}$ below the ſecond mark. When the air was let in, it was a pretty while before the weight did manifeſtly begin to ſubſide; the bladder being taken out, and the place it had poſſeſſed in the cylinder being ſupplied with a ſleeve, or ſome ſuch thing, and the weight laid again upon the plug, we found, that at twenty-four exſuctions the mercury was depreſſed to the loweſt mark of the gage; and it was the thirty-fourth or thirty-fifth exſuction before the receiver appeared to be ſo exhauſted, as to put an end to the ſinking of the mercury, which was then above $\frac{1}{8}$ beneath the loweſt mark.

YOUR lordſhip will eaſily believe, that moſt of the ſpectators of ſuch trials thought it ſomewhat ſtrange to ſee a ſmall quantity of air, which was not only uncompreſſed in the bladder, but did not near fill it, and left it very ſoft and yielding to the leaſt touch, lift up ſo eaſily by its bare ſpring ſuch great weights as endeavoured to oppreſs it. But this not being any thing near a ſufficient trial, how far the conjecture or hypotheſis formerly propoſed will hold, I thought fit to make the utmoſt trials, the talleſt receivers I could procure would admit: and having cauſed leaden weights to be purpoſely caſt flat like cheeſes, and as broad as we could conveniently put into the receiver, that by the advantage of this ſhape we might be able to pile up the more of them, without much danger that any of them ſhould be ſhaken down; we laid divers of them one upon another, and then the upper part of the receiver growing too narrow to admit more of them, we added a leſs broad weight or two; and then exhauſting

exhausting the receiver, till we perceived by the gage, that the air was manifestly withdrawn, we found, as near as we could measure, by the help of a mark and a pair of compasses, that the plug was so far raised, as that it was concluded, that the elevation would have been much greater, if the included air, being put upon so great a conatus, had not found it easier to produce some leak at the neck of the bladder, than to lift up so great a weight, which by our reckoning came to about 100 pound of 16 ounces to the pound. But this last experiment, for want of some requisite accommodations, we were hindered from repeating and promoting; though the above mentioned hypothesis made me presume, that a far greater weight might this way have been raised, if the bladder had been stanch, and the receiver high enough.

I need not tell your lordship, that if a larger bladder be employed and included in a brass vessel of a sufficiently wide orifice, a far greater weight may be lifted up by the spring of the internal air. But yet it will not be amiss to give your lordship on this occasion this advertisement, which may be fit to be taken notice of on divers others; that care must be had not to make receivers, that ought to be well emptied, too large, and especially too wide at the orifice; for otherwise they will be exposed to so great a pressure of the atmosphere, that they need be of an extraordinary strength to resist it; and even receivers, that seemed thick enough proportionably to their bulk, and which held out very well till the close of the operation, yet when they came to be very diligently exhausted, they did, by reason of the wideness of their orifices, begin to crack at the bottom.

EXPERIMENT XLIX.

Viz. the XXXVI. IN one of my published experiments I long since told your lordship, that when I endeavoured, by the help of a sealed bubble, weighed in an exhausted receiver, to compare the gravity of air and water, I was hindered by the casual breaking of the glass from compleating the experiment. Wherefore I afterwards thought fit to repeat the trial; and though when I had done so twice or thrice, having given away the large receiver I had made use of about them, and not being able ever since to procure a good one, that was capacious enough for the tender scales I thought so nice an experiment required, I did not prosecute that attempt so far as I intended; yet this very difficulty I met with, to procure the requisites of making the trial, invites me to subjoin the two following notes, which I find among my loose papers.

April the 19, 1662. WE weighed a bubble in the receiver, which we found to weigh above half a grain heavier, when much of the air was exhausted, than when it was full. Afterwards we took out this sealed bubble, and weighing it found it to weigh 68 grains and a half; then breaking off the small tip of it under water, we found, that the heat, by which it was sealed up, had rarefied its included air, so that it admitted 125 grains of water, for the admitted water and glass weighed $193\frac{1}{2}$ grains. Then filling it full with water, we found it to contain in all 739 grains of water, for it weighed $807\frac{1}{2}$ grains: whence it is evident, that the difference between the weight of water and air was less than 1228 to 1.

WE weighed in the receiver a bubble, the glass of which weighed 60 grains: the air that filled it, weighed *in vacuo* $\frac{27}{32}$ of a grain: the water, that filled it, weighed $720\frac{1}{4}$ grains: so that by this experiment the proportion of the weight of air to water is as one to $853\frac{17}{27}$.

THE trials mentioned in these notes, though May 26,
they were too few for me to acquiesce in, yet 1662.
being made in a new way, and which has some advantages above those, that have been hitherto employed to weigh the air, may yet serve to keep us from the contrary extremes, that have not been avoided by such eminent mathematicians as *Galileo* and *Ricciolus*; the former of which makes water to be about 400 times as heavy as the air; and the latter, whose conjecture is much remoter from the truth, 10,000 times heavier.

BUT it is so desireable a thing, and may prove of such importance, to know the proportion in weight betwixt air and water, that I shall not scruple to acquaint your lordship with an attempt or two, that I made to discover it by another way. For, though at first sight this experiment may seem to be the same with one published a pretty while ago in the learned *Schottus* his *Mechanica hydrolico-pneumatica*; yet your lordship will easily perceive this difference between them; that, whereas the industrious author of that experiment contents himself to shew, by the diminution of the weight of a glass, when the air has been drawn out of it, that the air, before it was drawn out, was not devoid of gravity; the following trial does not only perform the same thing, and by a superadded circumstance confirm the truth to be thereby proved, but it endeavours also to shew the proportion in gravity betwixt the air and water. The trials themselves were registered among my *Adversaria* as follow:

A small receiver being exhausted of air by the engine, and counterpoised whilst it continued so; the stop-cock was turned, and the air re-admitted, which made it weigh 36 grains more than it did before: and to prevent jealousies, we caused it to be applied the second time to the engine, by which the air being emptied once more, the glass was put into the other scale of the former ballance, and so counterpoised; and then the external air being re-admitted, (which rushed in as formerly with a whistling noise,) there was found 36 grains or better requisite to restore the ballance to an æquilibrium.

WE took a small glass receiver fitted with a stopcock, and having exhausted it of the air, and counterpoised it, and let in the outward air, we found the weight of the vessel to be increased by that admission 36 grains. This done, we took the receiver, after having well counterpoised it, out of the scale; and having applied it the second time to the engine, we once more withdrew the air, and then turning the stop-cock to keep out the external air, we took care, that none of the cement, employed to join it to the engine, should stick to it, as we had diligently freed it from adherent cement, before we last applied it to the engine. Then weighing it again, we found it to weigh either 35 or 36 grains (but rather the former) heavier than it did, when it was last counterpoised in the same ballance. This being also done, we immersed the stop-cock into a bason of fair water, and let in the liquor, that we might find, how much water would succeed in place of the air we had drawn out. When no more water was impelled in, we turned the stop-cock once more, to keep it from falling out, and then weighing it in the same scales, (after we had wiped the stop-cock, that no water might stick to it on the outside,) we found the water (without computing the vessel) to weigh 47 ounces, 3 drams, and 6 grains, which divided by 35 grains, (which I took to be the weight of the air, that was equal in bulk to this water that succeeded it,) the quotient was (wanting a very little) 650 grains, for the proportion of the weight between air and water of the same bigness, at the time when the experiment was made: which circumstance I therefore take notice of, because the atmosphere appeared by the baroscope (wherein the mercury stood then at 29 inches and three quarters) to be very heavy; which made me the less wonder to find this proportion not so great, as at other times I had observed it to be between water and air in point of weight: though I suspected, that because this odd experiment cannot be nimbly dispatched, some little air may have got in at the stop-cock, besides the air that disclosed itself in numerous bubbles in the water that was admitted, where, though it lay in such small particles as not to be discerned before, yet these particles by this opportunity to expand themselves, extricated themselves from the water, and by getting together might somewhat resist the ingress of more; which is a difficulty, whereto the measuring the proportion between water and air in a heated æolipile is liable. But the stealing in of any air, before the water was let in, is mentioned but as a suspicion.

YOUR lordship may perhaps think it somewhat strange, that I should present you trials, whose events do not so well agree together, as perchance you expected. But this very disagreement was one of the motives, that induced me to acquaint you with them: for all those comprised in these experiments being made faithfully, and not without, at the least, an ordinary diligence, as they seem to make it probable, that one may without any great error estimate the proportion of our English air to water to be as one to some number between 600 and 1100; so it is not to be expected, that the proportion, whatever it be that should be pitched upon, should be accurate and stable. For though learned men seem to have hitherto taken it for granted, that it may suffice once for all diligently to investigate the proportion betwixt those two bodies, yet, not only that I am apt to believe, that a determinate quantity of air (as a pint or quart) may be unequally heavy in distant countries, and even in differing places of the same country; but what I have taken notice of in the 17th of the printed experiments, and afterwards frequently observed of the great inequalities of the weight of the atmosphere, inclines me to think, that in the self same place two experiments may be made with the same instruments, and equal diligence, and yet the weights of the air may be found differing enough; which may keep your lordship from much wondering, that in the 36th printed experiment, made when I had the variations of the atmosphere's gravity in my eye, I found the air to be less ponderous in reference to water, than in these latter trials. But of this, I hope, I shall, if God permit, make further trials with the same vessels, at times when I shall perceive by the baroscope, that the gravity of the atmosphere is very great and very small. And I wish the curious would make the like trials in other regions. I do not forget, that not only the school-philosophers, but most of the moderns deny, that air hath any weight in air, no more than water in water; but having * elsewhere declared and explained my sense about this received opinion, I shall not here spend any of the little time I have remaining, to justify my dissent; for which your lordship may find sufficient grounds in the newly related experiments, especially if you please to consider, that though the opinion I disallow have been chiefly and generally grounded upon some arguments supposed to evince, that water has no weight in water, I have † elsewhere shewn those proofs not to be cogent, and taught a practical way of weighing water in water, with a pair of ordinary scales ‡.

EXPERIMENT L.

About the disjoining of two marbles (not otherwise to be pulled asunder without a great weight) by withdrawing the pressure of the air from them.

IN our formerly published experiments about the air ‖, I did, if I misremember not, acquaint your lordship with an attempt I had made to make a couple of coherent marbles fall asunder, by withdrawing the air from them. But though I then esteemed, that their cohesion depended upon the pressure of the air, yet not being at that time furnished with all the accommodations requisite to make an experiment not easy to be performed succeed, I thought fit, when I had afterwards opportunity, to prosecute what I then began, and add some circumstances, that I could not then make trial of, and yet whose success will not, I presume, be unwelcome, since it supplies us with no less than matters of fact; whence we may argue, this that experiment of coherent marbles (which not only the

* In the Hydrostatical Paradoxes. † In an Appendix to those Paradoxes. ‡ This method was omitted in the English edition of the newly mentioned appendix, but not in the Latin version. ‖ Experiment XXXI. See also the cause of this phænomenon discoursed of in the author's History of Fluidity and Firmness.

the Aristotelian plenists have much triumphed in, but which some recent favourers of our hypothesis have declared themselves to be troubled with) is not only reconcileable to our doctrine, but capable of being made a confirmation of it; notwithstanding what has lately been published (upon the supposition of a case, which at first blush may seem somewhat of a kin to our experiment,) by a very learned * writer, to whose objection against our hypothesis, though as well confidently as very civilly proposed, an answer may in due place, if your lordship desire it, be returned.

We took two flat round marbles, each of them of two inches and about three quarters in diameter; and having put a little oil between them to keep out the air, we hung at a hook fastened to the lowermost a pound weight to surmount the cohæsion, which the tenacity of the oil and the imperfect exhaustion of the receiver might give them. Then having suspended them in the cavity of a receiver, at a stick that lay horizontally a-cross it; when the engine was filled, and ready to work, she shook it so strongly, that those, that were wont to manage it, concluded, it would not be near so much shaken by the operation. Then beginning to pump out the air, we observed the marbles to continue joined, until it was so far drawn out, that we began to be diffident whether they would separate. But at the 16th suck, upon the turning of the stop-cock, (which gave the air a passage out of the receiver into the pump,) the shaking of the engine being almost, if not quite, over, the marbles spontaneously fell asunder, wanting that pressure of the air, that formerly had kept them together: which event was the more considerable, not only because they hung parallel to the horizon, but adhered so firmly together, when they were put in, that having tried to pull them asunder, and thereby observed how close they stuck together, I foretold it would cost a good deal of pains so far to withdraw the air, as to make them separate: which conjecture your lordship will the less wonder at, if I add, that a weight of 80 and odd pounds, fastened to the lowermost marble, may be drawn up together with the uppermost, by vertue of the firmness of their cohesion.

N. B. This is not the only time, that this experiment succeeded with us. For sometimes, when they were not so closely pressed together before they were put in, the disjunction was made at the 8th suck, or sooner, and we seemed to ourselves to observe, that when we hung but half a pound weight to the lower marble, it required a greater exhaustion of the receiver to separate them, than when we hung the whole pound.

After, having proceeded thus far with the instruments we then had, meeting with an artificer, that was not altogether unskilful, we directed him to make (what we wanted before in that place) such a brass-plate, to serve for a cover or cap to the upper orifice of receivers open at the top, as we have divers times had occasion to mention already in giving accounts of some of the foregoing trials: by the help of which contrivance we prosecuted the newly related experiment much farther than we could do before; as may appear by the following account.

We fastened to the lowermost of the two marbles a weight of a very few ounces, (for I remember not the precise number,) and having cemented the capped receiver with the marbles in it, as before, to the pump, we did by a string, whereof one end was tied to the bottom of this turning-key, and the other to the uppermost marble, and which (string) passed through the crank or hook belonging to the brass-cover; we did, I say, by the help of this string, and by turning round the key, draw up the superior marble, and by reason of their coherence the lowermost also, together with the weight that hung at it: by which means being sure, that the two marbles stuck close together, we began to pump out the air, that kept them coherent; and after a while, the air being pretty well withdrawn, the marbles fell asunder. But we having so ordered the matter, that the lowermost could fall but a little way beneath the other, we were able by inclining and shaking the engine to place them one upon another again, and then letting in the air somewhat hastily, that by its spring it might press them hard together, we found the expedient to succeed so well, that we were not only able by turning the abovementioned cylindrical key, to make the uppermost marble take up the other, and the annexed weight; but we were fain to make a much more laborious and diligent exhaustion of the air, to procure the disjunction of the marbles this second time, than was necessary to do it at the first. See Plate IV. Fig. 4.

And for further prevention of the objections or scruples, that I foresaw some prepossessions might suggest, I thought fit to make this further trial; that when the marbles were thus asunder, and the receiver exhausted, we did, before we let in the air, make the marbles fall upon one another as before; but the little and highly expanded air, that remained in the receiver, having not a spring near strong enough to press them together, by turning the key we very easily raised the uppermost marble alone, without finding it to stick to the other as before. Whereupon we once more joined the marbles together, and then letting in the external air, we found them afterwards to stick so close, that I could not without inconvenience strain any farther, than I fruitlesly did, to pull them fairly asunder; and therefore gave them to one that was stronger than I, to try, whether he could do it, which he also in vain attempted to perform.

And now, my lord, though I had thoughts of adding divers other experiments to those I have hitherto entertained you with; yet (upon a review) finding these to amount already to fifty, I think it not amiss to make a pause at so convenient a number. And the rather, because an odd quartainary distemper, that

* Dr. *H. M.* in the second chap. of the second book, of the new edition in folio, of his *Antidote against Atheism.*

that I slighted so long, as to give it time to take root, is now grown so troublesome, that I fear it may have too much influence upon my style; which apprehension obliges me, as well to avoid abusing or distressing your lordship's patience, as to allow myself some seasonable refreshment, to reserve the mention of the designed additions, until they can with less trouble to us both be presented you by,

My dear lord,

Your lordship's most humble servant, and affectionate uncle,

Oxford, March 24, 1667.

ROBERT BOYLE.

NOTES, &c.

ABOUT THE

ATMOSPHERES of CONSISTENT BODIES here below.

SHEWING,

That even HARD and SOLID BODIES (and some such, as one would scarce suspect) are capable of emitting EFFLUVIA, and so of having ATMOSPHERES.

ADVERTISEMENT.

HE that shall take the pains to peruse the following paper, will easily believe me, when I tell him, that it was not designed to come abroad with the experiments, in whose company it now appears. But the stationer earnestly representing, that divers experiments being reserved by me for another occasion, the remaining ones alone would not give the book a thickness any thing proportionable to its breadth; I consented, at his sollicitation, to annex to them the following observations, because of some affinity between the small atmospheres of lesser bodies, and the great atmosphere that surrounds the terrestrial globe; in which the other, that do at least help to compose it, are lost and confounded, as brooks and rivers are in the ocean. And to save the reader the pains of making guesses, to what kind of writing the ensuing discourse may belong, I shall here intimate, that it is dismembred from certain papers about occult qualities in general, which make part of the notes I long since designed, and also partly published, about the origin of qualities, of which notes those, that concerned effluviums, being the most copious, I referred them to four general heads; whereof the first only is treated of in the following discourse, the others being withheld, as having not affinity enough with the atmosphere to accompany this, whereon they have no such absolute dependence, but that they may well enough spare it. And I make the less scruple to let it appear without them, because the inducements already mentioned are not a little strengthened by this superadded consideration, that the following notes may give light to several of the observations I have made, of some less heeded phænomena of the alterations of the air, in case they be allowed to enter into the Appendix to this Continuation.

Of the ATMOSPHERES *of* CONSISTENT BODIES.

THE school philosophers, and the vulgar, in considering the more abstruse operations and phænomena of nature, are wont to run into extremes; which, though opposite to one another, do almost equally contribute to keep men ignorant of the true causes of those effects they admire. For the vulgar, being accustomed to converse with sensible objects, and to conceive grosly of things, cannot easily imagine any other agents in nature, than those that they can see, if not also touch, and handle; and as soon as they meet with an effect, that they cannot ascribe to some palpable, or at least sensible efficient, they are, and stick not to confess themselves utterly at a loss. And though the vulgar of philosophers will not acknowledge themselves, to be posed by the same phænomena with the vulgar of men, yet in effect they are so. But the school-philosophers, on the contrary, do not only refuse to acquiesce in sensible agents, but, to solve the more mysterious phænomena of nature, nay and most of the familiar ones too, they scruple not to run too far to the other side,

side, and have their recourse to agents, that are not only invisible, but inconceivable, at least to men, that cannot admit any, save rational and consistent notions: they ascribe all abstruse effects to certain substantial forms, which however they call material, because of their dependence on matter, they give such descriptions to, as belong but to spiritual beings: as if all the abstruser effects of nature, if they be not performed by visible bodies, must be so by immaterial substances; whereas betwixt visible bodies and spiritual beings there is a middle sort of agents, invisible corpuscles; by which a great part of the difficulter phænomena of nature are produced, and by which may intelligibly be explicated these phænomena, which it were absurd to refer to the former, and precarious to attribute to the latter. Now for method's sake I will refer the notes, that occur to me about effluviums, to four heads; whereof the first is mentioned in the title of this paper, and each of the other three shall be successively treated of in as many distinct ones.

THAT fluid bodies, as liquors, and such as are manifestly either moist, or soft, should easily send forth emanations, will I presume be granted without much difficulty; especially considering the sensible evaporation, that is obvious to be observed in water, wine, urine, &c. and the loose contexture of parts, that is supposed to be requisite to constitute soft bodies, (as flowers, balsoms, and the like:) but that even hard and ponderous bodies, notwithstanding the solidity and strict cohesion of their component parts, should likewise emit steams, will to many appear improbable enough to need to be solemnly proved.

WHETHER you admit the atomical hypothesis, or prefer the Cartesian, I think it may be probably deduced from either, that very many of the bodies, we are treating of, may be supposed exhaleable as to their very minute parts. For, according to the doctrine of *Lucippus*, *Democritus*, and *Epicurus*, each indivisible particle of matter hath essentially either a constant actual motion, or an unlooseable endeavour after it; so that though it may be so complicated in some concretions with other minute parts, as to have its avolation hindered for a while; yet it can scarce otherwise be, but by this incessant endeavour of all the atoms to get loose, some of them should from time to time be able to extricate themselves, and fly away. And though the Cartesians do not allow matter to have any innate motion, yet, according to them, both vegetables, animals, and minerals, consist of little parts so contexed, that their pores give passage to a celestial matter; so that this matter continually streaming through them, may well be presumed to shake the corpuscles, that compose them: by which continued concussion now some particles, and then others, will be thrown and carried off into the air, or other contiguous body, fitted to receive them. But though by these, and perhaps other considerations, I might endeavour to shew *à priori*, as they speak, that it is probable consistent bodies themselves are exhaleable, yet I think it may be as satisfactory, and more useful, to prove it *à posteriori*, by particular experiments, and other examples.

THAT then a dry and consistent form does not necessarily infer, in the bodies that are endowed with it, an indisposition to send forth steams, which are, as it were, little colonies of particles, is evident, not only in the leaves of damask roses, whether fresh or dried; as also in wormwood, mint, rue, &c. but in ambergreese, musk, storax, cinnamon, nutmegs, and other odoriferous and spicy bodies. But more eminent examples to our present purpose may be afforded us by camphire, and volatile salts, such as are chemically obtained from hartshorn, blood, &c. for these are so fugitive, that sometimes I have had a considerable lump of volatile salt (either of fermented urine, or of harts-horn) fly away by little and little out of a glass, that had been carefully stopped with a cork, without leaving so much as a grain of salt behind it. And as for camphire, though by its being uneasy to be powdered, it seem to have something of toughness or tenacity in it; yet I remember, that having for trial's sake counterpoised it in nice scales, even a small lump of it would in a few hours suffer a visible loss of its weight, by the avolation of strongly scented corpuscles, and this, though the experiment were made both in a north window, and in winter.

BUT I expect you should require instances of the effluviums of bodies of a close or solid texture: wherefore I proceed to take notice, that amber, hard-wax, and many other electrical bodies do, when they are rubbed, emit effluviums. For though I will not now meddle with the several opinions about the cause and manner of electrical attraction, yet besides that almost all the modern naturalists, that aim at explicating things intelligibly, ascribe the attraction we are speaking of to corporeal effluxes; and besides that I shall ere long have occasion to shew you, that there is no need to admit with Cartesius, that because some electrical bodies are very close and fixed, what they emit upon rubbing is not part of their own substance, but somewhat that was harboured in their pores: besides these things, I say, I have found, that many electrical bodies may by the very nostrils be discovered, when they are well rubbed, to part with store of corpuscles, as I have particularly, but not without attention, been able to observe in amber, rosin, brimstone, &c.

I know not, whether it will be worth while to take notice of the great evaporation I have observed, even in winter, of fruits, as apples, and of bodies, that seem to be better covered, as eggs; which, notwithstanding the closeness of their shels, did daily grow manifestly lighter and lighter; as I observed in them, and divers other bodies, that I kept long in scales, and noted their decrements of weight. But perhaps you will be pleased to hear, that having a mind to shew, how considerable an evaporation is made from wood, I caused a thin cup, capable of holding about a pint, or more, to to be turned of a wood, that was chosen by the turner as solid and dry enough, though it were not of the closest sort of woods, such as are lignum vitæ, and box. And as I caused the shape of a cup to be given it, that it might have a greater superficies exposed to the air, and consequently might be the fitter to emit store of steams into it; so the success did not only answer my expectation, but exceed it: for though the trial were made some time in winter, there was so quick and plentiful an evaporation made from the cup, that I found it no easy matter to counterpoise it; for whilst grains were putting into the opposite scale, to bring the tender balance to an æquilibrium, the copious avolation of invisible steams from the wood (which had so much of superficies contiguous to the air) would make the scale that held it sensibly too light. And I remember, that for further satisfaction, being afterwards in a city, where there were both good materials and workmen, I ordered to be made a bowl, about the same bigness with the former, of well seasoned wood, which being suspended in the chamber I lay in, (which circumstance I therefore mention, because the weather and a little physick I had taken obliged me to keep a fire there,) it quickly began manifestly to loose of its weight; and though the whole cup wanted near two drachms of near two ounces, yet in twelve hours, viz. from ten of the clock in the morning to the same hour at night, it lost about 40 grains, (for it was above 39.) But of such experiments, and the cautions belonging to them, I may elsewhere speak farther.

It were not difficult for me to multiply instances of the continual emanation of steams from vegetable and animal substances; but I am not willing to enlarge myself upon this subject, because I consider, that there are other bodies, which seem so much more indisposed to part with effluviums, that a few instances given in such may evince what I would prove, much more than a multitude produced in other bodies. And since I consider, that those substances are the most unlikely to afford effluvia, that are either very cold, or very ponderous, or very solid and hard, or very fixed; if I can shew you, that neither of these qualifications can keep a body from emitting steams, I hope I shall have made it probable, that there is no sort of bodies here below, that may not be thought capable of affording the corporeal emanations we speak of.

And first I remember, that I have not only taken eggs, and in a very sharp winter found them, notwithstanding the coldness of the air where I kept them, to grow sensible lighter, in a faithful pair of scales, in not very many hours; but because ice is thought the coldest visible body we know, I thought fit to shew, that even this body will loose by evaporation; for having counterpoised a convenient quantity of ice in a good balance, and forthwith exposed it therein to the cold air of a frosty night, that the evaporations should be from ice not from water, I found the next morning, that though the scale, wherein the ice were put, was dry, which argued as well as the coldness of the weather, that the exposed concretion had not thawed; yet I found its weight to be considerably diminished, and this experiment I successfully made in more than one winter, and in more than one place. And it is now but a few days since, exposing not long before midnight less than two ounces of ice in a good balance to a sharply freezing air, I sent for it before I was up in the morning; and though by the driness of the scales the ice, that was in one of them, appeared not to have thawed, yet it had lost about ten grains of its former weight; so that here the evaporation was made in spite of a double cold, of the ice, and of the air.

I should now proceed to the mention of ponderous and solid bodies; but before I do so, it may be expedient to give you notice, that, to make the proof of what I have proposed more satisfactory, and more applicable to our future purposes, I shall forbear to give you any examples of the exhalations of bodies, where so potent an agent as the fire is made to intervene.

But though I purposely forbear to insist on such examples, yet it may not be amiss to intimate, that in explicating some occult qualities, even such exhalations, as are produced by the help of the fire, may be fit to be taken into consideration, as we may hereafter have occasion to shew. And therefore we may observe in general, that the fire is able to put the parts of bodies into so vehement a motion, that except gold, glass, and a very few more, there are not any bodies so fixed and solid, that it is not thought capable to dissipate either totally, or in part. It is known to those, that deal in the fusion of metals, that not only lead and tin, but much harder bodies will emit copious and hurtful steams. And there are some kinds of that iron, which our smiths call cold-share iron, about whose smell, whilst it was red hot, when I made inquiry, the ingeniousest smith I had then met with told me, that he had found it several times to be so strong, and rank, that he could scarce indure to work with his hammer those parcels of metal, whence it proceeded. And even without being brought to fusion, not only brass, and copper will, being well heated, become strongly scented, but iron will be so too, as is evident by the unpleasing smell of many iron stoves. And on this occasion I might not impertinently add here a trial we made to observe, whether the steams of iron may not be made, though not immediately visible, yet perceptible to the eye itself, though the metal had not a red, much less a white heat. But having elsewhere related it at large, in a discourse you may command a sight of, I shall rather refer you to it, than loose the time it would take up to transcribe it.

These things premised, I proceed now to the mention of ponderous bodies; and concerning them, to represent, that if you will admit what almost all the corpuscularians assert, and divers of the peripateticks do not now think fit to deny, that the magnetical operations

tions are performed by particles iſſuing forth of the body of the loadſtone, or other magnetical agent; I ſhall not need to go far for an inſtance to our preſent purpoſe, ſince I have hydroſtatically found, that ſome loadſtones (for I have found thoſe minerals very differing in gravity) are ſo ponderous, as to exceed double the weight of flints, or other ſtones of the ſame bulk.

BUT not to inſiſt on loadſtones, ſtone-cutters will inform you, as they did me, that black marble, and ſome other ſolid and heavy ſtones, will, upon the attrition they are expoſed to, when the workmen are poliſhing them, eſpecially without water, emit, and that without the help of external heat, a very ſenſible ſmell; which I found to be much more ſtrong and offenſive, when, to make it ſo, I had the curioſity to cauſe a piece of ſolid black marble to have divers fragments ſtruck off from it with a chizel and a hammer: for the ſtrokes ſucceeding one another faſt enough to make a great concuſſion of the parts of the black marble (for in white, which is not ſo ſolid, the trial will not ſucceed well) there quickly followed, as I expected, a rank unpleaſant ſmell; and you will grant me, I know, that odours are not diffuſed without corporeal emanations. I remember alſo, that having procured ſome of thoſe acuminated and almoſt conical ſtones, that paſs among the vulgar for thunder-ſtones, by rubbing them a little one againſt the other, I could eaſily, according to my expectation, excite a ſtrong ſulphureous ſtink. I have alſo tried upon a certain mineral maſs, that was ponderous almoſt as a metal, but to me it ſeemed rather an unuſual kind of marchaſite, that I could in a trice, without external heat, make it emit more ſtrongly ſcented exhalations, than I could contentedly endure: to which I ſhall add this example more, that having once made a chemical mixture of a metalline body and coagulated mercury, which you will believe could not but be ponderous, though this mixture had already endured as violent a fire as was neceſſary to bring it to fuſion, in order to caſt it into rings; yet it was ſo diſpoſed to part with corporeal effluxes, that a very ingenious perſon, that practiced phyſick, and was there when I made it, earneſtly begged a little of it of me for ſome patients troubled with diſtempers in the eyes, and other parts remote enough from the hand; which he affirmed himſelf to have very happily cured, by making the patient wear a ring of this odd mixture, or wearing a little of it as an appenſum near the diſaffected part. If you make a *vitrum Saturni* with a good quantity of minium in reference to the ſand or cryſtal, which it helps to bring to fuſion, you ſhall have a glaſs exceeding ponderous, and yet not devoid of electricity: and I remember, that having ſometimes cauſed braſs it ſelf to be turned like wood, that I might try, whether ſo great, though inviſible, a concuſſion of all the parts would not throw off ſome ſteams, that might be ſmelled, I was not reduced to forego my expectation; but yet becauſe it was not fully anſwered, and becauſe alſo there is great difference of braſs upon the ſcore of the *lapis calaminaris*, whereof together with copper it is made, I enquired of the workman, who uſed to turn great quantities of braſs, whether he did not often after find it more ſtrong; and he informed me, that he did, the ſmell being ſometimes ſo ſtrong, as to be offenſive to ſtrangers, that came to his ſhop, and were not uſed to it.

I proceed now to the effluviums of ſolid and hard bodies; of which, if moſt of our corpuſcularian philoſophers, and divers others be not much miſtaken, I may be allowed to give inſtances in all electrical bodies, which, as I have already noted, muſt according to their doctrine be acknowledged to operate by ſubſtantial emanations. Now among electrical bodies I have obſerved divers, that are of ſo cloſe a texture, that aqua fortis itſelf, nor ſpirit of ſalt will work upon them, and to be ſo hard, that ſome of them will ſtrike fire like flints. Of the former ſort I have found divers gems, which I named in my notes about electricity; and even the cornelian itſelf, which I found to attract hairs, though it be thought to be of a much ſlighter texture than precious ſtones, did yet reſiſt aqua fortis, as I tried in a large ring, brought out of the Eaſt-Indies, which I purpoſely broke, and reduced ſome part of it to powder, that I might make theſe and ſome other trials with it. Rock cryſtal alſo, though it have a very manifeſt attractive virtue, as they call it, I have yet found it ſo hard, as to ſtrike fire rather better than worſe than ordinary flints. And to ſhew, that no hardneſs of a body is inconſiſtent with its being electrical, I ſhall add, that though diamonds be confeſt to be the hardeſt bodies, that are yet known in the world, yet frequent experience has aſſured me, that even theſe, whether raw or poliſhed, are very manifeſtly, and ſometimes vigorouſly enough, electrical.

AND to let you ſee, that I need not to have recourſe to this kind of bodies, to prove, that very ſolid ones are capable of effluvia; I will, to what I have formerly noted about the odour of black marble, ſubjoin two or three examples of the like nature.

THE firſt ſhall be taken from a ſort of concretions very well known in divers parts of *Italy* by the name of cugoli, becauſe of the great uſe, that is made of it by the glaſs-men. Theſe concretions, you will eaſily believe, are very hard, as other minerals of that ſort are wont to be; and yet being invited by my conjectures about the atmoſpheres of bodies, to try them by rubbing them one againſt the other, I found, as I expected, that they afforded not only a perceptible, but a very ſtrong ſmell, which was far from that of a perfume.

AND this brings into my mind, that having met with ſome ſtones cut out of humane bladders, whoſe texture was ſo cloſe, that I could not with corroſive menſtruums make any ſenſible ſolution of one, whereon I made my trial, though, to facilitate the liquors operation, part of it were reduced to fine powder; yet by a little rubbing of one of theſe ſo cloſely contexed ſtones, it would preſently afford a rank ſmell, very like the ſtink of ſtale urine.

I remember I have caused iron to be turned with a lath, to examine, whether by the internal commotion, that would by that operation be produced in the corpuscles of the metal, even that solid, as well as ponderous body would not become capable of being smelled; and though by reason of the nature of that parcel of iron, whereon we made our trial, or some accidental disposition, which was at that time, being winter, in my organs of smelling, the odour seemed to me but very faint; yet upon the enquiry I made of the artificers, whether in turning greater pieces of iron they did not find the smell stronger? they told me, that they often found it very strong, and sometimes more so than they desired.

And this brings into my mind what I have carefully observed in grinding of iron; for there are many grindstones so qualified, that in case iron instruments be held upon the stone, whilst it is nimbly turned under it, though the water, that is wont to be used on such occasions, stifles, if I may so speak, the smell, and keeps it from being commonly taken notice of; yet if you purposely cause, as I remember I have done, the use of water to be forborn, your success will not be like mine, if you do not find, that store of fœtid exhalations will be produced. And though it be not always so easy to discern by the smell, from which of the two bodies they issue, or whether they proceed from both; yet it seems probable enough, that some of the steams come from the iron, and it is more than probable, that if they proceed not from that metal, they must from a body, that is so hard, as to be able to make impressions in a trice upon iron and steel themselves.

The last example I shall name under this head, is furnished me by marchasites, some of which would after a short concussion without external heat be made to exhale for a pretty while together a strong sulphureous odour, and yet were so hard, than when struck with a steel-hammer, (which would not easily break them) they afforded us such a number of sparks, as appeared strange enough. And it is known, that it is from their disposition to strike fire, (which yet I dare not attribute to all sorts of marchasites,) that this kind of mineral is, by a name frequently to be met with in writers, called pyrites. And in this example we may take notice, that a body, capable of being the source of corporeal emanations, may be at once both very solid and very ponderous.

It remains now, that I manifest, that even the fixedness of bodies is not incompatible with their disposition to emit effluviums.

I might allege on this occasion, that the regulus of antimony, and also its glass, though they must have endured fusion to attain their respective forms; yet they will without heat communicate to liquors antimonial expirations, with which those liquors being impregnated become emetick and purgative. I might also add, that divers electrical bodies are very fixed in the fire, and particularly that crystal, as we have more than once tried, will endure several ignitions and extinctions in water, without being truly calcined, being indeed but cracked into a great multitude of little parts: but because the above named antimonial bodies will after a while fly away in a strong fire, and because the effluviums of crystal are not so sensible as those, which can immediately affect our eyes or nostrils, I will here subjoin one instance, such as I hope will make it needless for me to add any more, it being of a body, which must have sustained any exceeding vehement fire, and is looked upon by most of the chemists as more undestroyable than gold itself; and that is glass, which is able, as you know, to endure so great a brunt of the fire, that you did not perhaps imagine I should of all bodies name it on this occasion. But my conjectures about the atmospheres of bodies leading me to think, that glass itself might afford me a confirmation of them; I quickly found, that by rubbing a very little while two solid pieces of it (not, as I remember, of the finer sort) one against the other, they would not only yield a sensible odour, but sometimes so strong an one, as to be offensive. By which you will easily perceive, why I told you above, that I did not acquiesce in the Cartesian argument against electrical bodies performing their operations by emanations of their own substance, drawn from hence, that glass does attract light bodies, as indeed it does, though but weakly; and yet is too fixed to emit effluviums, the contrary of which supposition the lately mentioned experiment, and by us often repeated, does sufficiently evince.

From what other solid bodies, and that will endure the fire, I have, or have not been able to obtain such odorous steams, it is not necessary to declare in this place, but may perhaps be done in another.

You may, I presume, have taken notice, that, according to what I intimated a while ago, I have forborn in the precedent examples to mention those effluvia of solid bodies, that need the action of the fire to be obtained. But since the sun is the grand agent of nature in the planetary world, and since during the summer, and especially at noon, and in southern climates, his heat makes many bodies have little atmospheres, that we cannot so well discern that they have constantly; I see not, why I may not be allowed to ascribe atmospheres to such bodies, as I have observed to have them, when the sun shines upon them; and also to think, that the like may be attributed at least sometimes to such other bodies, as will do the things usually performed by effluviums, when yet they are excited but by an external heat, which exceeds not that of the hot sun.

Of these two sorts of bodies I shall for brevity's sake name but two or three examples, and then hasten to a conclusion.

The first of these I must make bold to borrow from my observations about electricity, among which this is one, that to shew, that the particular and usual manner of exciting such bodies, namely by rubbing them, is not always necessary; I took a large piece of good amber, and having in a summer morning, whilst the air was yet fresh, tried, that it would not without being excited attract a light body I had exposed to it; I removed it into the sun's beams, till they had made it moderately hot, and then I found according to my expectation, that it had acquired an attractive virtue, and that not only in one particular place, as is usually observed, when it is excited by rubbing, but in divers and distant places at once; at any of which it would draw to it the light body placed within a convenient distance from it: so that even in this climate of ours a solid body may quickly acquire an atmosphere by the presence of the sun, and that long before the warmest part of the day.

THE next instance you will perchance think somewhat strange, it being that, when for want of an opportunity to make the like trial in the warm sun, I took a little but thick vessel made of glass, and held it near the fire, till it had got a convenient degree of heat, (which was not very great, though it exceeded that which I had given the amber) I found, as I had imagined, that the heat of fire had made even this body attractive, as that of the sun had made the other.

WHAT degree of heat I have observed to be either necessary, or the most convenient to excite electrical bodies, according to their different natures, (for the same degree will not indifferently serve for them all) this is not the properest place to declare, that it will be more to our present purpose to make some short reflection on what has been hitherto delivered.

IT seems then probably deduceable from the foregoing experiments and observations, that a very great number, if not the greatest part, even of consistent bodies, whether animal, vegetable, or mineral, may emit effluviums, and that even those that are solid may, at least sometimes, have their little atmospheres, though the neighbouring solids will often keep the evaporations from being every way ambient in reference to the bodies they issue from.

FOR as the instances hitherto alledged (which are not all that I could have named) do plainly shew, that divers bodies, and some that have not been thought very likely, are such as we speak of; so several things induce me to believe, that there may be many more of the like nature.

FOR first, very few, if any, have (that I know of) had the curiosity to make use of nice scales, which such trials require, to examine the expirations of inanimate bodies, which if they shall hereafter do, I make little doubt, but they will light on many things, that will confirm what we have been proposing, by their finding, that some bodies, which are not yet known to yield exhalations, do afford them, and that many others do part with far more copious ones than is imagined. For one would not easily have thought, that so extremely cold a body as a solid piece of ice should make a plentiful evaporation of itself in the cold air of a freezing night; or that a piece of wood, that had long lain in the house, and was light enough to be conveniently hung for a long time at a ballance, that would lose its æquilibrium with, as I remember, half a quarter of a grain, should in less than a minute of an hour send forth steams enough to make the scales manifestly turn, and that in winter.

BUT supposing (which is my second consideration) that trials were made with good instruments for weighing, though it will follow, that in case the exposed body grow lighter, something exhales from it; yet it will not follow, that if no diminution of weight be discovered by the instrument, nothing that is corporeal recedes from it. I will not urge, that it is affirmed, not only by the generality of our chemists, but by learned modern physicians, that when either glass of antimony, or crocus metallorum impregnate wine with vomitive and purgative particles, they do it without any decrement of their weight; because the scales in apothecaries shops, and the little accurateness wont to be employed in weighing things, by those that are not versed in statical affairs, made me, though not deny the tradition, which may perchance be true, yet, unwilling to build upon observations, which to be relied on are to be very nicely made; and therefore I shall rather take notice, that though the loadstone be concluded to have constantly about it a great multitude of magnetical effluvia, which may be called its atmosphere, yet it has not been observed to lose any thing of its weight by the recess of so many corpuscles. But because, if the Cartesian hypothesis about magnetisms be admitted, the argument drawn from this instance will not be so strong as it seems, and as it otherwise would be; I shall add a more unexceptionable example, for I know you will grant me, that odours are not diffused to a distance without corporeal emanations from the odorous body: and yet, though good amber-grease be, even without being excited by external heat, constantly surrounded by a large atmosphere, you will in one of the following discourses find cause to admire, how inconsiderable the waste of it is.

IF it be said, that in tract of time a decrement of weight may appear in bodies, that in a few hours or days discovers not any; the objection, if granted, overthrows not our doctrine, it being sufficient to establish what we have been saying, if we have evinced, that the effluvia of some bodies may be subtle enough not to make the body by their avolation appear lighter in statical trials, that are not extraordinarily (and as it were obstinately) protracted. And this very objection puts me in mind to add, that for ought we know the decrement of bodies, in statical experiments long continued, may be somewhat greater, than even nice scales discover to us; for how are we sure, that the weights themselves, which are commonly made of brass, (a metal very unfixed,) may not in tract of time suffer a little diminution of their weight, as well as the bodies counterpoised by them? and no man has I think yet tried, whether glass, and even gold may not in tract of time lose of their weight, which in case they should do, it would not be easily discovered, unless we had bodies, that were perfectly fixed, by comparison to which we might be better assisted,

assisted, than by comparing them with brass weights, or the like, which being themselves less fixed, will lose more than gold and glass.

My third and last consideration is, that there may be divers other ways, besides those furnished us by staticks, of discovering the effluvia of solid bodies, and consequently of shewing, that it is not safe to conclude, that because their operation is not constant or manifest, such bodies do never emit any effluvia at all, and so are uncapable to work by their intervention on any other body, though never so well disposed to receive their action. And this I the rather desire that you would take notice of, because my chief (though not only) design in those notes is (you know) to illustrate the doctrine of occult qualities; and it may conduce to explicate several of them, to know, that some particular bodies emit effluvia, though perhaps they do it not constantly, and uniformly; and though perchance to, they do not appear to emit any at all, if they be examined after the same manner with other exhaleable bodies, but only may be made to emit them by some peculiar way of handling them, or appear to have emitted them by some determinate operation on some other single body, or at most small number of bodies.

Perchance you did not think, until you read what I lately told you about glass, that from a body, that had endured so violent a fire, there could, by so slight a way as rubbing a little while one piece against another, be obtained such steams, as may not only affect but offend the nostrils. Nor should we easily believe, if experience did not assure us of it, that a diamond, that is justly reputed the hardest known body in the world, should by a little rubbing be made to part with electrical effluvia. Nay, (that I may give some kind of confirmation to that part of the last paragraph that seems most to need it,) I shall add, that I once had a diamond not much bigger than a large pea, which had never been polished or cut, whose electrical virtue was sometimes so easily excited, that if I did but pass my fingers over it to wipe, the virtue would disclose itself; and if as soon as I had taken it out of my pocket, I applied a hair to it, though I touched not the stone with my fingers, that I might be sure not to rub it, that hair would be attracted at some distance, and many times one after an other, especially by one of the sides of the stone, (whose surface was made up of several almost triangular planes;) and though this excitation of the diamond seemed to proceed only from the warmth that it had acquired in my pocket, yet I did not find that that warmth, though it seemed not to be altered, had always the same effect on it, though the wiping it with my finger failed not (that I remember) to excite it. Something like this uncertainty I always observed in another diamond of mine, that was much nobler than the first, and very well polished, and in a small ruby, that I have yet by me, which would sometimes be considerably electrical without being rubbed, when I but wore the ring it belonged to on my little finger; and sometimes again it seemed to have lost that virtue of operating without being excited by friction, and that sometimes within a few minutes, without my knowing whence so quick a change should proceed. But I must insist no longer on such particulars, of which I elsewhere say something; and therefore I proceed to take notice, that we should scarce have dreamed, that when a partridge or hunted deer has casually set a foot upon the ground, that part, where the footstep hath been (though invisibly) impressed, should continue for many hours a source of corporeal effluxes; if there were not setting dogs, and spaniels, and bloodhounds, whose noses can take notice at that distance of time of such emanations, though not only other sorts of animals, but other sorts of dogs are unable to do so.

I saw a stone in the hands of an academick, an acquaintance of mine, which I should by the eye have judged to be an agate, not a bloodstone; and consequently I should not have thought, that it could have communicated medicinal effluvia appropriated to excessive bleedings, if the wearer of it had not been subject to that disease, and had not often cured both himself and others, by wearing this stone about his neck; which if he left off, as sometimes he did for trial's sake, his exceedingly sanguine complexion (to which I have rarely seen a match) would in a few days cast him into relapses. What I have elsewhere told you about the true virtues of some stones, (for I fear that most of those that are wont to be ascribed to them are false,) may give some confirmation to what I have been delivering, which I cannot now stay to do, being to draw to a conclusion as soon as I have put you in mind, that it would not probably have ever been expected, that so ponderous and solid a body as the loadstone should be inviorned by an atmosphere, if iron had been a scarce mineral, and had not chanced to have been placed near it.

And with this instance I shall put an end to these notes, because it allows me to make this reflexion; that since solid bodies may have constant atmospheres about them, and yet not discover, that they have so, but by their operation upon one particular body, or those few which participate of that; and since there are already (as we have seen) very differing ways, whereby bodies may appear to be exhaleable, it is not unlikely, that there may be more and more bodies (even of those that are solid and hard) found to emit effluvia, as more and more ways of discovering, that they do so, shall either by chance or industry be brought to light.

AN INVENTION FOR

Eſtimating the Weight of Water in Water, with ordinary Balances and Weights.

Firſt Printed in the PHILOSOPHICAL TRANSACTIONS, No. L. p. 1001.

"THE author of this invention is the "noble *Robert Boyle*; who was pleaſed "to comply with our deſires of com- "municating it in Engliſh to the curious in "*England*, as by inſerting the ſame in the Latin "tranſlation of his Hydroſtatical Paradoxes he "hath gratified the ingenious abroad. And it will "doubtleſs be the more welcome, for as much "as no body, we know of, hath ſo much as "attempted to determine, how much water "may weigh in water; and poſſibly, if ſuch "a problem had been propoſed, it would "have been judged impracticable.

"THE method or expedient he made uſe of "to perform it, as near as he could, may be "eaſily learned by the enſuing account of a "trial or two he made for that purpoſe, "which among his notes he cauſed to be "regiſtered in the following words."

A glaſs-bubble of about the bigneſs of a pullet's-egg was purpoſely blown at the flame of a lamp, with a ſomewhat long ſtem turned up at the end, that it might the more conveniently be broken off. This bubble being well heated to rarefy the air, and thereby drive out a good part of it, was nimbly ſealed at the end, and by the help of the figure of the ſtem was by a convenient weight of lead depreſſed under water, the lead and glaſs being tied by a ſtring to one ſcale of a good ballance, in whoſe other there was put ſo much weight, as ſufficed to counterpoiſe the bubble, as it hung freely in the midſt of the water. Then with a long iron forceps I carefully broke off the ſealed end of the bubble under water, ſo as no bubble of air appeared to emerge or eſcape through the water, but the liquor by the weight of the atmoſphere ſprung into the unrepleniſhed part of the glaſs-bubble, and filled the whole cavity about half full; and preſently, as I foretold, the buble ſubſided and made the ſcale, it was faſtened to, preponderate ſo much, that there needed four drachms and 38 grains to reduce the balance to an æquilibrium. Then taking out the bubble with the water in it, we did, by the help of the flame of a candle warily applied, drive out the water (which otherwiſe is not eaſily excluded at a very narrow ſtem) into a glaſs counterpoiſed before; and we found it, as we expected, to weigh about four drachms and thirty grains, beſides ſome little that remained in the egg, and ſome ſmall matter that may have been rarefied into vapours, which added to the piece of glaſs, that was broken off under water and loſt there, might very well amount to ſeven or eight grains. By which it appears, not only that water hath ſome weight in water, but that it weighs very near * or altogether as much in water, as the ſelf ſame portion of liquor would weigh in the air.

THE ſame day we repeated the experiment with another ſealed bubble, larger than the former, being as big as a great hen-egg, and having broken this under water, it grew heavier by ſeven drachms and thirty-four grains; and having taken out the bubble, and driven out the water into a counterpoiſed glaſs, we found the tranſvaſated liquor to amount to the ſame weight, abating ſix or ſeven grains, which it might well have loſt upon ſuch accounts, as have been newly mentioned.

* This expreſſion was added, to leave liberty for a further inquiry, whether the experiment, which hereby appears convincing as to the main thing intended to be proved, may not admit the having ſomething further debated, and annexed about ſome circumſtantial thing or other.

TRACTS

ABOUT

The Cosmical Qualities of Things.
Cosmical Suspicions.
The Temperature of the Subterraneal Regions.
The Temperature of the Submarine Regions.
The Bottom of the Sea.

To which is prefixed,

An Introduction to the History of PARTICULAR QUALITIES.

An ADVERTISEMENT of the PUBLISHER to the READER.

THOUGH the noble author of the following tracts hath also written divers other short discourses, upon several occasions, yet, had he not been diverted from his purpose, he had continued to let them lie by him, intending, in case he should suffer them to come abroad, to dispose of them agreeably to a design, that it is not necessary the reader should be now acquainted with.

IN the mean while, several virtuosi, to whom some of these tracts had been shewn, and with whom the matters handled in some others had been discoursed, did out of a concern, (as they gave out,) for the common-wealth of learning, pressingly represent to the author:

FIRST, that divers of these loose tracts, having little or no dependency upon one another, might without inconvenience be published apart, in what number and order the author should please.

SECONDLY, that since his main design in these, as well as his other physical writings, was to provide materials for the history of nature, it would be thought enough, that they be substantial and fit for the work; in what order or association soever they should happen to be brought into the philosophical repository.

THIRDLY, that the communicating these tracts to the curious would be the best way to secure them from being lost or embezelled, as some others of his papers have been, not only formerly, but very lately.

FOURTHLY, that the kind reception, the curious had given to what he had hitherto presented them, might well invite, if it did not oblige him, not to envy them the early use of those experiments and hints, which will probably, before the time, wherein his design would suffer them to come abroad, prove serviceable to philosophy, by setting divers inquisitive heads on work, exciting the curiosity of some, and exercising the industry of others.

LASTLY, that, as of the peices, he had hitherto published (except where his own backwardness had expressedly interposed) the first edition had not long been the only; so probably within a moderate space of time, another edition of those tracts, he should first put out, would both allow him to increase their number, and change their order as he should judge most expedient, and, (in case he should in the mean while return to his library,) recruit his discourses with those passages, that he designed to borrow for them thence.

BUT, though these considerations, joined to the earnestness of the persons that made them, and the just respect he had for them, rendered it uneasy for him to resist their perswasions; yet they never obtained an actual compliance, until they were assisted by such an unhappy juncture of sickness and business, as, leaving him small hopes of accomplishing his first intentions in any reasonable time, made him consent to send away to the press some of those tracts, that he found the least unready for it, in the order wherein they chanced to come to his hands. Which being thus represented, the considering and ingenious reader will soon find, what cause there is, and how much it concerns the advancement of valuable philosophy, that, since this excellent author hath (to the publisher's knowledge, as also was insinuated above) many other rare tracts of a philosophical nature in store, he be sollicited from time to time, that he would be pleased, according to the measure of health he shall enjoy, to impart with all possible speed those discourses, which tend to the enlargement and progress of useful knowledge, maugre all envy and malice. H. O.

The

THE HISTORY OF PARTICULAR QUALITIES.

CHAP. I.

THE past discourse has, I hope, *Pyroph.* given you some tolerable account both of the nature, and of the origin of qualities in general. Wherefore it now follows, that we proceed to qualities in particular, and consider how far the manner, whereby they are produced, and those other phænomena of them, that we shall have occasion to take notice of, will accord with, and thereby confirm the doctrine I have hitherto proposed; and whether they will not, at least, much better comport with that, then with the opinions either of the peripateticks, or the chymists.

I shall not spend time to enquire into all the several significations of the word quality, which is used in such various senses, as to make it ambiguous enough: since by the subsequent discourse it will sufficiently appear, in which of the more usual of those significations we employ that term. But thus much I think it not amiss to intimate in this place, that there are some things, that have been looked upon as qualities, which ought rather to be looked upon as states of matter, or complexions of particular qualities, as animal, inanimal, &c. health and beauty, which last attribute seems to be made up of shape, symmetry, or comely proportion, and the pleasantness of the colours of the particular parts of the face. And there are some other attributes, namely, size, shape, motion, and rest, that are wont to be reckoned among qualities, which may more conveniently be esteemed the primary modes of the parts of matter; since from these simple attributes, or primordial affections, all the qualities are derived. But this consideration relating to words and names, I shall not insist upon it.

NOR do I think it worth while to enumerate and debate the several partitions, that have been made of qualities, (of which I have met with divers, and could perchance my self encrease the number of them,) for though one, that were disposed to criticise upon them, would not perhaps acquiesce in any of them, but look upon them as being more arbitrary, than grounded upon an attentive consideration of the nature of the things themselves; yet because it seems not to me so easy to make an accurate distribution of qualities, till some things that concern them be better cleared up than yet they are, I shall content my self for the present, to propose to you one of the more received divisions of physical qualities, (for you know, I do not pretend to treat of any other) allowing my self the liberty of making, where there seems cause, the members of the distribution somewhat more comprehensive. We will then, with many of the moderns, divide physical qualities into manifest and occult; and reserving the latter to be treated of apart, we will distribute the former into first, second, and third; to the two last of which we will reserve divers qualities not wont to be treated of by school writers of physical systems, which, for distinction sake, we may without much inconvenience stile some of them the chemical qualities of things, because as *Aristotle* and the school-men were not acquainted with them, so they have been principally introduced and taken notice of by means of chemical operations and experiments; such as are fumigation, amalgamation, cupellation, volatilization, precipitation, &c. by which operations, among other means, corporeal things come to appear volatile or fixed, soluble or insoluble in some menstruums, amalgable or unamalgamable, capable or uncapable to precipitate such bodies, or be precipitated by them, and, in a word, acquire or loose several powers to act on other bodies, or dispositions to be wrought on by them; which attributes do as well deserve the name of qualities, as divers other attributes, to which it is allowed. And to these chemical qualities we may add some others, which because of the use, that physicians either only, or above other men, make of them, may be called medical, whereby some bodies taken into that of a man are deoppilating, others inciding, resolving, discussing, suppurating, abstersive of noxious adherences, and thickning the blood and humours, being astringent, anodinous or appeasing pain, &c. For though some of the faculties of medicines, as those of heating, cooling, drying, attenuating, purging, &c. may be conveniently enough referred to the first, second, or third qualities wont to be mentioned by naturalists; and others are wont to be reckoned among occult ones; and though these medical qualities are wont to be treated of by physicians; yet it seems to me, that divers of them ought not to be referred to the qualities, to which they are wont to be so; and the handling of them may be looked upon as a desideratum in natural philosophy, and may well enough deserve a distinct place there; since the writers of that science are not wont to treat of them

them at all, and physicians handle them as physicians, whom it concerns but to know what bodies are endowed with them, and what good or ill effects they may have upon humane bodies, not as naturalists, whose business it is to enquire into the production and causes of those as well as of other qualities.

CHAP. II.

BEFORE we descend to the mention of any of these particular qualities, I think it very expedient to spend a little time in considering three grand scruples about our and the corpuscularian doctrine touching qualities, which three difficulties, though I remember not to have found them expresly objected by the adversaries of the corpuscularian philosophy, nor (perhaps only for that reason) to have been purposely solved by the patrons of it; are yet such, that having been suggested to me by considering the nature of the thing, I cannot but fear, that they also may occur to, and trouble you; since they seem to me of that importance, that unless they be removed, they may very much prejudice the reception of a good part of what I am to deliver about particular qualities.

THE first of the above mentioned objections is grounded upon the received opinion of vulgar and Aristotelian philosophers, that diversity of qualities must needs flow from substantial forms, either because it is part of their nature to be the principals of properties, and peculiar operations in the bodies they inform; or else because divers of them are such, that no mixture of the elements is capable of producing them.

OF the two suppositions, whereon this difficulty is founded, we have already shewed the former to be unfit to be admitted, by what has been said in our examen of substantial and subordinate forms; and therefore it will only remain, that we examine also this second supposition, which may therefore deserve the greater consideration, because it is much pressed and relied by the learned *Sennertus*, (and his followers,) who improves the argument by this addition, that as no bare mixture of the elements, so no general *forma mistionis* (such as divers of the moderns have introduced to help out the hypothesis) is sufficient to give an account of divers qualities, which he somewhere reckons up.

BUT, in the first place, whereas the proposers of this difficulty take it for granted, that there are four elements, from whose various mixtures all other sublunary bodies spring; and are therefore only solicitous to prove, that such and such qualities cannot flow from their mixture; I need not much concern my self for their whole discourse, since I admit not that hypothesis of the four elements, that is supposed in it; and yet I may be allowed to observe from hence, that by the confession of those modern peripateticks, that urge this argument, those ancient and other Aristotelians were mistaken, who ascribed to the mixture of the elements effects, for which these maintain them to be incompetent.

BUT since replies of this nature do rather concern the objectors than the objection, I proceed to consider the difficulty it self, not only as it may be proposed by peripateticks, but by chemists; who though some of them do not with others of their sect allow of the four elements, do yet agree with the schools in this, that there is a determinate number of ingredients of compounded bodies, from whose mixture and proportion many qualities must be derived; and those, that cannot, must be resolved to flow from a higher principle, whether it be a substantial form, or something, for which chemists have several names, though I doubt no settled and intelligible notion.

To consider then the difficulty it self, I shall for the removal of it present to you four principal considerations.

BUT before I begin by any of these to answer the objection, I shall readily acknowledge, that in some respects, and in some cases, it may not be ill grounded: but I shall add, that in those cases I look upon it rather as a part of the corpuscularian doctrine, than an objection against it; for when it happens, that there is a strict connexion betwixt that modification of matter, which is requisite to exhibit one phænomenon, and that from which another will necessarily follow; in such case we may not only grant, but teach, that he, who by a change of its texture gives a portion of matter the former modification, does likewise qualify it by the same change to exhibit the congruous phænomenon; though one would not perchance suspect them to have any such dependance upon one another. As for instance, strong spirit of distilled vinegar, by virtue of its being an acid spirit, hath the faculty to turn syrup of violets red: but if by making with this spirit as strong a solution as you can of corral, or some such body, you destroy the acidity of the spirit of vinegar; this liquor, as it has quite another taste, so it may, and indeed will have another operation than formerly upon syrup of violets. For I remember, that upon a trial I purposely devised to illustrate this matter, I found, that the lately mentioned solution, and some others made with spirit of vinegar, would presently like an alkalizate or urinous salt turn syrup of violets from its native blue, not any longer into a red, but into a lovely green. And prosecuting the experiment a little farther, I found, that spirit of salt it self deflegmed by a fit concrete, though the solution were horribly strong, had yet the same effect on syrup of violets. But because the cases, where the above-mentioned connection of qualities and modifications occur, are comparatively but few, I shall here consider them no farther, but proceed to the four particulars I was lately proposing.

AND in the first place, I say, that things may acquire by mixture very differing qualities from those of any of the ingredients.

OF this I shall have occasion to give a multitude of instances in the following notes upon particular qualities; and therefore it may now suffice to mention two or three, that are the more obvious in the laboratories of chemists; as, that sugar of lead is extreamly sweet, though the minium, and the ſpirit of vinegar of which it is made, be the former of them inſipid, and the latter ſour. And though neither aqua regis, nor crude copper, have any thing in them of blue; yet the ſolution of this metal in that liquor is of a deep blue; and ſometimes I have had the ſolution of crude mercury in good aqua fortis of a rich green, though it would not long continue ſo. And of ſuch inſtances you will, as I was ſaying, hereafter meet with plenty. So that they are much miſtaken, who imagine either, that no manifeſt qualities can be produced by mixture, except thoſe that reſide in the elements, or reſult immediately from the combinations of the four firſt qualities. For not to repeat what variations the mixtures of the moſt ſimple ingredients only may produce; it is manifeſt, that nature and art muſt continually make mixtures of bodies, both of already compounded bodies, as when aſhes and ſand compoſe the common coarſe glaſs, and when nature combines ſulphur with unripe vitriol, and perhaps other ſubſtances in a marchaſite; and alſo of bodies already decompounded, as native vitriol is made in the bowels of the earth of an aqueous liquor impregnated with an acid ſalt, and of a cupreous or martial mineral, ſtrictly united both to a combuſtible ſulphureous ſubſtance, and to another body of a more fixed terreſtrial nature. And thus artificers may eaſily, as trial hath aſſured me, produce new and fine colours, by ſkilfully mixing in the flame two pieces of ammels (which are already decompounded bodies) of colours more ſimple or primary than that, which reſults from their colliquation. And this way of ſo combining bodies, not ſimple or elementary, will be acknowledged capable of being made much more fertile in the production of various qualities and phænomena of nature, if you conſider, how much the variation of the proportion of the ingredients in a mixed body may alter the qualities and operations of it, and that proportion is capable of being varied almoſt *in infinitum.* Thus much may ſuffice for our firſt conſideration; eſpecially ſince divers things, by which it may be much confirmed, will be met with in the two following chapters.

In the ſecond place I obſerve, that it is but an ill grounded hypotheſis to ſuppoſe, that new qualities cannot be introduced into a mixed body, or thoſe that it had before be deſtroyed, unleſs by adding or taking away a ſenſible portion of ſome one or more of the Ariſtotelian elements, or chemical principles. For there may be many changes, as to quality, produced in a body without viſibly adding, or taking away any ingredient, barely by altering the texture, or the motion of the minute parts it conſiſts of. For when (for inſtance) water hermetically ſealed up in a glaſs is by the cold of the winter turned into ice, and thereby both looſeth its former fluidity, and tranſparency, and acquires firmneſs, brittleneſs, and oftentimes opacity, all which qualities it looſeth again upon a thaw; in this caſe, I ſay, I demand, what element or hypoſtatical principle can be proved to get into, or out of this ſealed glaſs, and by its intruſion and receſs produce theſe alterations in the included body. And ſo in that fixed metal, ſilver, what ſenſible acceſſion or decreement can be proved to be made as to ingredients, when by barely hammering it (which doth but change the ſituation and texture of the parts) it acquires a brittleneſs, which by ignition, wherein it doth not ſenſibly looſe any thing, it may preſently be made to exchange for its former malleableneſs? And the ſame experiment gives us an inſtance alſo, that the inviſible agitation of the parts may alone ſuffice, to give a body, at leaſt for a while, new qualities; ſince a thick piece of ſilver nimbly hammered will quickly acquire a conſiderable degree of heat, whereby it will be enabled to melt ſome bodies, to dry others, and to exhibit divers phænomena, that it could not produce when cold. I might add, that ſpirit of nitre, moderately ſtrong, though when included in a well ſtoped vial in the form of a liquor it will appear diaphanous, and without any redneſs, will yet fill the upper part of the vial with red fumes, if the warm ſun-beams or any fit heat (though but externally applied, and though the glaſs continue cloſe ſtopped) do put the nitrous ſpirits into a ſomewhat briſker motion, than they had or needed whilſt in the form of a liquor. I might alſo demand, both what new element or principle is added to a needle, when the bare approach of a vigorous loadſtone endows it with thoſe admirable qualities of reſpecting the poles, and (in due circumſtances) drawing to it other needles; and what ingredient the ſteel looſes, when by a contrary motion of the loadſtone it is in a minute deprived of its magnetiſm. And to theſe I might ſubjoin divers like queſtions: but of inſtances and reflections proper to confirm this ſecond conſideration, you may meet with ſo many, partly in another treatiſe, and partly in the enſuing chapters, that it will be needleſs to multiply them here. Wherefore in the third place, I ſhall obſerve, that when we are conſidering, how numerous and various phænomena may be exhibited by mixed bodies, we are not to look upon them preciſely in themſelves; that is, as they are portions of matter, of ſuch a determinate nature, or texture; but as they are parts of a world ſo conſtituted as ours is, and conſequently as portions of matter, which are placed among many other bodies. For being hereby fitted to receive impreſſions from ſome of thoſe bodies, and to make impreſſions upon others of them, they will upon this account be rendered capable of producing, either as principal, or auxiliary cauſes, a much greater number and variety of phænomena, than they could exhibit, if each of them were placed in *vacuo*, (or if a vacuum be a thing impoſible) in a medium, that could no way either contribute to, or hinder its operations.

This hath been partly proved already in the diſcourſe of the origin of forms, and will be farther manifeſted ere long; and therefore it may ſuffice, that of the particulars mentioned in thoſe writings, thoſe that are pertinent to this argument be mentally referred hither.

WHERE-

Wherefore having thus difpatched the third confideration, I now proceed to the fourth and laft, which is, that the four peripatetick elements and the three chemical principles are fo infufficient to give a good account of any thing near all the differing phænomena of nature, that we muft feek for fome more catholick principles; and that thofe of the corpufcularian philofophy have a great advantage of the other in being far more fertile and comprehenfive than they. I muft not here ftay to make full reprefentation of the deficiencies of the Ariftotelian hypothefis, having in other tracts faid much to that purpofe already; but yet our prefent argument invites me to intimate thefe two things; the firft, that fuch phænomena, as the conftant and determinate fhape and figure of the mountains, our telefcopes difcover (together with their fhadows) in the moon, and the ftrange generation and perifhing of the fpots of the fun, to omit the differing colour of the planets, and divers other qualities of celeftial bodies, cannot be afcribed to the four elements, or their mixtures, nor to thofe of the three chemical principles, which are allowed to be confined to the fublunary region. And the fecond, that there are very many phænomena in nature (divers of which I * elfewhere take notice of) feveral whereof neither the peripatetick nor the chemical doctrine about the elements, or the ingredients of bodies, will enable a man to give fo much as any probable account. Such are the eclipfes of the fun, the moon, and alfo the fatellites of Jupiter, the proportion of the acceleration of defcent obfervable in heavy bodies, the ebbing and flowing of the fea, a great number of magnetical, mufical, ftatical, dioptrical, catoptrical, and other forts of phænomena, which hafte makes me here leave unmentioned.

And having faid thus much about the firft part of our propofed confideration, and thereby fhewn, that the vulgar doctrine about the ingredients of bodies falls very fhort of being able to folve feveral kinds of natures phænomena; we may add in favour of the fecond part, that, it will follow in general, that it is fit to look out for fome more pregnant and univerfal principles; and that, in particular, thofe of the corpufculary hypothefis are, as to thofe two attributes, preferable by far to the vulgar ones, will I hope appear by our anfwers to the two objections, that remain to be examined in the two following chapters, to which that I might the more haften, I thought fit to infift the lefs upon the objection hitherto examined, efpecially becaufe partly in this and the two next chapters, and partly elfewhere, I fuppofe there is contained a very fufficient reply to that objection. And I confefs I fhould think it ftrange, that the confideration of the various motions and textures of bodies fhould not ferve to folve far more phænomena, than the bare knowledge of the number (and even that of the proportions) of their quiefcent ingredients: for as local motion is that, which enables natural bodies to act upon one another, fo the textures of bodies are the main things, that both modify the motion of agents, and diverfify their effects according to the various natures of the patients.

CHAP. III.

I ENTER now upon the confideration of the fecond, and indeed the grand difficulty objected againft the (corpufcularian) doctrine propofed by me about the origin of qualities, viz. that it is incredible, that fo great a variety of qualities, as we actually find to be in natural bodies, fhould fpring from principles fo few in number as two, and fo fimple as matter and local motion; whereof the latter is but one of the fix kinds of motion reckoned up by *Ariftotle* and his followers, who call it lation, and the former, being all of one uniform nature, is according to us diverfified only by the effects of local motion. Towards the folving this difficulty, I fhall endeavour to fhew, firft, that the other catholick affections of matter are manifeftly deducible from local-motion: and next, that thefe principles being varioufly affociated, are fo fruitful, that a vaft number of qualities and other phænomena of nature may refult from them.

The firft of thefe, will not take us up much time to make out. For fuppofing, what is evident, that the (1) local-motion belonging to fome parts of the univerfal matter, does not all tend the fame way, but has various determinations in feveral parts of that matter; it will follow, that by local-motion thus circumftanced, matter muft be divided into diftinct parts; each of which being finite, muft neceffarily be of fome (2) bignefs or fize, and have fome determinate (3) fhape or other.

And fince all the parts of the univerfal matter are not always in motion, fome of them being arrefted by their mutual implication, or having transferred (as far as our fenfes inform us) all that they had to other bodies, the confequence will be, that fome of thefe portions of the common matter will be, in a ftate of (4) reft (taking the word in the popular fence of it.) And thefe are the moft primary and fimple affections of matter.

But becaufe there are fome others, that flow naturally from thefe, and are, though not altogether univerfal, yet very general and pregnant; I fhall fubjoin thofe, that are the moft fertile principles of the qualities of bodies and other phænomena of nature.

Moreover, then, not only the greater fragments of matter, but thofe leffer ones, which we therefore call corpufcles or particles have certain local refpects to other bodies, and to thofe fituations, which we denominate from the horizon; fo that each of thefe minute fragments may have a particular (5) pofture, or pofition (as erect, inclining, horizontal, &c.) and as they refpect us men, that behold them, there may belong to them a certain (6) order or confecution, upon whofe fcore we fay one is before or behind another; and many of thefe fragments being affociated into one mafs or body, have a certain manner of exifting to-

* Principally in the Sceptical Chymift.

gether

gether, which we call (7) texture, or by a word more comprehensive, modification. And because there are very few bodies, whose constituent parts, can, because of the irregularity or difference of their figures, and for other reasons, touch one another every where so exquisitely, as to leave no intervals between them, therefore almost all consistent bodies, and those fluid ones, that are made up of grosser parts, will have (8) pores in them, and very many bodies having particles, which by their smallness, or their loose adherence to the bigger, or more stable parts of the bodies they belong unto, are more easily agitated and separated from the rest by heat and other agents; therefore there will be great store of bodies, that will emit those subtle emanations, that are commonly called (9) effluviums. And as those conventions of the simple corpuscles, that are so fitted to adhere to, or be complicated with one another, constitute those durable and uneasily dissoluble clusters of particles, that may be called the primary concretions or elements of things: so these themselves may be mingled with one another, and so constitute compounded bodies; and even those resulting bodies may by being mingled with other compounds, prove the ingredients of decompounded bodies; and so afford a way, whereby nature varies matter, which we may call (10) mixture, or composition; not that the name is so proper as to the primary concretions of corpuscles; but because it belongs to a multitude of associations, and seems to differ from texture, with which it hath so much affinity, as perhaps to be reducible to it, in this, that always in mixtures, but not still in textures, there is required a heterogeneity of the component parts. And every distinct portion of matter, whether it be a corpuscle or a primary concretion, or a body of the first, or of any other order of mixts, is to be considered, not as if it were placed in vacuo, nor as if it had relation only to the neighbouring bodies, but as being placed in the universe, constituted as it is, amongst an innumerable company of other bodies, whereof some are near it, and others very remote, and some are great and some small, some particular and some catholick agents, and all of them governed as well by (11) the universal fabrick of things, as by the laws of motion established by the author of nature in the world.

And now, *Pyrophylus*, that we have enumerated 11 very general affections of matter, which with itself make up 12 principles of variation in bodies; let me on the behalf of the corpuscularians apply to the origine of qualities a comparison of the old atomists imployed by *Lucretius*, and others, to illustrate the production of an infinite number of bodies, from such simple fragments of matter as they thought their atoms to be. For since of the 24 letters of the alphabet associated several ways, as to the number and placing of the letters, all the words of the several languages in the world may be made; so, say these naturalists, by variously connecting such and such numbers of atoms, of such shapes, sizes, and motions, into masses or concretions, an innumerable multitude of different bodies may be formed. Wherefore, if to those four affections of matter, which I lately called the most primary and simple, we add the seven other ways, whereby, or on whose account, it may be altered, that are, though not altogether, yet almost as catholick, we shall have eleven principles so fruitful, that from their various associations may result a much vaster multitude of phænomena, and among them of qualities, than one, that does not consider the matter attentively, would imagine. And to invite you to believe this, I shall desire you to take notice of these three things.

The first is, that supposing these ten principles were but so many letters of the alphabet, that could be only put together in differing numbers, and in various orders; the combinations and other associations, that might be made of them, may be far more numerous than you your self will expect, if you are not acquainted with the way of calculating the number of differing associations, that may be made between ten things proposed. The best way I know of doing this is by algebra or symbolical arithmetick, by which it appears, that of so few things so many (α) associations may be made, each of which will differ from every one of the rest, either in the number of the things associated, or in the order wherein they were placed.

But (which is the second thing to be taken notice of) each of these ten producers of phænomena admits of a scarce credible variety. For not to descend so low as insensible corpuscles (many thousands of which may be requisite to constitute a grain of mustard seed) what an innumerable company of different bignesses may we conceive between the bulk of a mite, a crowd of which is requisite to weigh one grain, and a mountain, or the body of the sun, which astronomers teach us to be above an hundred and threescore times bigger than the whole terrestrial globe?

And so though (β) figure be one of the most simple modes of matter; yet it is capable partly in regard of the surface, or surfaces of the figured corpuscles (which may consist of triangles, squares, pentagones, &c.) and partly in regard of the shape of the body itself, which may be either flat like a cheese, or lozenge; or sphærical like a bullet; or elliptical, almost like an egg; or cubical like a dye; or cylindrical like a rowling-stone; or pointed like a pyramid, or sugar-loaf: figure I say, though but a simple mode, is, upon these and other scores, capable of so great a multitude of differences, that it is concerning them, and their affections, that *Euclid*, *Apollonius*, *Archimedes*, *Theodosius*, *Clavius*, and later writers than he, have demonstrated so many propositions. And yet all the hitherto named figures are almost nothing to those irregular shapes, such as are to be met with among rubbish, and among hooked and branched particles, &c. that are to be met with among corpuscles and bodies; most of which have no particular appellations, their multitude and their variety having kept men from enumerating them, and much more from particular naming them.

To which let me add, that these varieties of figure and shape do also serve to modify the motion, and other affections of the corpuscle endowed with them, and of the compounded body, whereof it makes a part.

And that the (γ) shape and also size of bodies, whether small or great, may exceedingly diversify their nature and operation, I shall often have occasion to manifest, and therefore I shall now only give you a gross example of it; by inviting you to consider, how many differing sorts of tools and instruments, almost each of them fit for many different operations and uses, smiths, and other not the noblest sort of tradesmen, have been able to form out of pieces of iron, only by making them of differing sizes, and giving them differing shapes. For when I have named bodkins, forks, blades, hooks, scissars, anvils, hammers, files, rasps, chissels, gravers, screws, vices, saws, borers, wires, drills, &c. when (I say) I have named all these, I have left a far greater number undermentioned.

So likewise (δ) motion, which seems so simple a principle, especially in simple bodies, may even in them be very much diversified. For it may be more or less swift, and that in an almost infinite diversity of degrees. It may be simple or compounded, uniform, or difform, and the greater celerity may preceed or follow. The body may move in a streight line, or in a circular, or in some other curve line, as elliptical, hyperbolical, parabolical, &c. of which geometricans have described several, but of which there may be in all I know not how many more; or else the body's motion may be varied according to the situation, or nature of the body it hits against, as that is capable of reflecting it, or refracting it, or both, and that after several manners: the body may also have an undulating motion, and that with smaller or greater waves; or may have a rotation about its own middle parts; or may have both a progressive motion, and a rotation, and the one either equal to the other, or swifter than it, in almost infinite proportions. As to the determination of motion, the body may move directly upwards, or downwards, decliningly, or horizontally, east, west, north, or south, &c. according to the situation of the impellent body. And besides these and other modifications of the motion of a simple corpuscle or body, whose phænomena or effects will be also diversified, as I partly noted already, by its bulk, and by its figure: besides all these, I say, there will happen a new and great variety of phænomena, when divers corpuscles, though primogenial, and much more if they be compounded, move at once, and so the motion is considered in several bodies. For there will arise new diversifications from the greater or lesser number of the moving corpuscles; from their following one another close, or more at distance; from the order, wherein they follow each other; from the uniformity of their motion, or the confusedness of it; from the equality, or inequality of their bulk, and the similitude or dissimilitude of their figures; from the narrowness or wideness, &c. of the channel or passage, in which they move, and the thickness, thinness, pores, and the conditions of the medium, through which they move; from the equal or unequal celerity of their motion, and force of their impulse: and the effects of all these are variable by the differing situation and structure of the sensories, or other bodies on which these corpuscles beat.

What we have elsewhere said, to shew that local motion is, next the author of nature, the principal agent in the production of her phænomena, may I hope satisfy you, that these diversities in the motion of bodies may produce a strange variety in their nature and qualities. And as I lately did, so I shall now adumbrate my meaning to you, by desiring you to apply to our present purpose what you may familiarly observe in musick. For according as the strings, or other instruments of producing sounds, do tremble more or less swiftly, they put the air into a vibrating motion more or less brisk, and produce those diversities of sounds, which musicians have distinguished into notes, which they have also subdivided, and whereto they have given distinct names. And though the bodies, from whence these sounds proceed, may be of very differing (ε) natures; as metalline, as wire, gutstrings, bells, humane voices, wooden pipes, &c. yet provided they put the air into the like waving motion, the sound and even the note will be the same: which shews how much that greater variety, which may be taken notice of in sounds, is the effect of local motion. And if the sound come from an instrument, as a lute, where not only one string hath its proper sound, but many have among them several degrees of tension, and are touched, sometimes these, sometimes those, together; whereby more, or fewer, or none of their vibrations come to be coincident, they will so strike the air, as to produce, sometimes those pleasing sounds we call concords, and sometimes those harsh ones we call discords.

It would take up too much time to insist upon each of the ten remaining affections of matter, that I lately enumerated and represented to you as exceeding fertile; and by what I elsewhere deliver about pores alone, and the many sorts of phænomena, in which they may have an interest, I could add no small confirmation to what has been hitherto discoursed; if the inserting of it here would not enormously increase the bulk of this paper, which I rather decline doing, because what has been already said of those we have now, though we have but very breifly treated of, may, I hope, be sufficient to perswade you, that such principles as these are capable of being made far more pregnant, than one would expect so few principles should be. And this persuasion will be much facilitated, if we consider, how great a variety may be produced, not only by the diversifications, that each single principle (upon the score of the attributes that may belong to it) is capable of; but much more by the several (ζ) combinations, that may be made of them, especially considering withal, that our external and internal senses are so constituted,

stituted, that each, or almost each, of those diversifications or modifications may produce a distinct impression on the organ, and a correspondent perception in the discerning faculty; many of which perceptions, especially if distinguished by proper names, belong to the list of particular qualities.

CHAP. IV.

THE third and last difficulty, that now remains to be considered, may be thus proposed: that whereas, according to the copuscularian hypothesis, not only one or two qualities, but all of them proceed from the bigness, and shape, and contexture of the minute parts of matter, it is consonant to their principles, that if two bodies agree in one quality, and so in the structure, on which that quality depends, they ought to agree in other qualities also; since those do likewise depend upon the structure, wherein they do agree; and consequently it will be scarce possible to conceive, that two such bodies should be endowed with so many differing qualities, as experience shews they may.

To illustrate this objection by an example, it is pretended, that the whiteness of froth proceeds from the multitude and hemispherical figure of the bubbles it is made up of. And if this or any other mechanical fabrick or contexture be the cause of whiteness, how comes it to pass, that some white bodies are inodorous and insipid, as the calx of harts-horn; others both strongly scented, and strongly tasted, as the volatile salt of harts-horn or of blood; some dissoluble in water, as salt of tartar; others indissoluble in that liquor, as calcined harts-horn, &c. some fixed in the fire, as the bodies last named; others fugitive, as powdered sal armoniack; some incombustible, as salt of tartar; others very inflammable as camphire. To which examples a greater variety of white bodies might be added, if it were necessary.

THIS I confess is a considerable difficulty may puzzle more than a novice in the corpuscularian philosophy: wherefore to do somewhat in order to the clearing of it, I shall recommend to you the four following considerations:

1. AND first I shall consider, that in the pores of visible and stable bodies, there may be often lodged invisible and heterogeneous corpuscles, to which a particular quality, that belongs to the body as such, is to be referred. Thus we see in a perfumed glove, that in the pores of the leather odoriferous particles are harboured, which are of quite another nature than the leather it self, and wholly adventitious to it, and yet endue it with the fragrancy, for which it is prized. A like example is afforded us in rasberry wine made with claret. For the pleasing smell is imparted to the wine, by the corpuscles of the berries dispersed *per minima* through the whole body of it.

2. THE second thing that I consider is this, that oftentimes corpulcles of very differing natures, if they be but fitted to convene, or to be put together after certain manners, which yet require no radical change to be made in their essential structures, but only a certain juxtaposition or peculiar kind of composition; such bodies, I say, may notwithstanding their essential differences exhibit the same quality. For invisible changes made in the minute, and perhaps undescernible parts of a stable body may suffice to produce such alterations in its texture, as may give it new qualities, and consequently differing from those of other bodies of the same kind or denomination; and therefore though there remains as much of the former structure, as is necessary to make it retain its denomination, yet it may admit of alterations sufficient to produce new qualities. Thus when a bar of iron has been violently hammered, though it continues iron still, and is not visibly altered in its texture; yet the insensible parts may have been put into so vehement an agitation, as may make the bar too hot to be held in one's hand. And so if you hammer a long and thin piece of silver, though the change of texture will not be visible, it will acquire a springiness, that it had not before. And if you leave this hammered piece of silver a little while upon the glowing coals, and after let it cool, though your eye will perchance as little perceive, that the fire has altered its texture, as it did before, that the hammer had; yet you will find the elasticity destroyed.

IF on the surface of a body there arise or be protuberant a multitude of sharp and stiff parts, placed thick or close together, let the body be iron, silver, or wood, or of what matter you please, these extant and rigid parts will suffice to make all these bodies to exhibit the same quality of asperity, or roughness.

AND if all the extant parts of a (physical) superficies be so depressed to a level with the rest, that there is a co-œquation, if I may so speak, made of all the superficial parts of a body; this is sufficient to deprive it of former roughness, and give it that contrary quality we call smoothness. And if this smoothness be considerable exquisite, and happen to the surface of an opacous body of a close and solid contexture, and fit to reflect the incident rays of light and other bodies unperturbed; this is enough to make it specular, whether the body be steel, or silver, or brass, or marble, or flint, or quicksilver, &c.

AND so, as I noted in the last chapter on another occasion, if a body be so framed and stretched, as being duly moved by another body to put the air into an undulating motion, brisk enough to be heard by us, we call that sonorous, whether it be a metalline bell, or gut-strings, or wires, &c. Nay if waving motions, whereinto the air is put by such differing bodies, be alike, these bodies will not only in general give a sound, but will yield that particular degree of sound, that men call the same note.

FOR here it is to be considered, that besides that peculiar and essential modification, which constitutes a body, and distinguishes it from all others, that are not of the same species, there may be certain other attributes, that we call extra-essential; which may be common to that body

body with many others, and upon which may depend thoſe more external affections of the matter, which may ſuffice to give it this or that relation to other bodies, divers of which relations we ſtile qualities.

Of this I ſhall give you an evident example in the production of heat. For provided there be a ſufficient and confuſed agitation made in the inſenſible parts of a body, whether it be iron or braſs, or ſilver, or wood, or, ſtone; that vehement agitation, without deſtroying the nature of the body that admits it, will fit it for ſuch an operation upon our ſenſe of feeling, and upon bodies eaſy to be melted (as butter, wax, &c.) as we call heat.

And ſo in the inſtance named in the objection about whiteneſs; it is accidental to that quality, that the corpuſcles it proceeds from ſhould be little hemiſpheres. For though it happen to be ſo in water agitated into froth; yet in water frozen to ice, and beaten very ſmall, the corpuſcles may be of all manner of ſhapes; and yet the powder be white. And it being ſufficient to the producing of whiteneſs, that the incident light be reflected copiouſly every way, and untroubled by the reflecting body, it matters not, whether that body be water, or white wine, or ſome other clear liquor turned into froth, or ice, or glaſs, or cryſtal, or clarified rozin, &c. beaten into powder; ſince without diſſolving the eſſential texture of theſe formerly diaphanous bodies, it ſuffices, that there be a comminution into grains numerous and ſmall enough by the multitude of their ſurfaces, and thoſe of the air, or other fluid, that gets between them, to hinder the paſſage of the beams of light, and reflect them every way, as well copiouſly, as unperturbed.

Perhaps it may not be impertinent to add to this, that there may be other catholick affections of corpuſcles, beſides the ſhape or ſtructure of them, by virtue whereof aggregates even of ſuch as are (as to ſenſe) homogeneous, may exhibit differing qualities: as for inſtance, they may have ſome, when they are in a briſk motion, and others, when they are but in a languid one, or at reſt: as ſalt petre, when its parts are ſufficiently agitated by the fire in a crucible, is not only fluid but tranſparent almoſt like water; whereas when it cools again, it becomes a hard and white body; and butter, that is opacous in its moſt uſual ſtate, may be diaphanous when it is melted. So I ſhall hereafter have occaſion to ſhew you, that a great quantity of beaten alablaſter, which uſually retains the form of a moveleſs heap of white powder, by being after a due manner expoſed to heat, obtains, and that without being brought to fuſion, many of the principal qualities of a fluid body. And if with good ſpirit of nitre, or aqua fortis, you fill a glaſs half full, it will (unleſs it be extraordinarily deflegmed) exhibit no redneſs, nor approaching colour in the veſſel: but if you warm it a little, or caſt into it a bit of iron or of ſilver, that it may put the liquor into a commotion, then the nitrous ſpirits deveſting the form of a liquor, and aſcending in that of fumes, will make all the upper part of the glaſs look of a deep yellow, or a red.

3. The third thing I would recommend to your conſideration is, to reflect on what I propoſed in the laſt foregoing ſection, where I told you, that in reference to the production of qualities, a body is not to be conſidered barely in itſelf, but as it is placed in, and is a portion of the univerſe. But of this ſubject I have ſaid ſo much in the newly mentioned diſcourſe, and in that which you are there referred to, that I ſhall now only put you in mind, that divers of the particulars to be met with in thoſe diſcourſes are applicable to our preſent purpoſe.

4. To all this let me add in the laſt place, that, as to that part of the grand objection that we are clearing, which urges the difficulty of explicating upon the corpuſcular principles, how, for example, the ſame body, whoſe ſtructure makes it ſhaped ſo as to be fit to exhibit whiteneſs, ſhould likewiſe have divers other qualities, that ſeemed to have no affinity with whiteneſs. This ſcruple, I ſay, we may, by what we have already diſcourſed, be aſſiſted to remove; eſpecially if we ſubjoin another conſideration to it. For if corpuſcles without loſing that texture, which is eſſential to them, may (as we have ſhewed they may) have their ſhape, or their ſurfaces, or their ſituation changed; and may alſo admit of alterations, (eſpecially as theſe corpuſcles make up an aggregate or congeries,) as to motion or reſt, as to theſe or thoſe degrees, or other circumſtances of motion; as to laxity and denſity of parts, and divers other affections; why ſhould we not think it poſſible, that a ſingle, though not indiviſible, corpuſcle, and much more an aggregate of corpuſcles, may by ſome of theſe, or the like changes, which, as I was ſaying, deſtroy not the eſſential texture, be fitted to produce divers other qualities, beſides theſe that neceſſarily flow from it? Eſpecially conſidering (which is that I have now to add) that the qualities commonly called ſenſible, and many others too, being according to our opinion but relative attributes, one of theſe now mentioned alterations, though but mechanical, may endow the body, it happens to, with new relations both to the organs of ſenſe, and alſo to ſome other bodies, and conſequently may endow it with additional qualities.

If from good venice or other turpentine you gently evaporate, or abſtract about a third part of its whole weight; you may obtain a fine tranſparent, and almoſt reddiſh colophony. If you beat this very ſmall, it will looſe its colour and tranſparency, and will afford you an opacous and very white powder. If you expoſe this to a moderate heat, it will quickly and without violence both regain its colour and tranſparency, and acquire fluidity. And if, whilſt it is thus melted, you put the end of a quill or reed a little beneath the ſurface, and blow ſkilfully into it, you may obtain bubbles adorned with very various and vivid colours. If when it has loſt its fluidity, but whilſt it is yet pretty warm, you take it into your hands, you will find, that it has in that ſtate a viſcoſity, by vertue of which you may draw it out into threads, as you may past; but as soon as it grows quite cold, it becomes exceeding brittle: and if whilst it is is yet warm, you give it the shape of a triangular prism, and make it of a convenient bulk, it will exhibit variety of colours almost like a triangular glass. Whilst this colophony is cold, and its parts are not put into a due motion, straws and other light bodies may be held unmoved close to it; but if by rubbing it a little you put the parts into a convenient agitation, though perhaps without sensibly warming the colophony, it displays an electrical quality, and readily draws to it the hairs, straws, &c. that it would not move before. All or most of these things you may also perform, if I mistake not, with clarified rosin, though I am not sure it will do so well.

To this I shall add one instance more, which may let you see, how the same body, which the chemists themselves will tell you is simple and homogeneous, may, by vertue of its shape, and other mechanical affections, (for it is a factitious body, and that is made by the destruction of a natural one) have such differing respects to different sensories, and to the pores, &c. of divers other bodies, as to display several very differing qualities. The example I speak of, is afforded me by the distillation of putrefied urine. For though such urine have already lost its first texture before it come to be distilled; yet when it has undergone two or three distillations to deflegm it, the spirits of it swimming in a phlegmatick vehicle have a pungent saltness upon the tongue, and a very strong, and to most persons an offensive smell in the nostrils; and when they are freed from the water, they are wont to appear white to the eye; and to very tender parts, as to those that are excoriated, or to the conjunctiva they feel exceeding sharp, and seem to burn almost like a caustick, not to say like fire; insomuch that I have seen them presently make blisters upon the tongue itself, that was not raw or sore before they touched it; the same saline particles invisibly flying up to the eyes prick them, and make them water; and invading the nose often cause that great commotion in the head and other parts of the body, that we call sneezing. The same corpuscles, if they are much smelt to by a woman in hysterical fits, do very often suddenly relieve her, and so may be reckoned among the specifick remedies of that odd and manifold disease, which is not the only one, in which they are considerable medicines, as we have elsewhere declared. The same corpuscles taken into human bodies have the qualities, that in other medicines we call diaphoretick, and diuretick; the same particles being put upon filings of brass produce a fine blue, whereas upon the blue or purple juices of many plants they presently produce a green; being put to work upon copper, whether crude, or calcined, they do readily dissolve it, as corrosive menstruums are wont to do other metals; and yet the same corpuscles being blended in a due proportion with the acid salts of such menstruums, have the virtue to destroy their corrosiveness; and if they be put into solutions made with such menstruums, they have a power, excepting in very few cases, to precipitate the bodies therein dissolved. I might here add, *Pyrophylus*, how the same particles applied to several other bodies, to which they have differing relations, have such distinct operations on them, as may intitle these saline spirits to other qualities. But to enumerate them in this place were tedious, especially having already named so many qualities residing in this spirituous salt; which I therefore the rather pitched upon, because being a factitious body, and made out of a putrefied one, and so simple as to be a chemical salt (which, you know, spagyrists make one of the three principles of compounded bodies) I suppose you will make the less scruple to admit, that it works by virtue of its mechanical affections. Of which to persuade you the more, I shall add, that if you compound this urinous salt with the saline particles of common salt (which is also a factitious thing, and confessed by chemists to be a simple principle of the concrete, that yeilds it) these two being mingled in a due proportion, and suffered leisurely to combine, will associate themselves into corpuscles, wherein the urinous salt loses most of the qualities I have been ascribing to it, and with the acid spirit composes, as I have often tried, a body little differing from sal armoniack: which great change can be ascribed to nothing so probably, as to that of the shape and motion (not here to add the size) of the urinous salt, which changes the one, and loses a great part of the other by combining with the acid spirits. And to confirm that both these do happen, I have several times slowly exhaled the superfluous, but not near the whole liquor from a mixture made in a due proportion of the spirit of urine and that of salt, and found, that answerably to my conjecture, there remained in the bottom a salt, not only far more sluggish than the fugitive one of urine, but whose visible shape was quite differing from that of the volatile crystals of urine, this compounded salt being generally figured, either like combs or like feathers.

If after all this we do either add or inculcate, that the extraessential changes, that may be made in the shape, contexture and motion, &c. of bodies, that agree in their essential modifications, may not only qualify them to work themselves immediately after a differing manner upon differing sensories, and upon other bodies also, whose pores, &c. are differently constituted, but may dispose them to receive other impressions than before, or receive wonted ones after another manner from the more catholick agents of nature; if, I say, we recommend this also to your consideration, what has been delivered in the whole discourse will I hope let you see, that the scruple proposed at the beginning of it is not so perplexing a one to our philosophy, as perhaps you then imagined it.

The three difficulties considered partly in this, and partly in the two foregoing sections, I was the more inclined to take notice of in this place, (for in divers other passages of my writings

writings you will meet with things, that are applicable to the past discourse, and should be referred thither) partly because the scruples themselves are of great moment, and for ought I know have been discussed by others; and partly because these difficulties relating in some sort to the corpuscularian hypothesis in the general, the clearing of them may both serve to confirm several of these things, that have above been written about the origin of forms and qualities (to which it might therefore have been joined) and will be conducive to a clear understanding, and explicating divers of the particulars that I am about to deliver, and perhaps several other phænomena of nature.

OF THE SYSTEMATICAL or COSMICAL QUALITIES of THINGS.

CHAP. I.

I EXPECT, *Poriphilus*, that being somewhat surprized at the title of this discourse, you will presently ask, what I understand by Cosmical or Systematical Qualities; that name being new enough to require, that I should tell you, both what is meant by it, and why I make choice of it.

To answer so reasonable a question, I shall inform you, that I consider, that the qualities of particular bodies (for I speak not here of magnitude, shape, and motion, which are the primitive modes and catholick affections of matter itself) do for the most part consist in relations, upon whose account one body is fitted to act upon others, or disposed to be acted on by them, and receive impressions from them; as quick-silver has a quality or power (for I here take qualities in the larger sense) to dissolve gold and silver, and a capacity or disposition to be dissolved by aqua fortis, and (though less readily) by aqua regis. And this being premised, I observe farther, that, though in estimating the qualities of natural bodies, we are wont to consider but the power any particular one has of acting upon, or the capacity it has of suffering from such and such particular bodies, wherewith it is taken notice of to have manifest commerce in point of making or of receiving impressions; yet there may be some attributes, which may belong to a particular body, and divers alterations, to which it may be liable, not barely upon the score of these qualities, that are presumed to be evidently inherent in it, nor of the respects it has to those other particular bodies, to which it seems to be manifestly related, but upon the account of a system so constituted as our world is, whose fabrick is such, that there may be divers unheeded agents, which, by unperceived means, may have great operations upon the body we consider, and work such changes it, and enable it to work such changes on other bodies, as are rather to be ascribed to some unheeded agents, than to those other bodies, with which the body proposed is taken notice of to have to do. So that although if divers bodies, that I could name, were placed together in *vacuo*, or removed together into some of those imaginary spaces, which divers of the schoolmen fancy to be beyond the bounds of our universe, they would retain many of the qualities they are now endowed with; yet they would not have them all: but by being restored to their former places in this world, would regain a new set of faculties (or powers) and dispositions, which, because they depend upon some unheeded relations and impressions, which these bodies owe to the determinate fabrick of the grand system or world they are parts of, I have, until I can find a more proper appellation, thought fit to name their cosmical or their systematical qualities.

I have in the Origin of Forms touched upon this subject already, but otherwise than I am now about to do. For whereas that which I do principally, (and yet but transiently,) take notice of, is, that one body being surrounded with other bodies, is manifestly wrought on by many of those among whom it is placed; that which I chiefly in this discourse consider, is the impressions, that a body may receive, or the power it may acquire, from those vulgarly unknown, or at least unheeded agents, by which it is thus affected, not only upon the account of its own peculiar texture or disposition, but by virtue of the general fabrick of the world.

CHAP. II.

NOW though there be several of the grand mundane bodies, and divers laws and customs of nature, which may contribute (more or less) to the phænomena of the qualities we are treating of; yet because a distinct and particular inquiry into each of them would challenge a much longer discourse than this short essay is to be, and a much abler pen than his that writes it; I did not only think it fit to reserve what occurs to me about the laws and customs of nature, as they concern this subject, to another discourse, or an appendix to this; but to declare to you also, that whereas the three main bodies, whose more unobserved operations

rations and changes have the most considerable influence on the qualities we are to treat of, are, the subterraneal parts of the globe we inhabit; the stars, whether fixed or wandring, with the æther that is about them; and the atmosphere or air we live in; I foresee, that it will be requisite for me to assign the experiments and observations I have collected about these three subjects to other tracts. So that in this essay my chief work will be, to take notice to you of some considerations, that may be introductory in a more general way to the clearer knowledge of the subject to be discoursed of. To which I may, as time and my occasions may permit, subjoin some particulars, which, though perhaps they do not all of them so directly or properly belong to the solemnly proposed heads of this discourse, yet are not impertinent to the design of it; and on that score may be allowed their places in it.

And least you should think, that under the name of cosmical qualities, I should introduce chimæras into natural philosophy, I must betimes advertise you, that you will meet with divers particles in the following discourse, fit to shew, that these qualities are not meerly fictitious qualities; but such, whose existence I can manifest, not only by considerations not absurd, but also by real experiments and physical phænomena. And to prevent mistakes, I shall add, that under the name of catholick and unminded causes or agents, I comprehend not only divers invisible portions of matter, but also the established laws of the universe, or that which is commonly called the ordinary course of nature. And when I speak of unobserved agents or causes, I do not always mean, that they are not known or taken notice of to be *in rerum natura*, but that they are not vulgarly considered or looked upon, as the causes of some particular phænomena, wherein I ascribe to them an interest or efficiency.

But before I proceed any farther, it will not be amiss to intimate in this place, that the things, on which I founded the purposed notion of the cosmical attributes of bodies, were principally these three;

1. That there are many bodies, that in divers cases act not, unless they be acted on; and some of them act, either solely or chiefly, as they are acted on by the catholick and unheeded agents, we have been speaking of.

2. That there are certain subtle bodies in the world, that are ready to insinuate themselves into the pores of any body disposed to admit their action, or by some other way affect it, especially if they have the concurrence of other unobserved causes and the established laws of the universe.

3. That a body by a mechanical change of texture may acquire or loose a fitness to be wrought upon by such unheeded agents, and also to diversify their operations on it upon the score of its varying texture.

These three propositions I shall endeavour to confirm distinctly by the ensuing experiments and phænomena: but because divers of these proofs may each of them serve to confirm more than one of these propositions, and because the making out of the two last, which are the most important (and the least probable) is the main design of this discourse, I shall say the less to the first, leaving it for the most part to you, to refer to either of the three propositions what you shall meet with belonging to it in what is said upon either of the other two.

CHAP. III.

To begin then with the first proposition, namely, "That there are many bodies, "that in divers cases act not, unless they be "acted on; and some of them act either solely "or chiefly, as they are acted on by the catho- "lick and unheeded agents, we have been "speaking of:" the former part of it will, I presume, be easily granted, it being evident by such gross instances as these, that a wedge will not cleave a block, unless it be impelled against it by a hammer (or some æquivalent instrument) nor a knife attract a needle, unless it be excited by a magnet. But as to the second, it will not in likelihood be so readily assented to, and therefore having *in transitu* illustrated it by observing to you, that concave looking-glasses, and convex burning-glasses, kindle not other bodies, unless they be enabled to do so by the reflected or trajected beams of the sun, I shall proceed to prove it by a couple of instances.

The one is, an iron bar, that hath long stood in a window, or some other fit place in a perpendicular posture; for though this bar was not, when it was first erected, endowed with a magnetism any thing superior to that of other iron bars of the like shape and bigness, yet after it hath very long stood in that position, it will by the operations of invisible agents acquire a farther degree of magnetism, than belonged to it, as a bar of iron, and is enabled to produce some magnetical phænomena (elsewhere mentioned) that it could not before.

The second instance is afforded us by what happens to a very flat and exquisitely polished piece of marble; for though of itself it hath no power to help to lift up any other dry body that it is laid upon, yet if it come to be skilfully laid upon another piece of marble as flat and smooth as it, and of a bulk not too unweildy; this upper stone, by virtue of the fabrick of the world, which gives the ambient air fluidity and weight, is enabled without any other cement or fastening instrument than immediate contact, to raise with itself (in case a man lift it up) the lower marble, though perhaps an hundred times heavier than itself. * [Whereas, if this laying one of these stones upon the other had been done *in vacuo*, I doubt not but no such power had thereby accrued to the uppermost of them.]

CHAP.

* See this experimentally proved in the Continuation of the Author's New Experiments touching the air, experiment the fiftieth,

CHAP. IV.

PROCEED we now to our second proposition, which speaks to this purpose; "That there are certain subtle bodies in the "world, that are ready either to insinuate them"selves into the pores of any body, disposed "to admit their actions; or by some other "way to affect it; especially if they have the "concurrence of other unobserved causes, and "the established laws of the universe." I need not take notice on this occasion, that divers of the ancient philosophers thought, that there was a subtler body than the common air, and called æther; and that the Cartesians tells us, that there is such a substance diffused throughout the universe, which they call, according to the differing sizes of its parts, sometimes *primum elementum*, and sometimes *materia cœlestis*, to which they attribute the use of pervading all other bodies, and adequately filling those pores of theirs, that are correspondent in bigness and figure to the differing portions of this insinuating matter. That there may be such a substance in the universe, the asserters of it will probably bring for proofs several of the phænomena I am about to relate. But whether there be, or be not in the world any matter, that exactly answers to the descriptions they make of their first and second elements, I shall not here discuss; though divers experiments seem to argue, that there is in the world an æthereal substance very subtle and not a little diffused. But though these things seem, as I was saying, probable enough; yet the invisible agents, I shall here chiefly, though not only, take notice of, will be the air (as it hath a weight and spring) and the magnetical effluvia of the terrestrial globe.

IF you take a bar of iron, or rather of steel, and another like it of silver, and having heated each of them red hot, and put them to cool directly north and south; though they be both acted upon by the same agent, the fire, and the steel, as to sense, seems such as it was before, yet the texture of these two metals being different, the silver acquires no new quality by what hath been done to it, whereas the ignition of the steel having opened its pores, and made its parts more pliable (as may be argued from the swelling of iron heated red hot, and its softness under the hammer) it is easily, whilst in this state it lies north and south, pervaded by the magnetical effluvia of the earth, which glide perpetually through the air from one pole to another, and by the passage of these steams it becomes endowed with a magnetical property, which some call polarity, whereby being freely suspended and exactly poised, it will, as it were, spontaneously direct itself towards the north and south, and exercise some operations peculiar to magnetical bodies. And that it may seem the less strange, that I should ascribe to so gross and dull a body, as the earth, the power of invisible communicating to iron a magnetical virtue, which is thought to be of so spiritual a nature; I shall put you in mind of an experiment, that I acquainted you with divers years ago, about the earth's power to impart, in some cases, without the help of a loadstone, a directive faculty to the loadstone itself. For, having by ignition deprived an oblong magnet of its former attractive power, by taking it red hot out of the fire, and suffering it to cool north and south, I could at pleasure, by placing either end northward or southward, whilst the stone was refrigerating, make what end I had a mind to, point to the north pole; and when it had done so, I could, by a new ignition and refrigerating of it in a contrary position, make the same end of the stone become its southern pole.

IF you take a capacious glass vial with a slender neck, ending in a sharp angle, with only a pin-hole left open at the apex, (instead of which vessel, *Hero*'s egg, as some call it, though far smaller, and without such a neck may serve turn) and by suction or otherwise free it from as much of the included air as you can; and if then having stopped this hole with your finger, you immerse it somewhat deep under water, and, lastly, withdraw your finger; the water will, contrary to its own nature (as is vulgarly conceived) spring up with violence, and to a good height into the cavity of the vial; which motion of a heavy liquor upwards cannot be ascribed to the motion of the finger; for that did but unstop the orifice, and not impel up the water; nor need be attributed to nature's abhorrence of a vacuum, which (whether there be such a thing or not) it is altogether unnecessary to have recourse to in this case; the pressure of the ambient air, proceeding from its weight upon the surface of the water, being sufficient to force up that liquor into the vial, in which the remaining air, by being rarefied, upon the score of the absence of that which was taken out, hath its spring too much weakened to be able to resist the pressure of the outward air, as it formerly could do; whereas, if this experiment were tried in vacuo, the water would not be raised, there being no outward agent to impel it up.

CHAP. V.

I HAD sometimes the curiosity to consider beans and pease pulled up out of the ground by the stalks, in order to an inquire into their germination; and after having taken notice of their tumidness upon their having imbibed the moisture of the soil, and of their way through the ambient earth not only upwards with their stems, but downwards with their tender roots; I thought fit to try, with what strength and force the causes of their intumescence endeavoured to dilate them. Whereupon I filled with a quantity of such dry beans, as are in *England* wont to be given to horses, several vials and bottles, some of glass, and some of earth, whereof two or three were of a considerable strength: which done, the intervals between the beans were filled with water, and the vessels were exactly stopped with corks, strongly tied down with strings, that nothing might get out; for I supposed, that the water soaking into the pores of the beans, would alter the figure of the pores, and produce in them an endeavour to ſwell; which being checked by the ſides and ſtopples of the veſſels would diſcover, whether that endeavour were ſo forceable as I ſuſpected. The ſucceſs was, that moſt of theſe veſſels (for in one or two of them we found the ſtrings broke, that withſtood the raiſing of the ſtopples) whether of glaſs or earth, were burſt in ſunder.

BUT being deſirous to make a nearer eſtimate, how great this expanſive force of the ſwelling beans was, we put a convenient quantity of them into an hollow, but ſtrong cylinder of braſs, which I had cauſed to be purpoſedly made for ſuch kind of trials, whoſe cylindrical cavity was juſt ſix inches in length, and two in diameter: then having put in water enough to reach the top of the beans, we put into the upper part of the cylinder, which was purpoſedly left unfilled, a wooden plug, made fit for the orifice, by being turned into a cylindrical form, and a little narrower than the orifice, that it might move freely up and down, though the water ſhould make it ſomewhat ſwell. Upon the top of this plug, on which leaned a broad and thick piece of wood, ſhaped like a round trencher, and made of the ſame piece with the plug, was placed a common half hundred weight of lead, which yet could not depreſs the plug too low, being hindered by the breadth of the trencher, made as well to prevent the too great depreſſion of the plug, as to afford a convenient baſis to the weight. Laſtly, having kept the cylinder in a quiet place for a fit ſpace of time (which is in ſuch trials ſometimes two or three days, ſometimes more or leſs according to the temperature of the air, and quantity of the included matter,) we obſerved, as I expected, that the ſwelling beans had very manifeſtly heaved up the plug, and the incumbent weight beyond the former ſtation. And I ſuſpected, that if we had had ſmall weights (of a pound or two a piece) conveniently ſhaped, a heavier weight might have been raiſed by the ſame force.

IT is not neceſſary in this place, that I mention ſeveral particulars relating to the experiment, as how it ſucceeds in corn ground and unground, how in dried fruits, as raiſins and currants, how in dried peaſe, (which we found to dilate themſelves very ſtrongly) and what liquors will or will not cauſe an intumeſcence; nor ſhall I here ſpeak of divers circumſtances, that may be taken notice of in ſuch trials: only I muſt not omit this particular, that I had a mind to make ſome trial, whether the force of ſwelling beans, to preſs or thruſt up the incumbent weight, would not in cylinders of different ſizes be increaſed in ſomewhat near a duplicate proportion to that of the diameters, or the areas of the orifices of the differing cylinders (becauſe it is according to the greatneſs of thoſe areas, that the force can be applied upwards;) but having not weights enough ſo ſhaped as I needed, I could not make ſuch an experiment as I deſired; but thus much however I diſcovered in order to my purpoſe, that the preſſure upwards of the drenched beans was very much greater in wider cylindrical veſſels than in narrower ones: for having put a convenient quantity of dried beans into a metalline cylinder, that wanted a pretty deal of being ſo deep as ſix inches, and was not quite four inches broad; when the included beans began to ſwell, they manifeſtly lifted up ſuch a plug as was lately deſcribed (but broader) with weights upon it, amounting to an hundred pounds or better.

WHETHER this may paſs for a new phyſical *vis movens*, I freely leave to you to determine; as alſo to conſider, whether by mechanical contrivances, ſo great a force, as may be this way produced, and which ſlowly and ſilently proceeds, till it hath attained its utmoſt energy, and may be conveyed into bodies without working any effect before the due time, may not in ſome caſes be made applicable to uſeful purpoſes.

I ſhall not now examine, whether or how far the foregoing experiment may confirm the Carteſian hypotheſis about their *materia ſubtilis*; nor whether upon the notions, which our experiments may ſuggeſt, we may be enabled to explicate the force, wherewith fermenting liquors do often break the veſſels, wherein they are too exactly ſhut up; about which phænomena, and of ſome others of kin to it, I elſewhere propoſe ſome conjectures.

I think it fitter in this place to take notice to you of ſomething, that more directly belongs to our preſent ſubject; namely, that the air, within which name I here compriſe the æther, that may be harboured in its pores, may in ſome caſes, by its conſtant preſence, and in others, by its being always at hand, and its readineſs to inſinuate itſelf wherever it can get admittance, concur to the production of divers phænomena, wherein its co-operation has not been ſuſpected even by philoſophers: for, not to mention what I have by experiments purpoſely deviſed, that the air's being preſent to preſs upon the ſuperficies of liquors is ſo requiſite in ſuction, that they will not thereby be made to aſcend without it; and beſides that to the putrefying of ſome bodies within the time (or even within ten times the time) that nature is wont to putrefy them in, they will not be brought to putrefaction, if the air be all the while carefully ſecluded: beſides theſe things, I ſay, I found, [that the light, which appears in ſome rotten woods, and in ſome putrefied fiſhes, did ſo much depend upon the preſence of the air, that if that were quite withdrawn from them, the light would diſappear, and when they were reſtored to the contact of the air, they would ſhine forth again as formerly. But of this elſewhere.]

CHAP. VI.

I KNOW not, whether it will be fit to add, that beſides what the air (with the ſubtler matter that may be mingled with it) may do as a ſubſtance; it may perform divers things upon other accounts, as its finer parts may be, though inſenſibly, moved in phyſical ſtrait lines; or as it is the ſubject of ſwarms of corpuſcles

corpuſcles put into peculiar, though inviſible, motions. For inſtance, if I take a ſheet of paper, and rub it over with oil, or even a fit kind of greaſe; that, which the liquor apparently does, is only to pierce or ſoak into the pores of the paper, which before did by their crookedneſs, or upon ſome other mechanical account render the paper opacous. But this inſinuation of the unctuous body into the pores having altered them as to figure, or to ſize, or to both, and having by that alteration given the paper a texture diſpoſed to allow due paſſage to the corpuſcles of light, or to tranſmit their peculiar kind of impulſe (whence ſeveral naturaliſts derive light) the motions, as I was ſaying, or inviſible corpuſcles in the air, depending upon the conſtitution of the world, do preſently act upon the paper, and produce beyond it both a ſenſation of light, and the repreſentations of a multitude of objects, whence the light reflects, and which could not be ſeen through it before.

I need not perhaps tell you, that if a pretty large box be ſo contrived, that there may be towards the one end of it a fine ſheet of paper, ſtretched like the leather of a drum-head, at a convenient diſtance from the remoter end, where there is to be left an hole covered with a lenticular glaſs fitted for the purpoſe, you may at a little hole, left at the upper part of the box, ſee upon the paper ſuch a lively repreſentation, not only of the motions, but ſhapes and colours of outward objects, as did not a little delight me, when I firſt cauſed this portable darkned room, if I may ſo call it, to be made. Which inſtrument I ſhall not here more particularly deſcribe, partly becauſe I ſhewed it you ſeveral years ago, ſince when diverſe ingenious men have tried to imitate mine (which you know was to be drawn out or ſhortned like a teleſcope, as occaſion required) or improve the practice; and partly, becauſe that, which I pretendedin mentioning of it here is, to ſhew, that ſince that almoſt upon every turning of the inſtrument this way or that way, whether it be in the town or open fields, one may diſcover new objects, and ſometimes new landſcapes upon the paper, there muſt be all day long in all parts of the air, where this phænomenon can be exhibited, either certain effluvia emitted every way from the objects, or certain motions of inſenſible corpuſcles, which rebounding firſt from the external object, and then from the paper, produce in the eye the images of theſe objects: ſo that the air is every where full of viſible ſpecies, which cannot be intelligibly explicated without the local motions of ſome minute corpuſcles, which, whilſt the air is enlightened, are always paſſing thorough it.

You may remember, *Pyrophilus*, that in the clauſe of the ſecond propoſition, hitherto diſcourſed of, I take in the eſtabliſhed laws of the univerſe, as a part of the preſent conſtitution of this our world; ſome of thoſe laws contributing much to the operation of thoſe unheeded cauſes, we are treating of. Of theſe I may another time give you ſome inſtances; but for the preſent it may ſuffice to take notice of this one, that if you take a bar of iron, and holding it perpendicularly, apply the loweſt part of it to the northern point of a well-poiſed magnetical needle, the bar will preſently drive it away: but that magnetiſm, by which the bar does it, as it is preſently acquired by the poſture which it had, ſo it is as ſuddenly changed, if you invert that poſture; as appears by this, that though you hold the bar perpendicular, if it be held under the needle, ſo that the ſame part of the bar, which before was placed directly over the north point of the needle, be held directly under the ſame point, the bar will not, as before, drive it away, but, as they commonly ſpeak, attract it. But if this bar have been for a long time kept in an erected poſture, as if it be taken from ſome old window, or if, having been heated and refrigerated, it have very long lain north and ſouth, it will appear endowed with a ſtronger and more durable verticity, as we elſewhere more fully declare; which ſeems to proceed from this, that by lying north and ſouth, it lay in the way, which, according to the eſtabliſhed laws of nature, the magnetical effluvia of the earth muſt paſs along in ſteams from pole to pole; whereby they have the opportunity by little and little to work upon the pores of the iron, that lies in their way, and fit them to give paſſage to the effluvia of magnetical bodies; in which fitneſs ſeems principally to conſiſt the magnetiſm of iron: whereas, if this metal had all this while lain eaſt and weſt, inſtead of north and ſouth, it would have acquired little or no magnetical virtue. And the reaſon, why an erected poſture gives a rod or bar of iron a power to drive away the north point of the needle, has been probably conceived to be this, that the lower end being nearer the earth does more plentifully participate of the magnetick ſteams, which fly in a cloſer order there, than further off, and by powerfully affecting that part of the iron, turn it, for a time, into the iron's north pole, which according to the laws magnetical ought to drive away the north pole of the needle, and attract the ſouth; whereas, if the bar being inverted, that end, which was uppermoſt, becoming the lower, muſt for the ſame reaſon have a contrary operation, unleſs by having long ſtood, its verticity be too well ſettled to be ſuddenly deſtroyed or altered by the effluvia of ſo languid a magnet as the earth. But whether or no this explication be the right one (for I would not contend for its being ſo) it appears, by the requiſiteneſs both of a determinate poſition of the iron, and of its long continuance in that poſition, to make that metal acquire a durable verticity, that thoſe unheeded magnetical ſteams, which communicate ſuch a magnetiſm to the iron, move and act according to laws eſtabliſhed in nature: which is as much as my deſign in this diſcourſe makes neceſſary to be made out.

CHAP. VII.

IT remains now, that we diſcourſe of the laſt of our three grand propoſitions, namely, "That a body by a mechanical change of "texture may acquire or loſe a fitneſs to be "wrought upon by ſuch unheeded agents, "and

"and alſo to diverſify their operations on it "upon the ſcore of its varying texture."

THIS propoſition is of ſo much affinity with the foregoing, that there are divers caſes, wherein the ſame experiments and other arguments may ſerve for the confirmation of both.

BUT to illuſtrate a little what I mean, by groſs and ſenſible examples, it is a cuſtom we often obſerve at ſea, when we ſail with to ſlack a wind, to take up water with certain inſtruments, and throw it againſt the ſails. At the firſt propoſal, this may ſeem a very improper way to promote the ſwiftneſs of the ſhip, ſince there is the weight of ſo much water added to that of the veſſel itſelf; but yet I have ſeen the ſeamen make uſe of it as one of their beſt expedients, when we were cloſely chaſed by pirates, nor did I look upon it as irrational; for whereas, when the ſails are dry, a good part of the wind, that blows upon them, eaſily gets thorough thoſe meſhes or great pores, that are left between the threads of which a ſail conſiſts; when it comes to be wetted, the imbibed water makes the threads ſwell every way, and conſequently very much ſtreightens the pores or intervals, that were formerly left between them; by which means the wind cannot permeate them as freely as formerly, but by finding a greater reſiſtance in the ſail comes to beat more forcibly upon it, and conſequently drives it, and with it the ſhip, more ſtrongly on, than elſe it would have done: not to mention the ſtiffneſs of the ſail acquired by the imbibed ſea-water, becauſe I would not ſtay to take notice of other particulars, to which the ſucceſs of this practice may perhaps be in part aſcribed.

TO add another inſtance to the ſame purpoſe with the former; ſuppoſe an high wind to blow againſt a chamber, wherein the windows and doors are all ſhut, the effect will be only, to ſhake a little the room in general: but if one open the caſement, though he, that does it, do properly and immediately but diſplace ſome little piece of the iron, or other thing, that ſhuts the window, yet this being done in a place, where there is a ſtrong current of air, which we call a wind, there will preſently follow a blowing up of curtains or hangings, and blowing about of duſt, ſtraws, feathers, or other light bodies, that are not firmly enough faſtened, nor very ponderous, and yet are too heavy to be blown about.

BUT to proceed to inſtances, that are not ſo groſs, I might take notice, that though good common tartar does uſually of it ſelf keep dry in the air, nay, and will not eaſily be diſſolved in cold water; yet if it be calcined, though but very moderately, the ſalt in the remaining coal, the texture being now altered, will readily enough in the moiſt air (as that of a cellar) run into that liquor, that chemiſts have been pleaſed to call oil of tartar per diliquium. But in regard that to make the change the greater, part of the tartar muſt be driven away by the fire, I ſhall rather make uſe of an example eaſily drawn from an experiment, I elſewhere mentioned to another purpoſe; for having taken a loadſtone, and according to the way there delivered, heated it and cooled it, though it had loſt ſo little by the fire, that the eye took no notice of its being changed either as to ſhape or bulk, yet the operation of the fire, by changing the inviſible texture, did ſo diverſly alter the diſpoſition of it in reference to the magnetical effluvia of the earth, that I could preſently and at pleaſure change and re-alter the poles of the ſtone, making the ſame end point ſometimes to the north and ſometimes to the ſouth. The like change of verticity I have, as I elſewhere declare, made mere iron capable of, without the help of fire or any other magnet than the earth; and I have alſo found by trial, that a certain heavy ſtone, that is uſually thought to be not ſo much as of a metalline nature, may by a ſlight and quick preparation, that alters not the ſhape nor bigneſs, be enabled to attract and repel the poles of a magnetic needle.

CHAP. VIII.

TO the inſtances already given in ſolid bodies it will not be amiſs to annex two or three in liquid bodies, becauſe it may be thought ſtrange by ſome, that conſiderable changes of texture ſhould, without fire or any new ingredients, be produced in bodies, which, by reaſon of their fluidity, ſeem preſently to recover their texture if it be diſordered. If honey and water be each of them apart put into a convenient veſſel, they will both of them retain their nature; and though you mix them together in an undue proportion, ſo that by reaſon of overmuch honey the conſiſtence be too thick, or that by being diluted by too great a proportion of water, the ſolution of honey be too thin, they may continue honey and water: but if thoſe two liquors be duely proportioned (as if you put to one part of honey four or five of water) then their new texture ſo diſpoſes them to be acted on by the ſubtle permeating matter, or whatever other common agent nature imploys to produce fermentations, that the ingredients do no longer continue what they were, but begin to work like new muſt, or beer-wort: and I have tried, that ſo ſmall and ſhort a local motion (as carrying ſuch mixtures a while in a coach) has ſo excited the liquor, as to make it violently force its way out of the veſſel, or throw off the ſtopple, that I have wondered at it. And I remember, that an eminent merchant of wines, who ſpent divers years in the *Canaries*, being aſked by me about ſome things of this nature, aſſured me, that in thoſe fortunate iſlands (as the ancients ſtyle them) he had ſeveral times obſerved, that if a pipe of the beſt ſort of Canary were, when it was about a month old, rudely rolled, though but the length of an hall or moderate gallery, ſo tranſient and ſlight a diſcompoſure of the texture would quickly make ſo great a change in it, that oftentimes a good quantity of wine would be violently thrown out at the bung; or if the pipe were too cloſe ſtopped, that great veſſel it ſelf would oftentimes have the bottom beaten out; by which means he had known ſeveral pipes of that rich liquor loſt.

We have divers examples of the cracking of common glaſs, when it is too ſoon, after it hath been removed from the fire, expoſed to the cold air, and the ſubtle bodies that are in it; which would not have cracked it, if it had been cooled more ſlowly, ſo that its parts would have had leiſure to ſettle into a texture convenient for the paſſage of thoſe ſubtle bodies, which in that caſe would harmleſly have permeated it. But I have ſometimes ſhewn the curious a more quick and manifeſt inſtance of the importance of the preſent texture of a body in reference to the catholick and inviſible cauſes, that may work upon it. For having taken a plate of ſo ponderous and ſolid a body as copper, and heated it red hot, and then ſuffered it to cool a while upon ſome more moderately hot place in the fire, though it did not appear at all ignited, when I removed it to a plate, or even to a ſheet of paper; yet upon its being expoſed to the atmoſphere, the ſuperficial part would not only crack as in over haſtily cooled glaſs, but would, and that preſently, fly off in flakes in good number, and not without noiſe; ſo that in a ſhort time I have had the neighbouring part of the paper, on which the braſs plate reſted, almoſt quite covered with little ſcales, as it were, of that metal.

And to give you, in favour of what I have been hitherto diſcourſing, an inſtance of a very ſubtle nature, I will not, though I juſtly might, take notice, that in rotten fiſh and rotten wood, the change of texture is oftentimes inviſible, that will ſuffice to make the contact of the air, and the ſubtle corpuſcles, whereto it gives harbour or paſſage, confer or loſe a power of ſhining: but I will rather chuſe to inſtance in the Bolonian ſtone, which by calcination acquires this admired property, that if it be but expoſed to the ſun beams (to which I have found other ſtrong lights ſuccedaneous) it will not only in a few minutes acquire a luminouſneſs, but for ſome time after retain it in the dark.

COSMICAL SUSPICIONS

(SUBJOINED AS AN APPENDIX To the DISCOURSE of The COSMICAL QUALITIES of THINGS.)

In the former eſſay, *Pyrophilus*, I propoſed to you ſome things about the ſubject there treated of, that ſeemed to have in them ſuch a degree of probability, as is wont to be thought ſufficient to phyſical diſcourſes, or at leaſt is uſually to be met with in them. But in regard the world, whether we take it in the larger ſenſe for the whole univerſe, or, in the more narrow but not leſs common acception, for the globe we men inhabit, is a ſubject ſo vaſt, that not only all demonſtrable truths, that may be diſcovered concerning it, may be looked upon as important, but even conjectures and ſuſpitions themſelves, that relate to it in general, if they be not very groundleſs or extravagant, may deſerve not to be altogether paſſed by in ſilence. I will adventure to entertain you a while with ſome thoughts of this nature, eſpecially becauſe they will give me opportunity to alledge in their favour ſome hiſtorical obſervations, which, whatever the doubts or conjectures be thought of, may appear to be more new then deſpicable.

It may now therefore be not unſeaſonable to confeſs to you, that I have had ſome faint ſuſpicion, that beſides thoſe more numerous and uniform ſorts of minute particles, that are by ſome of the new philoſophers thought to compoſe the æther I lately diſcourſed of; there may poſſibly be ſome other kind of corpuſcles fitted to have conſiderable operations, when they find congruous bodies to be wrought on by them. But though it is poſſible, and perhaps probable, that the effects we are conſidering, may be plauſibly explicated by the æther, as it is already underſtood; yet I ſomewhat ſuſpect, that thoſe effects may not be due ſolely to the cauſes they are aſcribed to, but that there may be, as I was beginning to ſay, peculiar ſorts of corpuſcles, that have yet no diſtinct name, which may diſcover peculiar faculties, and ways of working, when they meet with bodies of ſuch a texture, as diſpoſes them to admit, or to concur with the efficacy of theſe unknown agents.

This ſuſpicion of mine will ſeem the leſs improbable, if you conſider, that though in the æther of the ancients there was nothing taken notice of but a diffuſed and very ſubtle ſubſtance; yet we are at preſent content to allow, that there is always in the air a ſwarm of ſteams moving in a determinate courſe betwixt the north pole and the ſouth; which ſubſtance we should not probably have dreamed of, if our inquisitive *Gilbert* had not happily found out the magnetism of the terrestrial globe. And few, perhaps, would have imagined, that when an hunted and wounded deer has hastily passed over a little grass, he should leave upon it such determinate, though invisible, effluviums, as should, for many hours, so impregnate the air, as to betray the individual flying and unseen deer; if there were no blood-hounds, upon whose peculiarly disposed organs of smelling these steams are fit to operate. And it is strange, that there should be such effluvia for a long time (perhaps a year or two together) residing in the air, that though our senses discern them not, and though they have no operation upon other men; yet if they meet with persons of a peculiar temperament, who by that, and by their formerly having had the plague, have attained a peculiar disposition, that fits them to be wrought on by pestilential steams, they may so operate upon them, that some of these persons may be able to discern those steams to be pestilential. To give some countenance to which paradox, I will here annex two or three testimonies, the first of which I find thus set down among my Adversaria. [Above three months before the late great plague began in *London*, in the year 1665, there came to Dr. *M.* a patient of his, to desire his advice for her husband; and the doctor having enquired what ailed him, she answered, that his chief distemper was a swelling in his groin, and upon that occasion added, that her husband assured her of his being confident, that the next summer the plague would be very rife in *London*; for which prediction he gave this reason, that in the last great plague he fell sick of that disease; and he then had a pestilential tumour.

So in two other plagues, that since happened, though much inferior to that great one, each of them had a rising in his body to be its forerunner, and now having a great tumour in the forementioned place, he doubted not but it would be followed by a raging pestilence, which accordingly ensued. Having heard much talk of something of this nature, and being this morning casually visited by the doctor, a person of great veracity, I enquired of him, how much of it was true, and I received for answer the foregoing narrative.]

THE second is a very remarkable story, which I remember that famous and excellent chirurgeon *Fabricius Hildanus* records of himself; namely, that having had a pestilential tumour during a plague, that happened in his youth, if for many years after he chanced to go to, or so much as to pass by, an house infected with the plague, he was admonished of the particular disease, that reigned there, by a sensible pain in that part, where he had had a pestilential tumour so long before.

THE third testimony is afforded me by that curious observer of the changes, that happened as to the phænomena of diseases at the famous siege of *Breda*, where this diligent physician, practising much among patients afflicted with malignant and pestilential diseases, was at length infected himself; whereupon he informs his readers; *Annotandum hic merito naturæ facultatem ad pestis præservationem momenti esse maximi. Observavi in meipso contaminatos invisente statim inguen dolore vel axillas: afficiebatur aliquando caput, noctu inde sudor, & secessus tres quatuorve. Hoc & aliis accidit, qui fideliter mihi retulerunt.*

IF these stories were related by ordinary persons of what happened to other men, the oddness of them might well tempt a wary man to suspend his judgment: but the judiciousness of the writers, and the profession they were of, and their relating these as things, that did more than a few times happen to themselves, may well be permitted to bring credit to their assertions. And these instances added to what has been already said, may, I hope, excuse me, if I think it not time misspent, to consider, whether there may not be other, and even unobserved sorts of effluvia in the air: to excite your curiosity and attention about which, rather than to declare a positive opinion, is that which is pretended to in what has been lately mentioned.

AND whereas, *Pyrophilus*, I have in the former discourse taken in the structure and established laws of the universe, as an help toward the giving an account of the cosmical attributes of things; I shall here also ingeniously confess to you, that I much fear, whether we have yet attentively enough taken notice either of the number, or the kinds of those laws.

FOR as I am by some notions and obsevations inclined to think, that there may be a greater number even of the more general laws, than have been yet distinctly enumerated; so I think, that when we speak of the established laws of nature in the popular sense of that phrase, they may be justly and commodiously enough distinguished; some of them being general rules, that have a very great reach, and are of greater affinity to laws more properly so called, and others seeming not so much to be general rules or laws, as the customs of nature in this, or that particular part of the world; of which there may be a greater number, and those may have a greater influence on many phænomena of nature, than we are wont to imagine.

AND first, whereas the structure of the world is a main help in our present disquisition; I shall venture to tell you, that though I do not only commend, but in divers cases admire, the industry of astronomers and geopraphers, especially of some later ones; yet they have not met with such difficulties, that they have hitherto presented us, rather a mathematical hypothesis of the universe, than a physical, having been careful to shew us the magnitudes, situations, and motions of the great globes, such as the fixed stars and the planets (under which one may comprise the earth) without being sollicitous to declare what simpler bodies, and what compounded ones, the terrestrial globe we inhabit does, or may consist of. And as of late years the discovery of the four planets about Jupiter, and the little moon (as some call it) that moves about Saturn, together with the phæno-

phænomena of comets, have obliged the skilful to alter divers things in the theory of celestial bodies: so I know not, but that future discoveries by improved telescopes and other philosophical instruments may reduce us to make changes in the grand system of the universe itself, and in that which we consider as the most important of the mundane bodies to us, the terraqueous globe we live on.

WHAT communication this may have with the other globes we call stars, and with the interstellar parts of heaven, we have very little knowledge of, though I may elsewhere make it probable, that there may be some commerce or other: but without speaking more particularly of that point, I confess I have some time suspected, that there may be in the terrestrial globe itself, and the ambient atmosphere, divers, whether laws or customs of nature, that belong to this orb, and may be denominated from it, and seemed to have been either unknown to, or overseen by both scholastical and mathematical writers. And first, I have often suspected, whether there may not be in the mass of the earth some great, though slow internal change (whether originated there, or produced by the help of other mundane globes) by considering, that almost in all countries, where observations have been made, there has been a plain and considerable alteration found in that, which is commonly called the variation (for it is rather the declination) of the sea compass or magnetick needle, which is the distance, by which the needle declines east or west from the true north pole. And whereas formerly, at or * near *London* the compass declined, as observations solemnly made, and upon record assure us, in the year 1580, above 11 degrees; in the year 1612, above 6 degrees; in the year 1633, no less than about 4 degrees; it has of late been found to have very little or no variation. And at a place within half a league of *London*, trying with a long and curious needle purposely made and poised, I could scarce discern any declination at all, and if the needle declined sensibly any way from the pole, it seemed to do so a little towards the other side of heaven, than that towards which it did decline before. And having † afterwards by the help of a meridian line, much prized for having been accurately drawn by eminent artificers, made an observation in *London* itself; though I made it with two instruments, whereof one was a choice one, differing from the former, and from one another; I could not satisfy myself, that I could discern the declination of the needle to exceed half a degree, if it amounted to so much. But since observations of this kind may prove more considerable than we yet know of, and since they ought to be made at distant places, I am contented to add here, by way of confirmation, that the *Cape of Good Hope* being one of the eminentest parts of the terrestrial globe in reference to magnetisms, the acquaintance I had with one of the ancientest and most experienced navigators of this part of *Europe*, invited me to address myself to him, purposely to enquire of him, whether he had taken the variation of the compass at the *Cape of Good Hope*; and whether, if at all, he had taken it more than once: he answered, that he had often done it: whereupon asking him what he found the variation to be, and whether he had observed any change of it in his several voyages, he replied, that when he was a young seaman, he observed the variation to be about two degrees westward, and afterwards during many years that he sailed to and fro betwixt *East-India* and *Europe*, he found the variation to encrease by degrees; and whereas he had learned from ancient writings, and the tradition of old seamen, that before his time, they had found no variation at all, he about 15 years ago (which was the last time he took it) found it by accurate instruments, to be 6 degrees and about 48 minutes. So that during the time, that he practised the seas about the *Cape of Good Hope*, the variation still westward had decreased near 5 degrees. Upon these grounds, which I may elsewhere have occasion to confirm by further observations, I cannot but think it probable, that there may be agents, that we know not of, that have a power to give the internal parts of the terrestrial globe itself a motion; of which we cannot yet certainly tell, according to what laws it is regulated, or so much as whether it be constantly regulated by certain laws or no. And what other changes, agents that can produce a change in the terrestrial globe itself may make in this, or that part of it, who can inform us?

IN the next place, I consider the great uncertainty and irregularity, that we have hitherto observed in the weight of the atmosphere by our new statical barometers, and much more sensibly by mercurial ones, without yet having discovered the causes of such considerable alterations in the air, (save that in general they proceed for the most part from subterraneal steams) whose influences upon other things may be more considerable, than we have yet had opportunity to detect.

IT is very remarkable what a late and ingenious writer, that lived in some of the American islands, relates about the hurricanes in those parts; namely, that before the Europeans came thither, the inhabitants observed, that they had those fatal tempests once in seven years and no oftener; afterwards, they were troubled with them but once in six years; and in process of time, the unwelcome visits of those winds grew so frequent, that in my relator's time they came once a year; and, as a prodigy, they once observed two in one year; and afterwards three in another. I remember also, that meeting with an inquisitive gentleman, that had lived in *New-England*, I desired to know of him, whether in that part of the country, where he resided, there were not a great change made in the very temperature of the climate? whereto he answered me, that there was, for it was grown much milder than formerly: and because I doubted, whether this change might not have been, either accidental for a year or two, or apparently to the English, whose bodies

Monsieur de Rochefort.

* At *Limehouse*.

† A. D. 1669.

dies by degrees might grow more accustomed to the coldness of the country, and less sensible of it; it was answered, that this change had been observed for many years after the English had planted a colony there, and that the change was manifestly perceived by the natives too, by the remisser operation of the cold upon running and standing waters, which were formerly wont to be frozen at such and such times. And I shall add for confirmation, that having one day the honour to be standing by his majesty, when he received a solemn address from *New-England*, delivered by the governor of a colony there; that very inquisitive monarch, amongst other questions, asking him about the temperature of the air, he told his majesty, in the presence of divers that came from *America* with him, "that the climate had much "altered and lost much of its former coldness "for divers years, since the English settled "there."

WHETHER this decrement of the sharpness of the air will proceed, or how long it will continue, time will discover. But in the mean while, supposing with him the matter of fact to be true, and that the change depends not on any manifest cause; that, which is happened already, seems to me very considerable, since I have lighted on a book * written by † one of the ancient planters of *New-England*, by way of description of that country; where, among other things, I find those notable passages. The one in the seventh page: in former times, says he, the rain came seldom, but very violently, continuing its drops, which were great and many, sometimes 24 hours togethers, sometimes 48, which watered the ground for a long time after: but of late the seasons are much altered, the rain coming oftner, but more moderately, with less thunder and lightnings, and sudden gusts of wind. And the other in the 84th page; where speaking of the heathen natives, he says, they acknowledge the power of the Englishman's God, as they call him, because they could never yet have power by their conjurations to damnify the English either in body or goods: and besides they say, he is a good God, that sends them so many good things, so much good corn, so many good cattle, temperate rains, fair seasons, which they likewise are the better for since the arrival of the English; the times and seasons being much altered in seven or eight years, free from lightning and thunder, long droughts, sudden and tempestuous dashes of rain, and lamentable cold winds.

So that by this it appears, that this grateful decrement of the coldness and rudeness of that climate was already taken notice of so || many years ago.

To these relations may pertinently be subjoined a passage of the learned *Magnenus* in his ingenious little tract *de Mannâ* ‡; where he very solemnly delivers this notable observation, that in the country he calls *Cenotria*, there was no manna to be found a little above three hundred years ago. And that in *Calabria* it self, a province so famous for manna, that the best is denominated thence, and that furnishes a great part of *Europe* with that odd drug, it is but since two ages, or thereabouts, that manna has fallen, or, as he expresses it, rained.

I know not whether it may be worth while to mention, after these more weighty observations, the œconomical traditions of house-wives; which I should not think worth taking notice of in this place, but that having purposely enquired after the truth of it, of two very sober persons (much versed in the art of making sweatmeats) that have, especially one of them, often tried it; they seriously affirmed to me, that they find the spots made in linnen by the juices of fruit, particularly of red currants in straining bags, will best wash out (nay scarce otherwise) at that time of the year, when those fruits are ripe the ensuing year.

To which may be for affinity's sake annexed, what is related by the ingenious French writer of the history *Des Isles Antilles***, where he lived divers years; who speaking of the fruit they there call *Acajou*, tells us, that the juice of some of the internal parts of it, though reputed an excellent remedy in fainting fits, is of such a nature, that if it chance to fall upon a piece of linnen, it turns to a red spot; which lasts till the tree come to be again in flower. Which phænomena, if the length of time, and the heat and temperature of the air, usual in the seasons of producing blossoms, and ripening of fruits, be found to have little or no interest in their causation, may prove of some use in our present enquiry.

WHATEVER be the true cause of the ebbing and flowing of the sea, yet at spring tides the motions of such vast masses of matter as the great ocean, and most of the seas, are so constantly coincident with the new and full moon; and the more stupendous spring-tides have been in most places, so long observed to happen regularly enough about the æquinoxes, that it is worth an enquiry, (though I cannot here afford it one) whether these conspicuous phænomena may not somewhat confirm the conjectures we are discoursing of.

AND when I remember, how many questions I have asked navigators about the luminousness of the sea; and how in some places the sea is wont to shine in the night as far as the eye can reach; at other times and places, only when the waves dash against the vessel, or the oars strike and cleave the water; how some seas shine often, and others have not been observed to shine; how in some places the sea has been taken notice

* Intitled, *New-England*'s prospect. † Mr. *W. Wood*. || The book was published thirty-five years since.

‡ Sanctiorum naturæ interpretum nullus fraxinum inter arbores gummiferas, aut resiniferas recensuit. Illud omnino, quo *Altomatus* sese jactare videtur, ignoravêre curiosissimi rerum indagatores, *Plinius*, *Galenus*, *Theophrastus*, & qui mediam ætatem impleverunt viri doctrinâ diligentiâque celebres: quia scilicet illis temporibus multum pluebat in *Calabria* manna, quod à duobus tantummodo seculis legi cœptum. Dic amabo, *Altomate*, cur ante trecentos annos multum manna fuit in *Cenotriâ*: jam certe aderant pagi ibidem urbesque vicinæ, neque vero fefellisset curiosam incolarum solertiam nihil plane video, quod pro te adduci possit ad hujus difficultatis evitandas angustias. *Magnenus de Manna*, *P. M.* 49.

** Histoire Naturelle des Isles Antilles. *Liv.* I. Chap. 6.

tice of, to shine when such and such winds blow, whereas in other seas, the observation holds not; and in the same tract of sea, within a narrow compass, one part of the water will be luminous, whilst the other shines not at all: when I say, I remember how many of these odd phænomena belonging to those great masses of liquor I have been told of by very credible eye-witnesses (whose narratives to me you may elsewhere meet with) I am tempted to suspect, that some cosmical law or custom of the terrestrial globe, or, at least, of the planetary vortex, may have a considerable agency in the production of these effects.

NOR am I sure, that some subterraneal changes, or some yet unobserved commerce between the earth and other mundane globes, has not an interest in the origin, continuance, and expiring of those diseases, that physicians call new, which invade whole countries (and sometimes greater portions of the earth) and last very many years, if not some ages, before they come to be extinct. Of which sorts of diseases divers learned men have reckoned up divers, and whereof the venereal pox, at least, as to its origine and spreading, is but too manifest and unhappy an instance; whereto, according to some eminent doctors, we may add the rickets, a disease, which though scarce known in other countries, is here in *England* so fatal to children, which first (as is affirmed) discovered itself among us within the memories of multitudes of men yet alive: but of this perhaps more elsewhere.

IF I should now further descend to the peculiar phænomena of particular regions, I must launch out into a discourse I could not have the leisure to finish. And therefore I shall only advertise you of two suspicions more, that I hold not unfit to intimate to you, about the established laws and customs of nature.

THE first of them is this, that I doubt those, that are thought the grand rules whereby things corporeal are transacted, and which suppose the constancy of the present fabrick of the world, and course of things, are not altogether so uniform complied with, as we are wont to presume; at least, as to the lines, according to which the great mundane bodies move, and the boundaries of their motions. For what reason the wise author of nature pleased to permit, that it should be sometimes, as it were, over-ruled by the boisterousness (if I may so call it) and exorbitant motions of unruly portions of matter, I must not in this place (though I do it in another) inquire: but when I consider the nature of brute matter, and the vastness of the bodies, that make up the world, the strange variety of those bodies, which the earth does comprize, and others of them may not absurdly be presumed to contain; and when I likewise consider the fluidity of that vast interstellar part of the world, wherein these globes swim; I cannot but suspect there may be less of accurateness, and of constant regularity, than we have been taught to believe, in the structure of the universe, and a greater obnoxiousness to deviations, than the schools, who were taught by their master *Aristotle* to be great admirers of the imaginary perfections of the cœlestial bodies, have allowed their disciples to think. And in effect, to speak only of the noblest of them, the sun, and to pass by about his motions the observation of the exactest astronomers, that natural days are not all of equal length (whatever the vulgar of philosophers suppose to the contrary;) and not to take notice of the great dispute betwixt the eminentest astronomers, even of our times, about the anomaly attributed to the motion of the sun's apogeum: to pass over these things, I say, the sun himself doth not only, from time to time, do what divers of our later astronomers stile to vomit our great quantities of opacous matter, (which are called his spots) some of them bigger, perhaps, than *Europe* or *Asia*, but has had almost his whole face so darkened with them, (as about the end of *Cæsar*'s, and the beginning of *Augustus*'s government) that for about a year together, he was, as it were, under an eclipse. To which, if we add those cœlestial comets, (for I dispute not now about sublunary ones) their number, vastness, duration, odd motions from orb to orb, (as the ancients would have spoken) and other phænomena, (whatever the causes of them be) it will appear, that even in the cœlestial part of the world, all is not so regular and unvariable, as men have been made to believe.

I had some doubts, whether this might not be much confirmed by what has been related by some navigators, that have been in the south-sea, about certain black clouds, said to move as regularly in the antartick hemisphere, as the neighbouring stars themselves; to which some of our English seamen (whether first or no, I know not) have added, certain white clouds in the same hemisphere move no less regularly. Of these relations, I say, I considered, whether some use might not be made to my present purpose; but having made the best inquiry I could, of those few persons of note I could meet with, that were likely to inform me, I do not yet see cause to alledge these phænomena by way of arguments. But yet since I find, that even pilots, who have been frequently in some parts of the *East-Indies*, have not (whether because they sailed not far enough to the southern pole, or upon some other score) taken notice of them; I shall subjoin as a part of natural history, not obvious to be met with, the best account I could procure of them; which was from an observing captain of an East-India ship, with which he lately adventured to unfrequented parts of the south-sea.

THE substance of his answers to me, about the fore-mentioned phænomena, was this, that he had divers times seen in the southern hemisphere, and in that part of the milky way, which is not to be seen upon our horizon (for he says, the galaxy is either compleatly, or almost a circle) two or three places, that look like clouds, and move about the earth regularly with the white part of the circle in 24 hours. But by what he replied to some further questions, that I asked him, I gathered, that if these be the black clouds, that navigators have spoken

of,

of, those, that gave them the name of clouds, were probably much mistaken; since, he answered me, that these are not black, but of a deep blue; which makes me suspect them to be but perforations, if I may so speak of the milky way, by which I mean parts of the azure-sky, that are suffered to be seen by the discontinuations of the parts of the galaxy. And to this account of the dark clouds, his further answers gave me this of the white ones; which, he says, some call the Magellanick clouds, about which he related:

THAT he had divers times seen towards the south-pole, the clouds, that some few navigators mention to be there, and to move about the pole in 24 hours.

THAT he began to discover them plainly, when he was in about 18 degrees, (as I remember) of south latitude.

THAT they were white, in number three (though two of them be not very distant from each other) the greatest being far from the south pole; the other not many degrees remoter than that star, which of the * conspicuous ones, they reckon to be nearest to the pole, though it be about eleven degrees distant from it.

BUT from this account of his I dare not, as I was intimating, conclude these to be such clouds as they are taken for, because, for ought I know, if they were looked on through a good telescope, they would be found constellations of small and singly inconspicuous stars, like those of the galaxy, the belt of Orion, &c. But to be resolved about these matters, it is not amiss to expect further observations; the proposed conjectures being made but upon a supposition of the truth and sufficiency of the relations.

AND thus much for the first of the two suspicions, that I above intimated, I would propose to you: the other is very different from it, and might seem contradictory to it, but that they belong not to the same cases. For though I lately told you, I suspected, that in some things, especially relating to the lines, according to which, and the limits within which some great masses of matter are supposed to perform their motions, there is more accurateness fancied than there really is; yet I shall now add, that there are cases, wherein I am not quite out of doubt, but that we may sometimes take such things for deviations and exorbitancies from the settled course of nature, as, if long and attentively enough observed, may be found to be but periodical phænomena, that have very long intervals between them. But because men have not skill and curiosity enough to observe them, nor longævity enough to be able to take notice of a competent number of them, they readily conclude them to be but accidental extravagancies, that spring not from any settled and durable causes. For the world, like a great animal producing some effects but at determinate seasons, as nature produces not beards in men, till they have attained such an age, and the menses (as they call them,) use not to happen to women before they come to such years, nor to last beyond such other years of their life; as may be also observed within a far shorter compass of time in the growth and falling of stags horns and bucks: if the first man had lived but one year in the world, he would perhaps have thought the blossoming of trees in spring, and their bearing fruit in summer, but an accidental thing, and would have looked upon the eclipse of the sun, as a prodigy of nature; observing, that though every new moon, the sun and she came very near together, yet neither before nor after was there any such terrible phænomena consequent thereupon. And we ourselves may easily remember, what strange conjectures we had of the strangely varying appearances of Saturn, for divers years after our telescopes first discovered them to us.

BUT most remarkable is that cœlestial phænomenon afforded us by the emerging, disappearing, and re-appearing stars of this age; which have been observed in the girdle of *Andromeda*, and in or about the swan's breast; (which is said to have been seen in the year 1600, and to have vanished in 1621.) and especially that, which having about 25 years ago appeared for a while in the whale's neck among the fixed ones, and afterwards by degrees disappeared, was looked upon by those astronomers of that time, who did not out-live it, as a cœlestial comet. * But afterwards an ingenious English gentleman of my acquaintance having observed here (as well as the vigilant curiosity of some few later astronomers hath taken notice of elsewhere) the return of the like phænomenon in the same part of heaven; it begat much wonder in all (which was encreased by the slow disappearing of it) and in some curious men a resolution to have a watchful eye upon that part of the sky. Since when the justly famous *Bullialdus*, and besides some eminent foreign virtuosi (whose names I know not) divers excellent persons of our own nation having taken notice of it in the wonted place, (where I had sometimes the satisfaction of seeing it;) these observations, and especially the last disappearance of a star judged to have been placed among the fixed ones, and estimated to be of the fourth (if not the third) magnitude, have somewhat confirmed me in the suspicion I am now treating of. For if this and the other new stars do continue to return periodically to the same part of heaven, where they have been already long ago seen; as at least for as much as concerns this, its gradual increasing after it first begins to shew it self, and decreasing afterwards, seem to promise; then I may with somewhat more of probability than before, suspect, that there may be vortices beyond the concave surface of what we call the firmament; which suspicion, if true, would much disfavour the hypothesis we now have about the system of the world, and will favour what I conjectured as possible about periodical phænomena. And however; if either the new star, without departing from its place, be only

* This wary expression keeps my relator from being contradicted by a curious modern astronomer, who tells of a star not three degrees distant from the southern pole; but then he says too, that it is a star of but the fifth magnitude.

only ſometimes by degrees overſpread and hid by ſpots, like thoſe I formerly mentioned to have obſcured the ſun, which are afterwards by degrees diſſipated, as I at firſt ſuſpected; or if it have a dark hemiſphere as well as a light one, (or rather a greater part of its globe obſcure than luminous, as *Bullialdus* ingeniouſly conjectures) and by turning ſlowly about its own center and axis, doth ſometimes obvert to our eyes its luminous part, and ſometimes its dark part (as Jupiter is ſaid to do its belt-like ſpots, whence it muſt gradually both appear and diſappear; according to either of theſe two hypotheſes, (though not ſo much as in that which preceded them,) there will be reaſon to queſtion the great uniformity imagined to be in the cœleſtial bodies and motions; and to favour what has been propoſed about periodical mutations in the mundane globes; eſpecially ſince theſe phænomena argue, that even thoſe ſtars we call fixed, and have looked upon as ſo invariable, are ſubject to mutations great enough to be taken notice of by our naked eyes at ſo immenſe a diſtance. I ſhall not here proſecute this diſcourſe, becauſe I would not anticipate what I foreſee I ſhall have occaſion to ſay about the terreſtrial effluvia with their cauſes and effects in another diſcourſe *, but I think myſelf obliged to mind you in this place, that doubts and ſuſpicions are the only things promiſed by the title of this diſcourſe; and therefore I ſhall not quarrel with you, if you conjecture, that though the laſt propoſed ſuſpicion may prove well grounded in ſome caſes, yet in ſome others, the exorbitancies of the matter may, if they chance to be repeated, occaſion a new cuſtom, that may have the force of a law in this, or that part of the mundan globes; particularly in this terreſtrial one we inhabit: As waters, by their frequent overflowings of the banks, that cannot contain them, do ſometimes make themſelves new paſſages by their own deviations, and as it were, affect to run in the chanel they once made. And as it happens alſo in animals, that noxious humours having once found a vent at an iſſue or an ulcer, do conſtantly take their courſe that way. Which brings into my mind this odd obſervation, that having occaſion to paſs ſome years ago out of *England* into *Ireland*, traverſing the maritime county of *Waterford*, the convoy, that went with me, ſhewed me once in my way, at a pretty diſtance off, a mountain, from whoſe higher parts there ran precipitouſly a river (which by my eſtimate was pretty broad) that within but two or three years before, at furtheſt, firſt broke out without any manifeſt cauſe from a great bog, that had been immemorially at the top of that mountain, and to the wonder of the inhabitants, after the firſt eruption of the water, had ſupplied the country with a river ever ſince: the circumſtances of which new phænomenon, I would gladly, at a nearer diſtance, have obſerved, but the convoy was not fond of a curioſity ſo dangerous, in an enemy's country.

OTHER inſtances to the ſame purpoſe I cannot now conveniently ſtay to preſent you, having already made the conjectural part of this eſſay diſproportionate to the other: and I hope there is already enough ſaid in this latter part, to anſwer my deſign, which was to excite your curioſity to ſeek after ſome certainty touching the things doubted of; and ſtrive to enable yourſelf by watchful obſervations, ſomewhat to eaſe me of the troubleſome ſuſpicions I have confeſſed to you, by telling me, whether they are altogether groundleſs or not.

* The reference here made, is to a Tract about the Effects and Cauſes of ſome unheeded Changes in the Air.

OF THE TEMPERATURE OF THE SUBTERRANEAL REGIONS, As to HEAT and COLD.

ADVERTISEMENT.

THE two following tracts were designed to have been accompanied by three or four others, whereof the first treated about the temperature of the regions of the air, as to heat and cold, and had been premised to the two, that now come forth, had it not been judged more proper to reserve them to accompany some other papers concerning the air. To the following tract about the submarine regions, it is thought fit to adjoin some relations about the bottom of the sea; to which was to have been added some observations concerning the saltness of the sea: but in that treatise, some blanks having been left for particulars, which the author could not seasonably find among his loose papers to fill them up with, these that now appear, having no dependance on them, it was not thought fit they should stay any longer for them.

BUT about these several tracts, this general advertisement is to be here given, that being historical pieces, consisting chiefly, (though not only,) of such particulars, as the author must owe to the informations of others, he would not stake his reputation for the truth of every one of them; contenting himself, to have performed what can be reasonably expected of him; which is, that he should carefully make his inquiries from credible persons, who, for the most part, deliver their answer upon their own knowledge; and that he should faithfully set down the accounts he procured from such relators.

Of the TEMPERATURE *of the* SUBTERRANEAL REGIONS, *as to* HEAT *and* COLD.

CHAP. I.

IF when I used to visit mines, I had thought of writing on the subject I am now about to treat of, and had designed to satisfy myself about the temperature of the subterraneal air, as much as I did about the other subjects I was then concerned to be informed of; I think I should have enabled myself to deliver much more upon my own observation, than I shall now pretend to do. But though for the reason newly intimated, and because of my being particularly subject to be offended by any thing, that hinders a full freedom of respiration, I was not solicitous to go down into the deep mines; yet after having discoursed of the temperature of the air above ground, I presume it may not be improper or unwelcome, to say something of the temperature of the subterraneal regions, and of the air reaching thither. For deep mines being places, which very few have had the opportunity, and fewer have had the curiosity to visit, and of which I have scarce found any thing at all observable by classick authors, and by other writers, but very little, especially that I think probable enough to make use of; I presume it will not be unacceptable to you, if of regions so little frequented, and less known, I report what I have been able to learn (by diligent enquiry purposely made) from the credible relations of several eye-witnesses differing in nation, and for the most part unacquainted with each other.

THOUGH I do not think it absurd to suspect, that in some places of the earth, the peculiar constitution of the soil, and other circumstances, may make it reasonable to assign those places fewer or more regions than three; yet speaking in the general, the ternary number seems not inconvenient to be assigned to the subterraneal regions; not so much upon the score of the analogy, that by this division will be established between the regions of the earth and of the air, as because there seems to be a reason of the division included in the division itself. And indeed experience appears to favour it in the subterraneal cavity, that I have hitherto been able to procure an account of from any ocular witness, and (very few excepted) one of the deepest, that we yet know of in the world. And since it has been received for a rule among philosophers, that, which is perfectest or compleatest in its kind, ought to be the standard, whereby the rest are to be measured, or estimated; I shall begin the remaining part of this essay by a relation, that I obtained from a chymist, that had purposely travelled into *Hungary*, and other places, to visit the mines those parts are justly famous for; and who bringing *me the honour of a compliment from* a prince, to whom he belonged, gave me the opportunity of asking him divers questions, his answers whereunto (which I presently after put into writing,) afforded me the ensuing account.

CHAP. II.

THAT very near the orifice of the groove, he felt the air yet warm; but afterwards descending towards the lower parts of the groove, he felt it cold, until he came to such a depth, as he had scarce attained by a quarter of an hour's descent, and that the cold he felt during this time seemed to him considerable, especially when in descending he had reached to a good depth.

THAT after he had passed that cold region, he began by degrees to come into a warmer one, which increased in heat, as he went deeper and deeper. So that in the deeper veins he found the workmen digging with only a slight garment over them; and the subterraneal heat was much greater than that of the free air on the top of the groove, though it were then summer.

[WHAT is here mentioned of a cold region in the earth, has been since confirmed to me by an ingenious physician, upon an observation made in another Hungarian mine, (near a town *whose name I remember not,*) *that was not of gold, but copper,* and of much lesser deepness than that newly spoken of. For this relator answered me, that in going down, he felt a considerable degree of cold. And when I asked, whether he found the like in his return upwards, he told me, he observed it then too. And when I further inquired after the extent of this cold region, he replied, that not expecting to be asked about such circumstances, he had not taken particular notice of them; but thus much information my questions procured me, that he began to feel the above-mentioned coldness, when he could receive no more light at all by the mouth of the groove; and that this cold region lasted, till he came somewhat near the bottom, which was estimated to be about an 100 fathom or more distant (in a streight line) from the top.)

THIS relation agrees well enough for the main with that short, but considerable one of *Morinus*, which I elsewhere cite; who above forty five years ago, visited the deep Hungarian mines in the month of july, and takes notice, that when he came down to the burrows, as he calls them, he did not find any heat, as at the mouth of the well, but the beginning of a very cold, as well as considerably thick region: though I easily believe him, when he confesses, that he felt it much the colder, because he had left off his own cloaths, and put on the slight garments used there by the diggers. He further informs his reader, that when they had descended about 80 fathoms beneath the surface of the earth, he began to feel a breath of an almost lukewarm air; which warmth increased upon him, as he descended lower, pleasing him not a little, because it *freed him from the troublesome scents* of his former coldness. *Adding, that the overseer of* the mine, who conducted him, affirmed to him, as also the officers of other Hungarian mines unanimously did, that in all their mines, at least all the deep ones, after a thick tract of cold earth, there succeeds a lower region, that is always hot. And that after they arrived at *such* a depth, they felt not any more cold, but always heat, how deep soever they dig. And to add upon the by, though this learned man lay much weight upon antiperistasis; yet in the next page to those, that contain what I have been just now relating, he either very candidly or inconsiderately takes notice, that they informed him, that in their mines, whether *more or less deep,* they observed, that at some times in the year a somewhat intenser heat was felt; and the two times, that he expresly names, are those oppositely qualified seasons of summer and winter.

HAVING laid down these general narratives, I now proceed to consider the earth's regions in particular; about which the sum of what I yet have to propound, may be conveniently enough comprised in the four following propositions.

CHAP. III.

PROPOSITION. I.

"THE first region of the earth is very "variable, both as to bounds, and as to "temperature."

THE former part of this observation will not be difficult to prove, since it will be easily granted, that the manifest operation of the sun-beams is, *cæteris paribus*, greater, and reaches further in hot climates than in cold ones; in the midst of summer, than in the depth of winter.

THE second part of the observation may be proved by the same arguments as the first; to which may be added, as to some places, the solidity or porousness of the earth; as also the nature of some salts, marchasites, and other bodies contained in it, which by their natural temperature may dispose the soil to coldness or *heat. As I shall have* occasion to shew, when *I come to speak of the second region.*

IN the mean time I have this to observe further, that in this first region, the air is usually more temperate, as to cold and heat, than that above the surface of the earth; and that this region is not wont to be considerably deep: both parts of which observation are capable of being made good by the same reasons, and therefore I shall endeavour to prove them jointly.

THAT in the uppermost region of the earth it should be less cold than above the surface, seems reasonable to be allowed upon this consideration; that the subterraneal cavities of the earth are sheltered by the thickness of the sides from the direct action of the sun-beams, the winds, &c. and is also kept from an immediate

diate, or at leaſt from ſo full a contact of the external air, when that is vehemently, either heated or refrigerated.

AND firſt as to the heat of the ſun, that that does much leſs powerfully affect ſuch places as are ſheltered from its action by ſolid bodies, may appear by the conſervatories of ice and ſnow, wherein frozen water is kept in that ſtate during all the heat of ſummer, and that oftentimes in cavities, that are at no conſiderable depth beneath the ſuperficies of the earth. Nay I remember, that having had occaſion (for the perfecting of ſome concluſions I was trying) to keep ice many weeks after the froſty weather was gone, and a milder ſeaſon was come in, I was able to do it, contrary to the expectation of ſome curious men, without either digging to a notable depth in the ground, or building any ſubſtantial ſtructure over the cavity. For wanting conveniencies, I contented my ſelf, though it were in a champain place, with a pit ſomewhat broad at the bottom, of about four foot deep or leſs, whoſe mouth was ſheltered only by a little low thatched hovel, that was wide open to the north, and only ſcreened the mouth or vent of the little pit from the direct beams of the ſun. And though I will not deny, that in deep conſervatories of ſnow, the natural coldneſs of the earth, eſpecially in ſome places, may contribute to the effect; yet I remember, that diſcourſing once with a traveller and ſcholar, that was born in hot countries, of a conjecture of mine, that in an arched building, whoſe walls were ſufficiently thick, and whoſe air were carefully kept from all avoidable intercourſe with the external air, one may, without digging ſo much as a man's depth into the ground, make a ſufficient conſervatory for ice in very open and unſheltered places, and even ſuch as *Saliſbury-Plain* itſelf: diſcourſing, as I began to ſay, with this traveller about this conjecture, he told me, that at a place he named to me, in the ſouthern part of *France*, whoſe heat ſeemed to me to exceed that of divers parts of *Italy*, ſome curious perſons, that were reſolved at any rate to have ice in ſummer, though the ſoil were ſuch, that they could not dig four foot without meeting with water, were yet able to make uſe of conſervatories, by covering the brick-building they made over their pits, with clay and ſand, to a very conſiderable thickneſs, and taking care, that the only place, that ſhould permit acceſs to the outward air, ſhould be a ſmall northern door to go in and out at, fitted to ſhut exactly cloſe, and fenced with a little porch, furniſhed with another door. And by this means he affirms theſe gentlemen to reſerve the included ice, not only all the ſummer long, but ſometimes for two or three years together, the heat of that region making many of their winters too mild to recruit them with ice.

To all theſe things I ſhall add, that even where the intercourſe is not quite debarred, but left free enough betwixt the ſubterraneal and the ſuperior air, the operation of the ſunbeams may be very much leſs in a cavity though but ſhallow, beneath the ſurface of the ground than above it. For beſides that trials have informed me, that liquors, that differ in little elſe than in conſiſtence, will not ſo eaſily pervade each other, as a man would ſurmiſe, unleſs ſome external motion haſten their intimate mingling with one another; I remember, that one morning pretty late, having had the curioſity to deſcend into a pit, where they were digging out iron oar; though this cavity had no very narrow orifice, and was dug directly downwards, and exceeded not ten or twelve foot in depth, yet I found not the heat at all troubleſome, whilſt I ſtayed there, though the pit were in an open field, unſhaded by trees, and though the air abroad were much heated at that time of the year, which was in that ſeaſon (or at leaſt very near it) that is wont to be called the dog-days.

CHAP. IV.

AND as we have ſhewn, that the ſubterraneal air, even in the firſt region, is uſually much leſs heated, than the ſuperterreſtrial air; ſo we may alſo eaſily obſerve, that that inferior air is (*cæteris paribus*) wont to be much leſs refrigerated by the grand efficients of intenſe cold, than the ſuperior air.

I will not urge on this occaſion what I have obſerved by a ſurer way, than for ought I know has been before practiced, about the ſmoaking of ſome ſprings in froſty weather; becauſe I do not know, but that thoſe ſprings may have come from, or paſſed a good way through, ſome place very deep beneath the ſurface of the directly incumbent ground, and perhaps from a ſoil peculiarly fitted to warm them; whence the water may have derived a warmth conſiderable enough not to be quite loſt, till it began to ſpring out of the ground, where it needed only not to be quite cold, to appear to ſmoke; the intenſe coldneſs of the air making thoſe exhalations viſible in froſty weather, which would not be ſo in milder: as is evident in a man's breath, which appears like a ſmoke in ſuch weather, though it be not viſible in ſummer.

THAT therefore, which I ſhall propoſe in favour of our obſervation, is firſt taken from the nature of the thing, which may perſuade us, that the ſubterraneal air being though comparatively cool, yet indeed moderately warm in ſummer, ought not to be affected with winter's cold, ſo much as that contiguous to the ſurface of the earth, from whoſe immediate contact it is by a thick arch of earth, if I may ſo call it, defended; and that the cold reigns moſt in the free air and the ſuperficial parts of the terreſtrial globe, may appear by water's beginning to freeze at the top, not at the bottom. To which reaſon from the nature of the thing, I ſhall add only this from experience, that we ſee, that in cellars, that are arched and carefully kept cloſe from the communication of the outward air, beer, and other liquors may be kept from freezing in froſty and ſnowy weather. As I have obſerved in a cellar, that was but ſhallow, but well arched in a winter, that was ſharp to a wonder, and froze ſtronger liquors than beer in another cellar very near it, that differed not much from it in depth, but had not ſo thick and ſolid a roof. And that not only here in *England*, where the cold is leſs violent, but even in *Ruſſia* itſelf, where it is wont to be ſo extreme, it reaches not near ſo deep as one would think, I learned by inquiry purpoſely made of an ingenious phyſician, that lived at *Moſcow*, who anſwered me, that others, and he himſelf, did in that city keep all the winter long, not only their wine, but their beer from freezing in cellars, that were not above twelve or fourteen foot deep, but well covered above, and carefully lined with planks of fir, without any entrance, but a ſmall trap-door (commonly at the top) which was fitted ſo exactly to the orifice it was to cloſe, as to exclude, as much as was poſſible, all communication between the internal and external air, that the latter might not affect the former with its coldneſs.

I have indeed ſuſpected, that in ſome cellars, the comparative warmth we find there may be partly due to ſubterraneal exhalations, that are pent up in them; and perhaps too in ſome meaſure from the ſteams of the fermenting, or fermented liquors lodged in thoſe places. And I was ſomewhat confirmed in this ſuſpicion, by an information my inquiries obtained from the newly mentioned doctor, who told me, upon his own obſervation, that in one of the cellars he made uſe of at *Moſcow*, having occaſion to open the above mentioned trap-door, after the cellar had for a good while been kept very cloſe ſhut, there came out at the vent, that was thereby given, a copious ſteam in the form of ſmoke, which to them, who had their bodies affected with the external air, was very ſenſibly warm, and was almoſt unfit for reſpiration. Which circumſtance increaſed my ſuſpicion, that there might be among theſe ſteams ſome of the nature of thoſe, that have been obſerved to come from fermenting liquors, eſpecially wine, and ſo abound in ſome cellars, as almoſt to ſtifle thoſe, that ventured into thoſe vaults, and to kill ſome of them outright. Which effects the long abode of ſubterraneal ſteams in ſtagnating air, even in many places, where no metalline oars at all, nor other noxious minerals have been found, has enabled that air to produce. Of which divers ſad inſtances have been given within leſs than a mile of this place, upon men's firſt going down into pits or wells, that had not in a long time been opened or made uſe of: but this is here mentioned only upon the by; nor have we any neceſſity to fly to ſubterraneal exhalations, for the comparative warmth, that good cellars in general afford in froſty weather; ſince that phænomenon may be accounted for by the reaſon formerly given, that the cloſeneſs of the cavity, and the thickneſs of the ſides and roof, keep it from being vehemently affected with the cold of the ambient air.

I know it is pretended, that the warmth we ſpeak of, proceeds from an antiperiſtaſis: but not now to engage in a controverſy, that would take up too much time, it may here ſuffice to repreſent, that in our caſe there appears no neceſſity of recurring to it, the phænomenon being ſolvable by the region newly cited, which may be confirmed by this experiment, that in the vaulted cellar above-mentioned, wherein beer was kept from freezing, in an almoſt prodigiouſly ſharp winter, the included air, though ſenſibly warm to thoſe, that came out of the free air, had not ſo intended its native heat, as the aſſertors of antiperiſtaſis would have expected; being colder than the free air commonly is in that place, not only in the heat of ſummer, but in other ſeaſons, when the weather is temperate; as I was aſſured by comparing my own obſervations, made at other times, with the account brought me by a ſkillful perſon, whom I employed into that cellar at late hours, in one or two of the ſharpeſt nights of the forementioned cruel winter, with the ſame excellent ſealed weather-glaſs, that I had long kept ſuſpended within a ſtone's caſt of that place.

CHAP. V.

HAVING ſaid thus much about the earth's uppermoſt region, I now proceed to that, which lies next beneath it; whoſe temperature I cannot ſo conveniently give an account of, in leſs than two propoſitions, whereof the firſt is this;

PROPOSITION II.

"THE ſecond region of the earth ſeems to "be for the moſt part cold in compariſon of "the other two."

THIS propoſition may be confirmed partly by reaſon, and partly by experience.

AND firſt it ſeems conſonant to reaſon, that ſince the earth is naturally a body, conſiſting of groſs and heavy parts, that are uſually much leſs agitated, than thoſe of our organs of feeling, it ſhould as to ſenſe be cold; and that therefore that quality may be juſtly aſcribed to it, in that region, where by virtue of its ſituation it is kept from being conſiderably affected, either by the heat of the ſuperior air, or by that of the deep parts of the earth: which upper and lower heat are the two agents, that ſeem of all others the moſt likely to put its parts into an unuſual motion, and thereby change its natural temper.

THAT our propoſition is alſo confirmable by experience, may be gathered from the relations ſet down in the former part of this diſcourſe.

AND here it will be proper to take notice of the advertiſement intimated in the cloſe of our above delivered propoſition, that this coldneſs aſcribed to the ſecond region of the earth is to be underſtood comparatively to the other two. For otherwiſe, that even this earth is not, as many naturaliſts would have it, the *ſummum frigidum*, I gather from this, that I could never hear of any ice met with there, at any time of the year, though ſnow or hail may be produced in the middle region at differing, and ſometimes quite oppoſite ſeaſons of the year. Nay, I have not found by the anſwers, that were made me by thoſe, that have deſcended far enough into this region, that they found the cold any where very great, or that in ſome places they have found it at all conſiderable; as we ſhall ſee in the explication of the next propoſition. I know not, whether it will much ſtrengthen what has been ſaid, if I add, that I learned by enquiry of ſuch perſons as I lately mentioned, that at the mouth of deep grooves, in mines, the ſteams, that aſcend, do often feel warm; though the outward air, where the obſervation is made, be affected with the heat of ſummer. But though this probably argue, that if the middle region of the earth, through which theſe ſteams muſt aſcend, were very intenſely cold, they would be ſo refrigerated in their paſſage, as to feel rather cold than hot at their appearing above ground, eſpecially in ſummer: yet I ſhall not lay much weight (for ſome may perhaps be allowed it) upon this argument; becauſe I have not yet tried, how far a warm ſteam may be altered in its paſſage, through a cold conduit: not to mention, that in the earth, the paſſage by being directly upwards may be much the nimblier traverſed.

CHAP. VI.

THE ſecond propoſition relating to the temperature of the ſecond region of the earth may be delivered in theſe terms.

PROPOSITION III.

"In ſeveral places, which by reaſon of their "diſtance from the ſurface of the earth, one "would refer to the middle region of it, the "temperature of the air is very differing at the "ſame times of the year.

I choſe to expreſs my ſelf thus, to prevent ſome ambiguities and objections, which I foreſaw, that ſhorter, but leſs dear and full expreſſions, might give occaſion to.

IN the proof of our propoſition, both experience and reaſon may diſtinctly be imployed. And to begin with experience,

WHEREAS in the above recited deſcent into the Hungarian mines, there was obſerved a notably cold region of a conſiderable thickneſs, I have purpoſely procured accounts from divers perſons, that have here in *England* had occaſion, ſome of them, frequently to deſcend into deep pits or grooves of differing minerals, without finding by the narratives they made me, that they took notice of any notably cold part that they paſſed thorough; unleſs I particularly aſked a queſtion about ſuch a thing. But for ought I could gather from their ſpontaneous relations, they felt in ſummer-time a remiſſion of the heat of the external air, as ſoon as ever they began to deſcend; which warmth did not ſo far decreaſe, as to terminate in any notable coldneſs, before they came into a deeper part of the earth, where they are never troubled with that quality. And ſome of theſe relations I had from profeſſed miners, and was curious, that the relations I procured ſhould be of ſubterraneal parts ſeated in very differing parts of *England*, as well as of places not all, or moſt of them having veins of one and the ſame mineral. And I learned by particular inquiry from a practical mathematician, that was often imployed about lead mines, that at ſuch depths, as (according to *Morinus*) the ſecond region of the earth reaches to, he himſelf obſerved it to be ſenſibly warm at all ſeaſons of the year (for about that circumſtance, I was peculiarly ſollicitous to be ſatisfied.)

NOR is it unconſonant to reaſon, that the middle region of the earth, in the ſenſe meant in the propoſition, ſhould not be of the ſame temperature in all places; not only becauſe of the differences, which the climate may produce by reaſon of its being very much hotter, or very much colder in one place than in another: but from the peculiar conſtitution of the ſoil; to the conſideration whereof I ſhall here confine my ſelf.

Now this temperament of the ſoil it ſelf may be diverſified, not only by its greater or leſſer compactneſs (upon which account ſome ſoils are rocky or ſtony and others light and ſpongy) but from the nature of the ſprings or ſubterraneous liquors, that may abound in it, or ſtrain through it into the groove or pit, we ſuppoſe the obſerver to be in; and that eſpecially by the minerals, particularly ſalts, and marchaſites, that grow near the ſides of the well, or are brought thither by the waters.

To illuſtrate this, give me leave to conſider, that nature does not regulate her ſelf under ground by our imaginary diviſions; but, without taking notice of them, produces marchaſites, ſalts, and other minerals, moſt frequently perhaps in what we call the lower region of the earth; but yet ſometimes too in our upper region, and oftentimes in our middlemoſt region. Let us then ſuppoſe, that in ſome places of this laſt named region, there be a mine of that earth, that naturally abounds with embryonated nitre, or with ſome other ſalt, that is apt, eſpecially being diſſolved or moiſtened with water, (a thing very familiarly to be met with in mines;) to ſend out a refrigerating effluvium, or by its contact to cool the air. Let us alſo ſuppoſe, that by the ſides of another well of the ſame depth, there are ſtore of unripe minerals, that are in the proceſs of generation, or rather a great quantity of marchaſitical earth, if I may ſo call it, that is ſuch a ſubſtance, as I have met with in more than one place, copiouſly impregnated, and as it were blended with minerals of a marchaſitical nature; and yet of ſo open and looſe a texture, as not only water would in a few hours, but air alſo would not in very many evidently work upon it. And ſince during the time, that marchaſites are ſlowly diſſolving, it has been obſerved, according to what we have elſewhere delivered*, that many of them will conceive a very conſiderable degree of heat; will it not be very probable, that the temperature of the earth in the place, that abounds with theſe marchaſitical minerals, will be very warm in compariſon of the temperature of the other place, where the ſoil does plentifully produce nitrous, and other refrigerating bodies; though

* The tract here pointed at is a Diſcourſe of Subterraneal fires and heats.

though both the places be suppofed to be at the fame diftance from the furface of the earth, and confequently in the fame fubterraneous region.

Upon the like grounds, it may alfo be fufpected, that in the fame places the temperature may not be always the fame, even upon the account of the foil. For I elfewhere fhew, that fome faline earths, efpecially nitrous, and fome minerals, that partake of the nature of marchafites, admit a kind of maturation, and perhaps other changes, that feem to be fpontaneous; and that fuch changes happen the more notably in thofe parts of fuch bodies, that are expofed to the air, as thofe are, that chance to be placed at the fides of the deep wells we are talking of. Which things being pre-fuppofed, it will not be abfurd to conceive, that the mineral, to which either heat or cold is to be referred, may be more copious, ripe, and operative at one time, than at another; or, that at length all the earth capable of being, as it were, affimilated by the mineral rudiments harboured in it, may be confumed, or the mineral it felf may arrive at a perfection of maturity, which will make its texture fo clofe, as to be unfit to be penetrated, and wrought upon, as before, by the water or other liquor, that occafioned its incalefcence.

CHAP. VII.

I OMIT to fpeak of the tranfient changes, that may be occafioned in the temperature of the fecond region of the earth by feveral accidents, and efpecially by the fubterraneal exhalations, that in fome places and times copioufly afcend out of the lower regions of the earth. Nor fhall I infift upon any of the other caufes of a more durable difference of temper in fome parts of the fecond region, fuch as may be the vicinity of fubteraneal fires in the third region that heat the incumbent foil; becaufe I would haften to the third and laft part of this difcourfe; which yet I muft not do, without premifing this advertifement, that I think myfelf obliged to fpeak the more hefitantly and diffidently about the temperature of fubterraneal air, becaufe mineralifts have not had the curiofity to examine it by weather-glaffes, which would give us much more trufty informations, than our fenfe of feeling powerfully pre-affected by the cold or heat of the external air. I did indeed fend fit inftruments to fome days journey from this place, to examine the air at the bottom of fome of our deep mines; but through fome unlucky cafualties upon the place, the attempt mifcarried. But when I fhall (God affifting) recover an opportunity, that I have fince wanted, I hope an accurate fealed weather-glafs, joined with a portable barofcope, will give me better information than mineralifts have yet done. I fay a fealed weather-glafs, becaufe though common thermofcopes had been employed by miners, I durft not rely upon them; being perfwaded by trials purpofely made, as well as by the reafon of the thing, of the fallacioufnefs of fuch thermofcopes: for in them the included air is liable to be wrought upon, not only by the heat and coldnefs, but by the weight or preffure of the external air. So that if a thermofcope be let down from a very confiderable height, at the top of which the ftation of the pendulous liquor be well marked, that liquor will be found to have rifen, when the inftrument refts at the bottom, as if the included air were manifeftly refrigerated; though the temper of the external air may be in both places alike, the caufe of the pendulous liquor's rifing being indeed, that the aereal pillar incumbent on the ftagnant liquor is higher and heavier at the bottom, where the inftrument refts, than that, which leaned upon it, at its firft or upper ftation nearer the top of the atmofphere. From whence it will be eafy to conclude, that at the bottom of a deep groove, where the atmofpherical pillar, that preffes the ftagnant water, will be much longer and heavier than at the top, the air may appear by the inftrument to be colder in places, where it is really much hotter, the increafed weight of the incumbent air being more forcible to impel up the pendulous liquor, than the endeavour of expanfion procured in the included air by the warmth of the place is to deprefs it.

CHAP. VIII.

THAT, which challenges the third and laft part of my difcourfe, is the lowermoft region of the earth, about whofe temperature I fhall comprize, what I have to fay in the following propofition.

Proposition IV.

" The third region of the earth has been
" obferved to be conftantly and fenfibly warm,
" but not uniformly fo, being in fome places
" confiderably hot."

I mention, that the recited temperature has been obferved in the lower region, becaufe I would intimate, that I would have the propofition underftood with this limitation, as far as has been yet (that I know of) obferved. For almoft all the deep grooves, that mineralifts have given us accounts of, and wherein men have wrought long enough to take fufficient notice of the temperature of the air, have been made in foils furnifhed with metalline oars, or other minerals, without which men would not be invited to be at fo great a charge, as that of finking fo very deep pits, and maintaining workmen in them. So that experience has yet but flenderly, or at leaft not fufficiently informed us of the temperature of thofe parts of the third region of the earth, that are not furnifhed with ponderous minerals; and confequently has not informed us of the temperature of the lowermoft region in general; as will better appear by what I fhall ere long reprefent.

Having premifed this advertifement about our propofition, we may proceed to the diftinct proof of the two parts or members it confifts of.

And to begin with the firft, whatever the peripateticks teach of the innate coldnefs of the earth, efpecially where it is remoteft from the mixture

mixture of the other elements; yet having purposely enquired of several persons, that visited and also frequented the third region in differing countries, soils, and at differing depths under ground, and seasons of the year, I did not perceive, that any of them had ever found it sensibly and troublesomely cold in the third region of the earth. And on this occasion I remember, I had some light suspicion, that at least in some cases, the narrowness of the cavities, wherein the diggers were in divers places reduced to work, might make the warmth they felt proceed in great part from the steams of their own bodies, and perhaps of the minerals, and from the difficulty of cooling or ventilating the blood in an air clogged with steams. And I was the rather induced to think this possible, because I had (even in metalline mines, that were but shallow and very freely accessible to the air) observed a strong smell of the metal abounding there.

I have likewise found by several trials, that the exhalations, that proceed from the bodies of animals, do so vitiate the air they abound in, as to make it much less fit for their respiration, and to be apt to make them sick and faint. Wherefore I thought it not altogether unfit to inquire, whether the heat of the subterraneal air, in such places as have been newly mentioned, might not be referred to these causes. But I was answered in the negative; especially by an inquisitive person, that had been in the deepest and hottest mines, that have been visited by any acquaintances of mine.

This way of accounting for the subterraneal warmth being laid aside, it seemed, I confess somewhat difficult to conceive, how it should be produced yet; two principal causes there are, to which I think we may probably refer the temperature of those places, where the air is but moderately warm. To which a third is to be added; when we come to give an account, why some places are troublesomely hot.

And first, why the coldness of winter should not be felt in the lowermost region of the earth may be, that the air there is too remote from the superterrestrial air, to be much affected with those adventitious causes of cold, that make that quality intense in the air above ground. But because this reason shews rather, why it should not be in the earth's lower region much colder in winter than in summer, but not why it should be in all seasons warm there; I shall add as a conjecture, that the positive cause of the actual warmth may proceed from those deeper parts of the subterraneal region, which lie beneath those places, which men have yet had occasion and ability to dig. For it seems probable to me, that in these yet unpenetrated bowels of the earth, there are great store-houses of either actual fires, or places considerably hot, or, (in some regions) of both; from which reconditories (if I may so call them) or magazines of hypogeal heat, that quality is communicated, especially by subterraneal channels, clefts, fibres, or other conveyances, to the less deep parts of the earth, either by a propagation of heat through the substance of the interposed part of the soil, (as when the upper part of an oven is remisly heated by the same agents, that produce an intense heat in the cavity,) or by a more easy diffusion of the fire or heat through the above-mentioned conveyances, as may be exemplified by the pipes, that convey heat in some chemical structures:) or else, (which is perhaps the most usual way,) by sending upwards hot mineral exhalations and steams, which by reason of the comparatively heavy minerals they consist of, and by reason of their being less dispersed nearer the places whence they proceed, are usually more plentiful in the deeper parts of the earth, and somewhat affect them with the quality, that they brought from the work-houses where they were formed, and that they retain for some time after.

CHAP. IX.

That manifest steams oftentimes are found in grooves, especially in deep ones, is evident by the damps, that infest most of them, and that in distant regions, as in several provinces of *Germany*, *Bohemia*, *Hungary*, &c. as also in several parts of *England*, in grooves, some of which I have received relations from the mine-men themselves. By which it appears, that several of these exhalations ascending from the entrails of the earth are sulphureous and bituminous in smell, and in some grooves (one whereof I elsewhere mention my self to have visited) these steams are apt, actually to take fire.

The warmth of many subterraneal exhalations, I think, may be made further probable by some other observations. For though these newly mentioned are not to be rejected, and may be employed for want of better; yet I have several times questioned, whether I ought to acquiesce in them alone. For I do not think the easy inflammableness of bodies to be always a sure proof of the actual sensible warmth of the minute parts it consists of, or may be reduced into. For though salt-petre be very inflammable, yet being by a solution in fair water reduced to invisible corpuscles, it highly refrigerates that liquor. Nor have I observed its fumes, (when far from the fire,) to have any heat sensible to our touch. And the like may be said of the exhalations of highly rectified spirit of wine; which yet we know is itself totally inflammable. Nay I know not, whether (for a reason elsewhere declared) copious exhalations may not ascend from the lower parts of the earth, and yet be rather cold than hot. For, in another paper, I mention a way by which I made a mixture, that plentifully enough emitted steams, of whose being rather of a cold, than a hot nature, there was this probability, that the mixture whence they ascended, even whilst its component ingredients were briskly acting upon one another, was not only sensibly, but considerably, cold.

One main thing therefore, that induces me to assent to the opinion, whereto the former instances do but incline me, is, that having purposely inquired of an observing man, that frequented deep mines, (wherein he had a considerable share,) he answered me, that he plainly observed the fumes, that came out of the mouths of the deep pits, to be actually and sensibly warm, and, that in a warm season of the year. And *Morinus* (above cited) speaking of the deep Hungarian mines, makes it the first epithet of the copious exhalation, that ascended from the bottom, that it was hot. And a few pages after he says, that at the mouth of the well, the ascending fumes were sensibly hot in summer itself. And the same arguments, that I have elsewhere given to shew, that there are very hot places, and, as it were æstuary in the bowels of the earth, may serve to make it probable, that the steams ascending thence may be actually warm.

That also in many places of the earth, where no grooves are dug, and no visible exhalations are taken notice of, they may yet pervade the soil, and exercise some operations of warmth, may be probable by this, that the experienced *Agricola* himself reckons it among the signs of a latent mineral vein, that the hoar-frost does not lie upon that tract of the surface of the earth, under which a vein (though perhaps very deep) runs. The like directions I have known given by the skilful in *England*, for the discovery of places, that contain coal-mines. And I remember a near relation of mine shewed me a great scope of land of his, which (though in an outward appearance likely to be as cold as any place thereabouts,) he affirmed, would not suffer snow to lie upon it above a day or two in the midst of winter.

The probability of which relation was confirmed to me by the answer I received from a very ingenious gentleman, who lives among mines, and is not a little concerned in some of them. For having inquired of him, what he had observed about the lying, or not lying of the snow on the mineral soils near the place of his residence; he replied, that in some of them, he did not take notice of any peculiar indisposition to let the ice and snow continue on them: which I conceive may proceed, either from the want of such minerals in the subjacent parts, as were then in the state of incalescence; or else from this, that (according to what we have elsewhere observed about the snow on *Ætna*) the direct ascension of the hot steams was hindered by some layers of rocks or stone, through which the steams could not penetrate, or could do it but so slowly, as to loose their actual warmth by the way. But this gentleman added, that in other places, near that of his abode, and such as he knew to have mineral veins beneath them, he observed, that the snow, (nor the ice) would scarce continue at all upon the surface of the ground, even in an extraordinary cold winter.

It will be a considerable instance to our purpose, if it be indeed true, which some learned men have written, that near the gold-mines in *Hungary*, the leaves of the trees (especially those, that respect the ground) are oftentimes found enobled with a golden colour from the metalline exhalations of the gold mines; which, one would think, must by reason of their ponderousness need a considerable heat to elevate them, especially into the open air. But though doubting of this relation, as not made by mineralists or accurate observers, I inquired about it of a person, whose curiosity carried him purposely to visit those mines, I was answered, that he could not be a witness to the truth of the observation; yet he told me an observation (which I elsewhere mention) that doth not discountenance that tradition.

If it be objected, that what has hitherto been said about latent fires and heats in the bowels of the earth, will give an account of the warmth only of those places, that are within reach of the action of such magazines of heat, which probably may be wanting in many places of the earth; I shall readily confess, that as I first made this objection to myself, so I do not yet discern it to be unreasonable; and, that for ought I know, if men had occasion to dig as deep, and be as far conversant in many other low places of the earth, where there are no signs of minerals, as they have done, where the hopes of actual discovery of veins of metals, and other minerals worth working, have invited then, divers places in the third region of the earth would be met with, that would be destitute of the warmth, that has hitherto been generally found in places of the same region, that either abound with minerals themselves, or are near some of the deep and latent æstuaries above-mentioned.

And as for those parts of the third region of the earth, which men feel not only warm, but troublesomely hot, that incommodious degree of heat seems not, (at least in some places) to be derivable from the two above-mentioned causes; which must, (to produce so considerable an effect,) be assisted by a third cause more potent than themselves: which seems to be the incalescence there is produced in many mines, and other places, by the mutual action of the component parts promoted by water of immature and more loosely contexed minerals, especially such as are of a marchasitical nature. That such an incalescence may by such a way be produced in the bowels of the earth, I have elsewhere shewn (in my discourse of subterraneal fires and heats) by the examples of such incalescences producible in mineral bodies here above ground. That marchasites, which for the most part abound in vitriol, are bodies very fit to procure this subterraneal heat, may be confirmed not only by the sulphureous and saline parts they abound with, and by this, that many of them may be wrought on, as we have tried, both by simple water, and even by moist air, which argues the resolubleness of their constitution; but also by this, that having purposely inquired of a gentleman, that went out of curiosity to visit one of the deeper Hungarian mines, he confirmed to me what I had otherwise been informed of, by answering me, that in the lower parts of the mine, he had gathered vitriol, that appeared above ground to be of a golden nature; and, that in a cave, that is on one side of the groove, in the deep gold mine near *Cremnitzo*, the corrosive smell is so strong and noxious, that men have not dared to dig out the native gold it richly abounds with, being deterred by the ill fate of divers, that ventured to work in it. Adding, that though he paſſed by it in great haſte, yet he could not avoid the being offended by the noiſome exhalations. And on this occaſion, it will not be, I preſume, diſliked, if I illuſtrate what I was ſaying of immature minerals, by ſubjoining, that having aſked this chemiſt, whether the vitriol he found very deep under ground were all ſolid, or ſome of it ſoft? he affirmed, that as he gathered it, he found ſome of it ſoft. And to ſatisfy my curioſity, to know whether it continued that yeilding conſiſtence? he farther told me, that it was ſoft in the deeper part of the mine; but when he had brought it into the ſuperterreſtial air, it hardened there, and appeared to have nine divers golden ſtreaks in it.

CHAP. X.

ONE thing there is, which muſt not be here omitted, though it will probably be great news to thoſe, that philoſophize only in their ſtudies, and have not received information from any that viſited the deeper parts of the earth. The phænomenon is this, that the diggers in mines, having found by unwelcome experience, that in deep grooves, the air (unleſs ventilated and renewed) does in a ſhort time become unfit for reſpiration, have been put upon this expedient, to ſink, at ſome convenient diſtance from the groove where the miners work, another pit, by ſome called a vent pint, that uſually tends directly downwards (though ſometimes it make angles) to which our Engliſh mine-men do in ſeveral parts of this kingdom give differing names, whereof the moſt ſignificant ſeems to be that given it in the lead mines of *Derbyſhire*, where they call it an air-ſhaft, and are wont to make it 40, 50, and ſometimes 80 or 100 paces off; and, as one of the chief and ſkilful miners there informed me, as deep as the groove or well; (though I find, that the beſt German and ſome Engliſh miners think a leſs depth will often ſuffice) from this air-ſhaft to the groove the men work in there paſſes a channel, or, if I may ſo call it, ventiduct, to convey the air from the former to the latter; which is that, that *Agricola* ſometimes (for he employs not the term always in the ſame ſenſe) denotes by his cuniculus; and which though differingly named by our miners in ſeveral parts of *England*, is in the above-mentioned lead mines called a drift, becauſe the air does uſually in the form of wind drive through it; and thereby enables the workmen to breathe freely and conveniently enough at the very bottom of the well. On this occaſion I remember, that a very obſerving man, who much frequented theſe mines, told me, that at the depth of no leſs than about 200 yards, he found, that by the help of the air-ſhaft, the air was not only very commodious for reſpiration, but temperate as to heat and cold. And when I further aſked, what time of the year it then was? he told me it was about the latter end of Auguſt, and the beginning of September.

Lib. V. & VI. de re metall.

NOW that, which ſeems to me to deſerve a farther and accurate obſervation about the motion and temperature of the air in theſe artificial under-ground cavities, is a relation of *Agricola*'s, which (though he be the moſt claſſick author we have about mines) has not, that I know of, been taken notice of, in him. For this experienced writer, though in his treatiſe * *de ortu & cauſis ſubterraneorum*, he only ſays indefinitely, that by means of the cuniculus or drift, which connects the air-ſhaft and the well, that air, which comes in at one of thoſe two, paſſes out at the other; yet in his fifth book, *de re metallicâ*, he gives a more particular and odd account of the courſe of the air in theſe not over-clear terms, *aer autem exterior ſe ſuâ ſponte fundit in cava terræ, atque cum per ea penetrare poteſt, rurſus evolat foras. Sed diverſâ ratione hoc fieri ſolet; etenim vernis & æſtivis diebus in altiorem puteum influit, & per cuniculum vel foſſam latentem permeat, ac ex humiliori effluit; ſimiliter iiſdem diebus in altiorem cuniculum infunditur, & interjecto puteo defluit in humiliorem cuniculum, atque ex eo emanat. Autumnali verò & hyberno tempore contra in cuniculum vel puteum humiliorem intrat, & ex altiori exit: verum ea fluxionum aeris mutatio in temperatis regionibus fit in initio veris, & in fine autumni: in frigidis autem, in fine veris & in initio autumni.* To which he adds, † that which is more remarkable, that the air in both the mentioned times, before its wonted courſe come to be durably ſettled, uſes to be for the ſpace of a fortnight liable to frequent changes, ſometimes flowing into the upper or higher groove or drift, and ſometimes into the lower, and paſſing out at the other. If this obſervation conſtantly hold, though but in ſome deep mines, it may hint ſome odd inquiries about conſiderable and periodical changes in the ſubterraneal parts of the earth, or in the air, or in both; which, though they have not yet been conſidered, deſerve to be ſo. I have endeavoured to learn, whether any ſuch thing has been obſerved in ſome deep lead mines, whence I have procured divers informations about other particulars. But a very obſerving perſon, that had the chief hand in contriving the ſubterraneal ſtructures there, aſſured me, that both winter and ſummer, the current air, went conſtantly the ſame way; the air entring in at the mouth of the air-ſhaft, and coming out at the perpendicular groove, which takes its denomination from a cave, (or *caſa putealis*) uſually built over the orifice of it, to ſhelter the workmen from rain, and other inconveniences.

AND ſince the writing of this, I found in *Morinus* (his relation already mentioned) a paſſage, that may ſomewhat illuſtrate the darkly expreſſed

* Idcirco ſcrobes, putei cuniculi effoſſi complentur exteriore aere. Atque ipſum in eos influere imprimis hyemali tempore evidens eſt in duobus puteis, ad quorum utrumque ex modico intervallo cuniculus aliquis pertinet. Nam aer in unum continuo influit, rectàque per cuniculum permeat & tranſit ad alterum; atque ex eo rurſus evolat foras.

† Sed aer utroque tempore, anteaquam curſum ſuum illum conſuetum conſtanter teneat plerumque quatuor decem dierum ſpatio crebas habet mutationes, modo in altiorem puteum vel cuniculum influens, modo in humiliorem.

expressed observation of *Agricola*. For the lately mentioned author writes, that in the deep Hungarian mines he visited, the outward air passed, first, through the boroughs, and so through by-ways, if I may so call them, that tended not directly downwards, reached at length to the bottom of the well, or perpendicular groove, whence, together with the steams proceeding from the mine, it ascended strait upwards. But *Morinus* taking no notice at all of *Agricola*'s observation about the differing course of the subterraneal air at differing seasons of the year, though, as I find by what he writes elsewhere, it was summer when he visited the mines, and so what he reports, agrees well with one part of what *Agricola* seems to say; yet, as to the other, and principal part of his observation, he says not any thing. And the sensible heat he ascribes to the steams ascending out of the perpendicular well, leaves it somewhat dubious, what interest the rarefaction of the air by the subterraneous heat may have in the phænomena we have been discoursing of.

But to return to what I was saying before I had occasion to mention *Morinus*. Which perhaps it will not be impertinent to add, that I learned by inquiry, that the air-shafts and the wells were in these mines much of a depth. But I hope before long to have accounts of what happens in other mines, in other parts of *England*, as to the course of the subterraneal air, especially when its issuing out of the well or the air-shaft depend not on the changes of the winds, that blow above ground: and I wish the curious would employ the like endeavours in other countries.

For indeed, what I have hitherto discoursed in this treatise, is accommodated but to the scant information I have hitherto received; and therefore ought to be rectified, or confirmed, by farther informations, if they can be procured.

In the mean time, I think, I may probably enough gather from the passed discourse, that though in some mines, three subterraneal regions, and their distinguishing attributes, may be not inconveniently assigned; yet generally speaking of the whole body of the terrestial globe, as far as we know it, both the bounds and the temperature of the regions of the earth, as well as those of the air, are various and uncertain enough.

And much less have we any certain knowledge of the temperature of the more inward, and, if I may so speak, the more central parts of the earth; in which, whether there be not a continued solidity, or great tracts of fluid matter, and whether or no differing regions are to be distinguished, and what their number, order, thickness, and qualifications may be, we are as yet ignorant, and shall, I fear, long continue so; for it is to be noted (with which observation I shall conclude) that what has been hitherto discoursed belongs only to the temper of those subterraneal parts, to which men have been enabled to reach by digging. It is true indeed, that some mines, especially in *Germany* and *Hungary*, are of a stupendous depth, in comparison of the generality of ours, and of the more obvious cavities of the earth; yet I find it boasted in a discourse, written purposely of the various mines in the world, that the rich mine at *Sueberg* is 400 yards deep: and they are scarce believed, that relate one Hungarian mine, which they visited to be 400 fathom; which, though double the depth of the former, reaches not to half a mile. But the deepest of all the mines, that I have as yet read or heard of from any credible relator, is that, which the experienced *Agricola*, in the tract he calls *Bermannus*, cap. 12. mentions to be at *Cotteberg*. But this itself, though it reach to above 500 fathom, that is, 3000 foot, yet this prodigious depth does not much exceed half a mile, and falls short of three quarters; * and how small a part is that of the whole depth of the terrestial globe? whose semidiameter, if we admit the recent account of the learned *Gassendus*, is reckoned at 4177 Italian miles; in comparison of which, as I was saying, how small a thing is a depth, that falls very short of a single mile?

* Licet variæ de ambitu terræ opiniones sint, nobis tamen propemodum constet, esse ipsam milliarium Italicorum 26255, quod in maximo ad terræ superficiem circulo respondeant uni gradui milliaria proximè 73. &c. Gassend. Instit. Astronom. lib. 2. cap. 13.

OF THE TEMPERATURE OF THE SUBMARINE REGIONS, As to HEAT and COLD.

CHAP. I.

THOUGH the Ariſtotelians, who believe water and air to be reciprocally tranſmutable, do thereby fancy an affinity between them, that I am not yet convinced of; yet I readily allow of ſo much affinity betwixt thoſe two fluid bodies, as invites me (after having treated of the temperature of the aerial regions) to ſay ſomething of that of the ſubmarine regions: which name of ſubmarine, though I know it may ſeem improper, I therefore ſcruple not to make uſe of, becauſe even among the generality of learned men, uſe has authorized the name of ſubterraneous places. For as theſe are not by this name, and indeed cannot in reaſon be ſuppoſed to be beneath the whole body of the earth, but only the ſuperficial parts of it; ſo by the appellation of ſubmarine regions it is not to be ſuppoſed, that the places ſo called are below the bottom of the ſea, but only below the ſurface of it.

BUT to come from words to things, I preſume it will not be expected, that I, that never pretended to be a diver, ſhould give of the regions, I am to treat of, an account built on my own obſervations; and I hope it may gratify a reaſonable curioſity about a ſubject, of which claſſick authors are ſo very ſilent, and about which philoſophers ſeem not ſo much as to have attempted any experiments (for want of opportunities and means to make them.) I offer the beſt information I could ſupply myſelf with, by purpoſely converſing with perſons, that have dived, ſome without, and ſome by the help of engines. To which I have added ſome reports, that I judge fit to be allowed, made me by perſons, that had converſed with the divers upon thoſe African and Indian-coaſts, where the moſt famous and expert are thought to be found.

AND I the rather report the anſwers and relations my inquiries procured, becauſe the informations they give us concern a ſubject conſiderable as well as vaſt, about which nevertheleſs I among many others am not in a condition to ſatisfy at all my curioſity by trials of my own making; and becauſe alſo, what I ſhall ſay will probably ſpoil the credit of the vulgar error, that in all deep water, of which the ſea is the chiefeſt, the lowermoſt are ſtill the warmeſt parts, unleſs in caſe that in ſome very hot climates, or ſeaſons, the ſuperficial ones happen to be a little warmed by the extraordinary or violent heat of the ſun.

CHAP. II.

THOUGH the air and the earth have been diſcriminated as to temperature, into three regions; yet the informations I have hitherto met with, invite me to aſſign to the ſea any more than two. The former of which may be ſuppoſed to reach from the ſuperficies of it, as far downwards, as the manifeſt operation of the variouſly reflected and refracted beams of the ſun, or other cauſes of warmth penetrate; from which to the bottom of the ſea, the other region may be ſuppoſed to extend.

ACCORDING to this diviſion, the limits of this upper region will not be always conſtant; for in the torrid zone, and other hotter climates, it will, *cæteris paribus*, be greater than in the frigid zone or in the temperate zones; and ſo it will be in ſummer than in winter; and in hot weather than in cold; ſuppoſing in theſe caſes the heat to come from the ſun and air, and not, as ſometimes it may do, from the ſubterraneal exhalations.

THE ſame cauſes are likewiſe proper, as it is manifeſt, to alter the temperature, as well as the bounds of this region; but this temperature may alſo be changed in ſome few places, by at leaſt two other cauſes; the one is the differing conſtitution of the ſoil, that compoſes the ſhore, which may affect the neighbouring water, if it do extraordinarily abound with nitre, looſely contexed marchaſites, or other ſubſtances capable conſiderably to encreaſe or leſſen the coldneſs of the water. Another, though unfrequent cauſe, may be the figure and ſituation of the leſs deep parts of the ſhore, which may in ſome ſort reverberate the heat, that proceeds from the ſun; and upon ſuch an account may either add to the warmth, or allay the coldneſs, that would elſe be found in the neighbouring water. For whatever the ſchools are wont to teach about the intereſt of the attrition of air in the heat produced by the ſun beams, I have elſewhere ſhewn by experiments, that thoſe beams may conſiderably operate upon bodies placed quite under water.

BESIDES theſe two caſes, that may occaſion exceptions to the general obſervation; I intimated by the words, *at leaſt*, that there might be others. Becauſe, to mention now but one example, though it ſeem probable from what I have elſewhere delivered concerning the ſubterraneal fires and heats, that may in ſome places

places be met with, even beneath the bottom of the ſea, that the phænomenon I am going to recite may be reduced to the cauſes newly intimated; yet I am not abſolutely certain, but that in this caſe, whereto ſome others may perhaps be found reſembling, ſome other cauſe than thoſe hitherto mentioned may produce or concur to the effect. The relation here meant is afforded us by the following paſſage, taken out of the voyage of Monſieur *de Monts*, into *New-France*, (whereof he went to be governor) where the relator thus recites his obſervation: About the eighteenth day of june we found the ſea-water during three days ſpace very warm, and by the ſame warmth our wine alſo was warm in the bottom of our ſhip; yet the air was no hotter than before. And the 21ſt of the ſaid month, quite contrary, we were two or three days ſo much compaſſed with miſts and cold, that we thought ourſelves to be in the month of january, and the water of the ſea was extreme cold; which continued with us, until we came upon the bank by reaſon of the ſaid miſts, which outwardly did procure this cold unto us. This effect he attributes to a kind of antiperiſtaſis in the following part of his narrative; which I ſhall not now either tranſcribe or examine.

CHAP. III.

AND thus much being briefly noted touching the upper region of the ſea, and the requiſite cautions (that may perhaps extend further than it) being premiſed; it remains, that I take notice of the temperature of the lower region, which, in one word, is cold; unleſs in ſome few places to be preſently mentioned. For water being in it's natural or moſt ordinary ſtate a liquor, whoſe parts are more ſlowly agitated than thoſe of men's organs of feeling, muſt be upon that account cold as to ſenſe; and conſequently it need not be ſtrange, that thoſe parts of the ſea, which are too remote to be ſenſibly agitated by the ſun-beams, or wrought upon by the warmth, which the air and upper parts of the earth may from other cauſes receive, ſhould be felt cold by thoſe that deſcend into it; unleſs in thoſe few places, where the coldneſs may be either expelled or allayed by hot ſprings, or ſubterreſtial exhalations, flowing or aſcending from the ſubjacent earth, or the lower parts of the ſhore, into the incumbent or adjacent parts of the water.

To juſtify my aſcribing of this coldueſs to the ſecond, or lower region of the ſea, I ſhall now ſubjoin ſome relations I procured from perſons, that had occaſion to go down into it, or otherwiſe take notice of its temperature in very differing regions of the world, and at very unequal depths.

AND firſt as to the temperature of the lower region in the northern ſea, I had the opportunity to converſe often, and ſometimes to oblige a man bold and curious enough, who for ſome years got the beſt part of his ſubſiſtence by deſcending to the bottom of the ſea in an engine, whoſe ſtructure I elſewhere deſcribe, to ſeek for, and recover goods loſt in ſhip-wrecked veſſels. This perſon I diligently examined about divers ſubmarine phænomena, about which his anſwers may be elſewhere met with. And as to the temperature of the lower parts of the ſea (the knowledge of which is that alone, that concerns us in this place) he ſeveral times complained to me of the coldneſs of the deep water, which kept him from being able to ſtay in it ſo long as he might have been put into a condition of doing by the goodneſs of his engine; for I remember, that he related to me, that he ſtaid once betwixt an hour or two, at a depth, that was no greater than 14 foot and a half upon the coaſt of *Sweden*, in a place, that was near the ſhore; and I afterwards learned, that he ſtaid much longer in a deeper place; (uſe having probably made the cold more ſupportable to him.) He told me then, that about two years before, he was engaged by a good reward to go down with his engine to the bottom of the ſea to fetch up ſome goods of value out of a ſhip, that had been caſt away there within about a miles diſtance from a very little iſland, and, if I miſtake not, about ſix miles from the ſhore. He further anſwered me, that though he felt it not at all cold on the ſurface of the water, (his attempt being made in june) yet about the depth of the ſhip, it was ſo very cold, that he felt it not ſo cold in *England's* winter and froſty weather. And he told me, that an exceſſive cold was there felt, not only by him, but by very ſturdy men, who invited by his example would needs alſo go down themſelves to participate and promote the hoped for diſcovery. He told me alſo, that the upper water did but cool and refreſh him; but the deeper he went, the colder he felt it, which is the more conſiderable, becauſe he had ſome times occaſion to ſtay at 10 fathoms or even 80 foot under water. And I ſince found, that he informed divers virtuoſi, that purpoſely conſulted him, that he found the coldneſs of the water encreaſe with its depth; and gave that for the reaſon, why he could not ſtay ſo many hours as otherwiſe he might, at the bottom of the ſea. Adding, that before his engine was well fitted, he was once ſo covered over with it, that he was forced to touch the ground with his hands and feet, and the neighbouring parts, to which he found a coldneſs communicated by the fundus he leaned upon; though the cloſeneſs of his diſordered engine made the other, and (whilſt he was in that poſture) upper parts of his body, of a very differing temper.

AN inquiſitive perſon of my acquaintance, that made a long ſtay in the *Northern America* (at about two or three and forty degrees of Latitude) and diverted himſelf often with ſwimming under water, anſwered me, that though he ſcarce remembered himſelf to have dived above two fathoms beneath the ſurface of the ſea; yet even at that ſmall depth, he obſerved the water to encreaſe in coldneſs, the lower he deſcended into it. Which argues, that though the ſun-beams do often penetrate plentifully enough to carry light to a great depth under water, yet they do not always carry with them a ſenſible heat; and that, at leaſt, in ſome places,

places, the upper region of the ſea reaches but a little way.

THE coldneſs of the climate in theſe weſtern parts of *Europe*, and the want of conſiderable inducements to invite men to dive often to any great depth into our ſeas, has kept me from being able to procure many obſervations about the temperature of their lower region; but upon the hotter coaſts of *Africk*, and the *Eaſt-Indies*, the frequent invitations men have to dive for coral, pearls, and other ſubmarine productions, have made it poſſible for me to get more numerous obſervations; ſome of which I ſhall now annex.

CHAP. IV.

MEETING with a perſon of quality, who had been preſent at the fiſhing of coral upon the ſhoar of *Africa*, and who was himſelf practiſed in diving, I inquired of him, whether he found the ſea upon the African coaſt to be much colder at a good depth, than nearer the ſurface; whereto he anſwered me, that though he had ſeldom dived above three or four fathoms deep, yet, at that depth, he found it ſo much colder than nearer the top of the water, that he could not well endure the coldneſs of it.

AND when I farther aſked him, whether, when he was let down to the bottom of the ſea, in a great diving bell (as he told me he had been) he felt it very cold, though the water could not come immediately to touch him; he replied, that when the bell came firſt to the ground, he found the air in it very cold, though after he had ſtaid a-while there, his breath and the ſteams of his body made him very hot.

THAT alſo at a greater depth in thoſe hotter climates, the ſea-water is ſenſibly cold, may be thus made probable: inquiring of a famous ſea-commander, who had been upon the African coaſt, to what depth he was wont to ſink his bottles to preſerve his wine any thing cool in that exceſſive hot climate, he anſwered me, that in the day time he kept it in a tolerable temper ſo as to be drinkable, by keeping it in the bottom of the ſhip, and in ſand; but in the morning he had it cool enough by ſinking his bottles over night into the ſea, and letting them hang all night at 20 or 30 fathom deep under water.

INQUIRING alſo of an intelligent gentleman, that was imployed to the river of *Gambra*, and ſailed up 700 miles in it, in a ſmall frigate, whether he had obſerved, that in the ſea, even of thoſe hot climates, wine may be preſerved cool; he told me, that it might, and, that by the means I hinted to him, which was to let down, when the ſhip came to an anchor in the evening, ſeveral bottles full of wine (they uſed that of *Madera*) exactly ſtopped to ten, twelve, or fourteen fathoms deep; whence being the next morning drawn up, they found the wine cool and freſh (as if the veſſels had been in theſe parts drawn up out of a well) provided it were preſently drank, for if that circumſtance were omitted, the heat of the air on the upper part of the water would quickly warm the liquor.

I remember too, that having met with a man of letters, that ſailed to the *Eaſt-Indies* in a Portugal-caraet, I learned by inquiry of him, that it was the practice in that great veſſel for the captain and other perſons of note, whilſt they paſs through the torrid zone, to keep their drink, whether wine or water, cool, by letting it down in bottles to the depth of 80, 90, and ſometimes an hundred fathom or better, and letting it ſtay there a competent time; after which, he told me, he found it to be exceeding cool and refreſhing.

LASTLY, to ſatisfy myſelf as far as I could, to how great a depth the coldneſs of the ſea reached; meeting an obſerving traveller, whoſe affairs or curioſity had carried him to divers parts, both *Eaſt* and *Weſt-Indies*, I inquired of him, whether he had taken notice of any extraordinary deep ſoundings in the vaſter ſeas, to which being anſwered, that ſome years ago, ſailing to the *Eaſt-Indies*, in a very great ſhip, over a place on the other ſide the line, that was ſuſpected to be very deep, they had the curioſity to let down 400 fathom of line, and found they needed no leſs. Whereupon I inquired of him, whether he had taken notice of the temperature of the ſounding lead as ſoon as it was drawn up: to which he told me, that he, and ſome others did; and that the lead, which was of the weight of about 30, or 35 lb. had received ſo intenſe a degree of coldneſs, as was very remarkable; inſomuch, that he thought, that if it had been a maſs of ice, it could not have more vehemently refrigerated his hands: and when I aſked in what climate this obſervation was made, he told me, it was in the antarctick hemiſphere, but at a great diſtance from the line. As indeed, I concluded by ſome circumſtances he mentioned to me, that it was about the 35th degree of ſouthern latitude.

CHAP. V.

THESE are the chief relations I have hitherto been able to procure about the temperature of the ſea; which, if they be ſo confirmed by others, as that we may conclude they will generally hold, it will not be irrational to conceive, that in reference to temperature, thoſe two fluids, air and water, may have this in common, that where their ſurfaces are contiguous, and in the neighbouring parts, they happen to be ſometimes cold, ſometimes hot, as the particles they conſiſt of chance to be more or leſs agitated by the variouſly reflected ſun-beams, or more or leſs affected by other cauſes of heat. But that part of the air, which they call the ſecond, and is ſuperior to the firſt, as alſo the lower region of the ſea, being more remote from the operation of thoſe cauſes, do retain their natural, or more undiſturbed temperature, which, as to us men, is a conſiderable degree of coldneſs, the agitation of their ſmall parts being uſually in thoſe regions much inferior to that of the ſpirits, blood, and other parts of our organs

organs of feeling. So that the regions of the water and air seem to answer one another, but in an inverted order of situation; and the analogy might perhaps be carried further, if I had time and opportunity to do it in this place. And here I shall not dissemble, that I was somewhat perplexed by meeting with a traveller, that had visited the East-indian coast, near the famous Cape of *Comory*: for asking him some questions touching the neighbouring sea, I gathered from his discourse, that he concluded from that of some divers, that the sea near *Ceylon* was warmer at the bottom than at the top. And when I thereupon asked him, whether this happened not in their winter, he replied, that it was indeed winter, though not with us, yet with them. It occurred indeed to my thoughts on this occasion, that perhaps in a part of the torrid zone so near the line as about 80 degrees, if the sea were not of a considerable depth, the heat of the two not far distant shores of *Coromandel* and *Ceylon* might have no small influence upon the temperature of the water. I considered also, which did not a little weigh with me, that in divers parts of the *East-Indies*, and even in a region bordering upon *Coromandel*, where an ingenious acquaintance of mine lived some years, it has been observed, that winter and summer are not so much discriminated by cold weather and hot, as by very rainy weather and very dry. Nay, in some places the sultry heat of the climate is more complained of, in what they call their winter than their summer. So that there will be no necessity to recur to an antiperistasis occasioned by the coldness of the winter. I thought too, that it may perhaps be without absurdity suspected, that as the bottom of the sea in this place had a peculiar constitution, that fitted it more than others for the copious production of pearls; so there might be some peculiarity in the nature of the subjacent soil, or there may be some subterraneal fire or heat beneath it, which may occasion an unusual warmth in that part of the sea, by which cherishing warmth, perhaps, such abundance of shell fishes teeming with pearls may be invited to settle there, rather than in any of the neighbouring places. But with all these conjectures I should not have been so well satisfied, as with the answer I afterwards obtained by a gentleman, whose curiosity had carried him to be an assiduous spectator of the famous pearl-fishing, near the island of *Manar*, between that and the coast of *Coromandel*, which reaches near, if not fully to the Cape of *Comory*. For this person having had much conversation with the divers for pearls, not only learned from them, that they found the water very sensibly cold at the bottom, which in some places he estimated to be 80 or 100 fathom deep; but observed divers of them at their return to the boats, to be ready to shake with cold, and hasten to the fires, that were kept ready for them in little cabbins upon the shore: which relation being accompanied with divers circumstances of credibility, and arguing, the person that made it to have been acquainted with the report above-mentioned, and had met with some, that had dived in the place whereto it had relation, made me conclude, that as to that report, something extraordinary had happened in that place; or, that there was some mistake of him to whom it was made; or, that divers did not descend to a sufficiently considerable depth.

If I had been furnished with opportunity, I would have engaged some ingenious navigators to examine the temperature of the submarine regions, both of differing seasons of the year, especially the hottest part of summer, and coldness of winter, and with hermetically sealed weather-glasses, in order to the discovery of such particulars as these, whether there be in some seas any such varying differences of temperature, as may invite us at least in some places, to make more than two submarine regions: whether the submarine coldness do at the bottom of the sea, or elsewhere, either equal or surpass that degree, which we here find sufficient to freeze common water: whether the parts of the sea-water are still the colder, as they are the deeper: and whether or no this increase of coldness be regular enough to be reducible to any settled proportion. But for the resolving of these and the like questions, I did not causelesly intimate, that a sealed weather-glass was to be employed; for I take a common one to be altogether unfit for such purposes, not only because the sea-water would mingle with such liquors as are wont to be employed in it, for that inconveniency I could easily remedy, by substituting, as I have several times done in other cases, mercury instead of ordinary liquors; but chiefly, because the incumbent sea water would gravitate upon the restagnant liquor of the weather-glass, and thereby render its informations false or uncertain. According to what I have had occasion to observe in another tract.

Whereto, that there may not in this place be any need to recur, I shall add a slight experiment, that I made for the satisfaction of some ingenious men not well acquainted with hydrostaticks, or not rightly principled in them. And this trial I shall the rather mention, because many will not allow water to press upon mercury immersed therein, this being a far more ponderous liquor than that; and others will expect, that the included air, having no place to escape out at, should resist the ascension of the subjacent mercury, more than indeed it will. We made then a small weather-glass differing from common ones, besides the bigness, in that it was furnished with mercury instead of water; and in that we employed to contain the stagnant mercury a glass vial with a narrow neck, wherein, by a piece of cork or two, the stem of the glass ball was well fastened that this globular part of the instrument might not be lifted up when it was under water. Then having by applying cold water to the outside of the ball endeavoured to reduce the air to the same temper with the water, or at least to an approaching degree of coldness; and having taken notice of the station of the mercury in the shank or stem above-mentioned, we did, by strings tied about the neck of the

 small

ſmall vial, let the inſtrument gently down into a large tall glaſs body, filled with fair water, that the liquor and veſſel being both tranſparent, we might eaſily perceive the motions of the mercury in the ſlender pipe. By which means it appeared, that, as the thermometer deſcended deeper and deeper into the water, the mercury was preſſed up higher and higher in the ſtem. And that it may not be ſuſpected, that this aſcenſion proceeded only, or chiefly, from the refrigeration of the air by the water, I ſhall add to what I have juſt now noted, that though the coldneſs of the water may well be ſuppoſed uniform, as at leaſt to ſenſe; yet the whole inſtrument being leiſurely removed ſometimes to the upper ſurface of the water, ſometimes to the lower, the riſing and falling of the quickſilver in the ſlender pipe was ſuitable to the depth of its ſurface, or its diſtance beneath that of the water. (The like experiment we might have tried with a thermoſcope furniſhed with water, and let into oil, or with deliquated ſalt of tartar and pure ſpirit of wine inſtead of mercury and water; if we had been furniſhed with ſufficient quantities of thoſe liquors, and had judged it to be requiſite.) But this circumſtance I thought fit to admoniſh the ſpectators of, that it is not to be expected, that the mercury ſhould riſe as much in proportion, when it is (for example) a foot under water, as when it is but two or three inches; becauſe, according as the inſtrument is let down deeper, and the air crouded into a leſs room, the ſpring of that compreſſed air becomes the ſtronger, and makes the more reſiſtance. Which advertiſement agreed well with the experiment, whoſe other phænomena I paſs over as not pertinent to this place, where I would only juſtify what I ſaid of the unfitneſs of weather-glaſſes made, (though with other liquors) after the common ways for making the ſubmarine trials I propoſed.

BUT till ſuch artificial obſervations can be obtained, we may from what has been above delivered probably gather, that though the lowermoſt of the ſubmarine regions be very ſenſibly cold, yet water, at leaſt that of the ſea, does not by theſe phænomena appear to be the *ſummum frigidum*. Though I have been ſeveral times able to produce ice in ſalt-water, yet I find not by any obſervation, that there has been ice met with, and generated at the bottom of the ſea, under which the earth has been found unfrozen by our divers; and appears to be ſoft at depths exceedingly ſurpaſſing the greateſt they have reached; as is evident by the mud, gravel, &c. fetched from the bottom of the ſea by ſounding plummets, let down to 80 or 100 fathom, or even a greater depth, whereof examples may be met with in the journals of navigators: nay, my curioſity procured me this account, from the ſober commander of a ſhip, that came this year from the remoter parts of the great ocean, that at about 35 degrees of ſouthern latitude, the tallow, with which his ſounding lead was anointed, brought him up grey ſand from the immenſe depth of no leſs than two hundred and twenty fathom. But to this obſervation it is juſt to annex this caution, that we cannot ſafely conclude from men's finding no ice at the bottom of the ſea, that the cold there cannot be very intenſe; for, as I have found by more than one relation (*elſewhere recited) that, whatever the ſchools ſurmiſe, the ſea is at leaſt as ſalt at the bottom, as at the top; ſo I have more than once tried, that ſalt water will without freezing admit a much greater degree of cold than is neceſſary to turn freſh water into ice.

* Notes about the ſaltneſs of the ſea.

RELATIONS ABOUT THE BOTTOM of the SEA.

SECTION. I.

I DO not pretend to have vifited the bottom of the fea; but fince none of the naturalifts, whofe writings I have yet met with, have been there any more than I; and it is great rarity in thofe cold parts of *Europe* to meet with any men at all, that have had at once the boldnefs, the occafion, the opportunity, and the fkill, to penetrate into thofe concealed and dangerous receffes of nature, much lefs to make any ftay there; I prefume it will not be unpleafant, if about a fubject, of which, though none of thofe very few naturalifts, that write any thing at all, write otherwife than by hear-fay, I recite in this place, what I have learned by enquiry from thofe perfons, that among the many navigators and travellers I have had opportunity to converfe with, were the likelieft to give me good information about thefe matters.

It would be needlefs here to take notice, that the fea is ufually cold and falt at the bottom; nor to repeat thofe other things, that I have already delivered in other difcourfes. I fhall therefore begin what I have to fay in this, by relating, that one of the chief things, that I was folicitous to enquire after about the bottom of the fea, was, the inequality I fuppofed to be in the foil. For though the furface of the fea, when it is not agitated by the winds, appears very plain and level, and though it be indeed, at leaft in this, or that particular fea, fphærical and (phyfically fpeaking) concentrical to the earth; yet I could not think it probable, for reafons not neceffary to be here difcourfed of, that the bottom, the fuperficies of the ground, or of the veffel, that contained it, fhould be either flat or level, or regularly concave.

To fatisfy myfelf about this matter, I enquired of a perfon, that had vifited the famous pearl-fifhing at the little ifland of *Manar* (near the rich ifle of *Ceylon*) in the *Eaft-Indies*, and had by his ftay there much opportunity to fee divers at their work, and converfe with them. By the anfwers of this man, who was a fcholar, I learned, that the divers had affured him, that they found the floor of the fea, if I may fo call it, in divers places, exceedingly unequal, in fome places being flat, in others afperated with crabby rocks a confiderable height, and elfewhere finking into precipitous depths, in which they found it very cold.

Besides the recited teftimonies of the divers, I enquired of feveral pilots and other navigators, that had made long voyages, what gradual or abrupt inequality they had obferved at their foundings in very neighbouring places; it being eafy to be gathered from thence, whether the fea were there uniformly deep, or did at leaft, with fome regularity, alter its depth by degrees; or whether, as I fufpected, there were not at the bottom of the fea, hilly places, and fteep præcipices, and perhaps deep vallies or wells, as we obferve in the difcovered part of the terreftrial globe.

By thefe inquiries, I obtained feveral obfervations, whereof the moft material are thofe that follow:

First, an ancient fea-commander, that had many years frequented *Africa* and the *Indies*, told me, as others had done before, that when they failed in the ocean very far from fight of land, they did not often put themfelves to the trouble of founding; but that as far as they had founded, he had ufually found the depth of the fea, to increafe or decreafe gradually, without very great irregularities, excepting fome places, inftancing particularly in the excavation, that makes the bottom of the fea, within fight of the *Cape of Good Hope*, where though for the moft part, he found the water to deepen more and more, as he failed farther from fhore; yet in one place, he and others had met with a bank (as he conceived it to be) at a confiderable diftance from the furface of the water. So, that though when they were as they imagined near the edge of that bank, they found but a moderate number of fathoms, yet when failing a very little way farther, they had gone beyond it, they found the fea of an immenfe depth. In fhort, I gathered from his anfwers, that in the greater feas, he had found, for the moft part, the ground at the bottom, to fall away by degrees; but nearer the fhores, that is, within a moderate number of leagues, he obferved in divers places, that the fubmarine ground was very unequal, and had as it were, hills and præcipices.

A man of letters, that had failed both to the *Eaft* and *Weft-Indies*, and in divers other regions befides, and had made fome of his voyages in fhips of fuch great burthen, as obliged the mariners to be very frequent and careful in founding, informed me, that fometimes at confiderable diftances from fhore, he had obferved the fea to be 20, 30, or perhaps 40 fathom deeper when they caft the founding lead from one fide of the fhip, than it had been juft before, when they had founded from the other; and from other things, that he told me, I found myself much confirmed in the above proposed opinion.

Hearing of a sea-captain, of extraordinary skill in maritime affairs, that was come home this year from *East-India*, his reputation made me endeavour to have a little conference with him about the subject of this discourse; but his occasions hastening him to another place, before I could send to him, I procured from the chief persons, that employed him, a sight of some notes touching his last voyage, which he had left with them; hoping to find there something at least about the soundings of so accurate a seaman; and accordingly I met with a passage, very pertinent to my purpose, and worthy to be here transcribed.

February 12. After our observation, (he means a former one very agreeable to this) seeing the ground under us, we heaved the lead, and had but 19 fathom rocky ground, then haled by N. N. E. the wind at N. W. and found out water to shoal from 19 to 10 and 8 fathom hard coral ground, then suddenly deepened again from 8 to 20 and 22 fathom sandy ground, and then suddenly saw rocks under us, where we had but 7 fathom, and the next cast 14 fathom again. And so having run N. N. E. from 6 in the morning until 12 at noon about 19 mile, we deepened our water, from 16 to 25, and the next cast, no ground with 35 fathom of line.

Lastly, having opportunely met with an ancient navigator, who passes for the most experienced pilot in our nation for an East-Indian voyage; I asked him about his own observations concerning these unequal sounding, I was answered, that he had not only met with them elsewhere, but, that not far from the mouth of our channel, he had sometimes found the bottom of the sea so abrupt, that in sailing twice the length of the ship, he had found the water deepen from 30 fathom to a hundred, if not also much more.

Since I received these relations, having the honour to discourse with a noble person, who has divers times deservedly had the command of English fleets, and is no less curious than intelligent in maritime affairs, I took the opportunity to inquire of his lordship, whether he had not observed the bottom of the sea to be very unequal in neighbouring places? To which he replied, that he had found it exceedingly so. And to satisfy me, that he spoke not upon meer conjecture, he told me, that sailing once with his fleet, even in our channel, he perceived the water to make a ripling noise (as the seamen call it) as the *Thames* does under *London-Bridge*. So that he was afraid they were falling upon some shoal, the water being 12 or 14 fathom deep, and going on a little farther, he cast out the plummer again, and found it about 30 fathom. He added, that he made divers such observations, but took notice of such ripling waters, only when the tide was ebbing: and yet in a deep sea meeting with the like appearance in the upper part of the water, and thinking it improbable, that there should be any shoal there, he ordered the depth to be sounded, and found it to exceed 30 fathoms; and after he had passed on a very little farther, he found the sea so deep, that he could not fathom it with his ordinary line.

SECTION. II.

Another thing observed at the bottom of the sea is the great pressure of the water there against other bodies. For what ever men may philosophize in their studies, and may conclude from the principles, that are generally received about the non-gravitation of water in its proper place, yet experience seems very little to favour that general doctrine.

For first I remember, that having caused a pretty large cylinder of glass, that was open only at one end, to be so depressed into a large glass-vessel full of water, with a conveniently applied weight of lead, that none of the air could get out, I could easily discern through the liquor and vessels, which were all transparent, that as the inverted cylinder descended deeper and deeper, the external water compressed the imprisoned air, and ascended higher and higher in the cavity of the cylinder, against whose side we had beforehand placed a row of marks, whereby to take notice of the gradual ascent and descent of the internal water.

Secondly, having inquired of two several observing persons, whereof one had with a diving engine visited the bottom of the sea in a cold northern region; and the other had done the like in an engine much of the same sort, upon the coast of Africk; I found their relations to agree in this, that the deeper they descended into the sea, the more the air they carried down with them was compressed, and the higher the water ascended above the lip, or brim of the engine into the cavity of it.

But I shall now add a more considerable experiment or two, to the same purpose. For discoursing one day with an engineer of my acquaintance, that had been often at sea, and loved to try conclusions, of a way I had thought of, to make some estimate of the pressure of the water at a considerable depth beneath the surface, and shew, that the pressure is great there; he told me, he could save me the labour of some trials by those he had made already, and assured me, that having divers times opportunity to sail near the streights mouth, over a place where the sea was observed to be of a notable depth, he had found, that if he had let down, with a weight into the sea, not a strong round glass bottle, but a vial, such as the seamen use to carry their brandy and strong waters in; such a vessel, which might contain a pint or quart of water, would, when it come to be sunk 40 fathom under water, if not sooner, be so oppressed, by the pressure of the incumbent, and lateral water, as to be thereby broken to pieces.

He also averred to me, that having exactly closed an æolipile of metal, and with a competent weight, sunk it to a great depth in the sea, as to forty, fifty, or sixty fathom deep, when he pulled it up again, he found to his wonder, that the great pressure of the water had in divers

vers places crushed it inwards. And though I had some suspicion, that the coldness of the sea, at such a depth, might, by weakening the spring of the included air, something contribute to the effect, yet I did not admire the event, having divers years before had a thin æolipile of copper crushed inwards by the pressure of a much lighter fluid than sea-water.

SECTION III.

ANOTHER thing observed in the bottom of the sea is the tranquillity of the water there, if it be considerably distant from the surface. For though the winds have power to produce vast waves in that upper part of the sea, that is exposed to their violence; yet the vehement agitation diminishes by degrees, as the parts of the sea, by being deeper and deeper lie more and more remote from the superficies of the water. So that the calm being less and less disturbed towards the bottom of the water, if that lie considerably deep, the water is there either calm, or scarce sensibly disturbed.

BUT that is for the most part to be understood of places at some distance from the shore; for oftentimes, in those that are too near it, the progress of the waters being rudely checked, and other cicumstances concurring, the commotion of the water is so great, that it reaches to the very bottom, as may appear by the heaps of sand, the amber, and, in some places, the stones, that are wont to be thrown up by the sea, in and after storms.

THE above mentioned calmness of the sea at the bottom will (I doubt not) appear strange to many, who admiring the force of stormy winds, and the vastness of the waves they raise, do not, at the same time, consider the almost incomparably greater quantity, and weight of water that must be moved, to make any great commotion at the bottom of the sea, upon which so great a mass of salt water, which is heavier than fresh, is constantly incumbent. Wherefore for the proof of the proposed paradox, I will here set down a memorable relation, which my inquiries got me from the diver, elsewhere mentioned, who by the help of an engine could stay some hours under water.

THIS person then being asked, whether he observed any operation of the winds at the bottom of the sea, where it was of any considerable depth? answered me to this purpose, that the wind being stiff, so that the waves were manifestly six or seven foot high above the surface of the water, he found no sign of it at 15 fathom deep; but if the blasts continued long, then it moved the mud at the bottom, and made the water thick and dark. And I remember he told me, which was the circumstance I chiefly designed, that staying once at the bottom of the sea very long, where it was considerably deep, he was amazed at his return to the upper parts of the water, to find a storm there, which he dreamt not of, and which was raised in his absence, having taken no notice of it below, and having left the sea calm enough when he descended into it.

FOR farther confirmation, I shall add, that having inquired of a great traveller, who had assisted at a rich pearl-fishing in *East Indies*, whether he had not learned by his conversation with the divers, that storms reach not to the bottom of the sea, if it be of any considerable depth; he answered, that he had seen the divers take the water, when the sea was so very rough, that scarce any vessels would hazard themselves out of ports; that those returning divers told him, that at the bottom they had found no disturbance of the water at all. Which is the more considerable, because of the situation of that place where they dive for pearls; for this is near the shore of *Manar*, and that it self is seated between the great island of *Ceylon*, and the vast cape of *Comori*; and though it may be much nearer the former, is not yet far distant from the latter. Which situation and the neighbourhood of the vast Indian ocean, on the one side of *Ceylon*, and the great gulph of *Bengala*, (antiently *Sinus Gangeticus*) on the other, makes the place, where the pearls are fished for, exceeding likely to be subject to very troubled seas.

IT will perhaps be thought no slight addition to the fore-going arguments, if I here add, that meeting one day with an ancient and expert seaman, whom his merit had advanced to considerable employments in his profession, I was confirmed by the enquiries I made of him, not only in the opinion I had about the calmness of the bottom of the sea, but also, that the operation of good gales of wind, does oftentimes not reach to near so considerable depths into the sea, as hath been hitherto supposed, even by navigators themselves. For he assured me, that having sometimes sailed in great ships, that drew much water, as about 12 or 15 foot, he had dived to the keel of the ships, when they were under sail, and observed the agitations of the water to be exceedingly diminished, and grown very languid, even at that small distance, from the upper part of the waves. And he farther answered, that when in *America* he learned to dive of the Indians, they taught him by their examples, to creep along by the rocks and great stones, that lay near the shore, at the bottom of the water, to shelter themselves from the strokes, and other ill effects of the billows, which near the shore, and where the sea was so shallow, as it was there, did often hurt and endanger swimmers and unskilful divers. But when they were, by this means, got farther from shore and into deeper water, they would securely leave the shelter, they had till then then made use of, and swim within a few yards of the surface of the and commotions of the upper parts of the water.

BUT lastly, for further satisfaction, I had the opportunity to make inquiry about this matter of a great sea commander, who has both an extraordinary curiosity to make marine observations, and an unusual care in making of them accurately, I found the opinion countenanced by his answer, which was in short; that he had lately been at a place, where the sea was often tempestuous enough, and that they found by a sure mark, that the storm did not reach with any efficacy four fathom beneath the surface of the water.

About the tranquillity of the lower parts of very deep waters I had a ſuſpicion, which, though I fear it might ſeem ſomewhat extravagant, becauſe I have not met with it in authors; yet I thought it worth examining for the uſe it might be of, if reſolved, in reference to the ebbing and flowing of the ſea.

I made therefore a ſolicitous inquiry, whether the tides did reach to, or near the bottom of the deeper ſeas, but found it exceeding difficult, by reaſon of men's want of curioſity to obtain any ſatisfaction about a problem, that moſt navigators I have converſed with did not ſeem to have ſo much as dreamed of. But thus much I found indeed, by inquiring of an engineer, who was curious of marine obſervations, that a famous ſea-commander of his his acquaintance, being alſo a great mathematician, had affirmed to this relator, that he had divers times obſerved, that when he let down his plummet to a great depth, but yet not to reach ground, it would be quickly carried by a motion quite contrary to that of the ſhallop, whence they ſounded, and very much quicker than it; but I had this only at ſecond hand. Alſo, if I miſ-remember not, I was informed by a ſkilful obſerver, that commanded many of our Engliſh men of war, that he had, near the Sound, obſerved the upper and lower parts of the water to move with a conſiderable ſwiftneſs quite different ways; but not having committed this relation to writing, I dare not build much upon it. And among the anſwers I had received, and written down concerning thoſe matters, all that I can yet find among my adverſaria is a relation, which though ſingle, will not be unworthy to be tranſcribed in this place, becauſe the perſon who gave it me, is one of the ancienteſt and moſt experienced pilots of our nation.

This perſon therefore aſſured me, that ſailing beyond the *Cape of Good Hope*, into the *South-Seas*, he made trials of the motion of the upper part of the water above the lower, where ſometimes caſting out a large and heavy plummet, he let it down to ſeveral depths ſhort of 50 fathom, without any ſenſible operation upon the motion of the boat, or ſhallop, he ſtood in to make the trial; but when he let down the plummet lower, to about an hundred fathom or more, then he found, that though the plummet reached not to the bottom of the water, yet upon the ſcore of the ſtanding water beneath, the ſuperior water would make the boat turn towards the tide or current, as if it lay at anchor, and the water would run by the ſide of the boat, at the rate of about three miles an hour: *thus far this diligent obſerver.* But how far the inequality of the ſoil at the bottom of the ſea, and how far various depth of the water, and ſome other circumſtances, may alter the caſe, and make it hard to determine, what ought to be aſcribed to tides, and what to currents, are things, which I will by no means be poſitive in, till I can meet with further information.

[Since the writing of this, happening to meet with one, that ſpent ſome time at a famous eaſtern pearl-fiſhing, and aſked him, whether he had inquired of the divers about the problem lately propoſed, and whether the ſea were there deep enough to make obſervations of that kind: to the latter part of which queſtion he replied, that in ſome places it was of a very conſiderable depth, and fit to make the obſervation in; and to the former he anſwered, that he had inquired of the divers, who affirmed to him, that ſometimes at the bottom of the deep waters, there ſeemed to be a ſtagnation of the Sea for a great depth, ſo that till ſuch a height they could riſe directly upwards, but that at other heights, they would be carried away by the leſs deep waters; ſo as to be found, when they came to emerge a great way off from that point of the ſurface, which was perpendicular to that place at the bottom, whence they began to aſcend.]

NEW PNEUMATICAL EXPERIMENTS ABOUT RESPIRATION.

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TITLE I.

Obſervations made about the laſting of ducks included in the exhauſting receiver.

NATURE having, as Zoologiſts teach us, furniſhed ducks and other water fowl with a peculiar ſtructure of ſome veſſels about the heart, to enable them, when they have occaſion to dive, to forbear for a pretty while reſpiring under water without prejudice; I thought it worth the trial, whether ſuch birds would much better than other animals endure the abſence of the air in our exhauſted receiver. The accounts of, which trials were, when they were made, regiſtered as follows.

EXPERIMENT I.

WE put a full grown duck (being not then able to procure a fitter) into a receiver, whereof ſhe filled, by our gueſs, a third part, or ſomewhat more, but was not able to ſtand in any eaſy poſture in it: then pumping out the air, though ſhe ſeemed at firſt (which yet I am not too confident of, upon a ſingle trial,) to continue well ſomewhat longer than a hen in her condition would have done; yet within the ſhort ſpace of one minute ſhe appeared much diſcompoſed, and between that and the ſecond minute, her ſtrugling and convulſive motions increaſed ſo much, that, her head alſo hanging careleſly down, ſhe ſeemed to be juſt at the point of death; from which we preſently reſcued her by letting in the air upon her: ſo that this duck being reduced in our receiver, to a graſping condition, within leſs than two minutes, it did not appear, that, notwithſtanding the peculiar contrivance of nature, to enable theſe water birds to continue without reſpiration for ſome time under water, this duck was able to hold out conſiderably longer than a hen, or other bird not aquatick, might have done: and to manifeſt, that it was not cloſeneſs and narrowneſs of the veſſel, in reference to ſo bulky an animal, that produced in the ſubject of our trial the great and ſudden change above-recited, we ſoon after included the ſame bird in the ſame receiver, and having by a ſpecial way cemented it on very cloſe, we ſuffered her to ſtay thus ſhut up with the air for five times as long as formerly (by our gueſs, helped by a watch) without perceiving her to be diſcompoſed; and ſhe would probably have continued longer in the ſame condition, if my patience and leiſure would have held out ſo long, as ſhe could have done in that priſon.

EXPERIMENT II.

HAVING at the ſeaſon of the year procured a duckling, that was yet callow, we conveyed her into the ſame receiver, wherein the former had been included, and obſerved, that, though for a while ſhe appeared not much diſquieted, whilſt the air was pumping out of the glaſs, yet before the firſt minute was quite ended, ſhe gave manifeſt tokens of being much diſordered; and the operation being continued a while longer, ſhe grew ſo much worſe, that ſeveral convulſive motions, ſhe fell into before a ſecond minute was expired, obliged us to let in the air upon her, whereby ſhe quickly recovered,

N. B. I determine not, whether it be proper in this place to add, that when the receiver was pretty well exhauſted, the included bird appeared to the ſpectators manifeſtly bigger, than before the air was withdrawn, eſpecially about the crop, though that was very turgid before. And to manifeſt, that in this duck, as in the former, the convulſions, that uſed to be immediately followed by death, proceeded from the withdrawing of the ambient air, and not from the clogging of it; we kept the ſame duckling in the ſame receiver very cloſe, to keep out all external air, and to keep in the excrementitious ſteams of her body for above 6 minutes, without perceiving her to grow ſick upon her impriſonment; which yet laſted above thrice the time, that ſufficed to reduce her in the abſence of the air to a gaſping condition.

N. B. IT not being intended, that ducks and other water fowl, ſhould any more than other birds, live in an exceeding rarified air, but only be able to continue upon occaſion a pretty while under water, it may ſuffice, that the contrivance of thoſe parts, which relate to reſpiration, be ſo far fitted for the purpoſe, as we ſhall ſee it is, when we come to the tenth title.

TITLE

TITLE II.

Of the phænomena afforded by vipers included in an exhausted receiver.

CONSIDERING, that vipers are animals endowed with lungs (though of a different ſtructure from thoſe of men, dogs, cats, and birds, &c.) and that their blood is, as to ſenſe, actually cold; I thought it might, upon both theſe accounts, be very well worth trying, what effect the withdrawing and abſence of the air would have upon animals ſo conſtituted. I therefore made divers trials, ſome of which did not diſpleaſe me; but I know not by what misfortune the memorials of them were loſt, except two or three, which were not perfect, that I ſhall here ſubjoin.

EXPERIMENT I.

Jan. 2, 166⅔. WE included a viper in a ſmall receiver, and as we drew out the air, ſhe began to ſwell, and afforded us theſe phænomena.

1. IT was a good while after we had left pumping, before the viper began to ſwell ſo much as to be forced to gape, which afterwards ſhe did.

2. THAT ſhe continued, by our eſtimate, above two hours and half in the exhauſted receiver, without giving clear proof of her being killed.

3. THAT after ſhe was once ſo ſwelled, as to be compelled to open her jaws, ſhe appeared ſlender and lank again; and yet very ſoon after appeared ſwelled again, and had her jaws disjoined as before.

EXPERIMENT II.

WE took a viper, and including her in the greateſt ſort of ſmall receivers, we emptied the glaſs very carefully, and the viper moved up and down within, as if it were to ſeek for air, and after a while, foamed a little at the mouth, and left off that foam ſticking to the inſide of the glaſs: her body ſwelled not conſiderably, and her neck leſs, till a pretty while after we had left pumping; but afterwards the body and neck grew prodigiouſly tumid, and a bliſter appeared upon the back. An hour and an half after the exhauſtion of the receiver, which we then by trial found to be pretty ſtaunch, the diſtended viper, did give by motion manifeſt ſigns of life; but we obſerved none afterwards. The tumor reached to the neck, but did not ſeem much to ſwell the under-chap, both the neck, and a great part of the throat, being held betwixt the eye and the candle, were tranſparent enough, where the ſcales did not darken them. The jaws remained mightily opened, and ſomewhat diſtorted; the epiglottis with the rimula laryngis, which remained gaping, was protruded almoſt to the farther end of the nether-chap. As it were from beneath this epiglottis came the black tongue, and reached beyond it, but ſeemed by its poſture not to have any life, and the mouth alſo was grown blackiſh within: but the air being re-admitted after 23 hours in all, the viper's mouth was preſently cloſed, though ſoon after it was opened again, and continued long ſo; and ſcorching or pinching the tail made a motion in the whole body, that argued ſome life.

EXPERIMENT III.

To theſe experiments upon vipers I ſhall add one, made upon an ordinary harmleſs ſnake. April 25.

WE included ſuch an animal, together with a gage, in a pretty portable receiver, which, being exhauſted, and well ſecured againſt the ingreſs of the air, was laid aſide in a quiet place, where it continued from 10 or 11 of the clock in the forenoon, till about nine the next morning; and then my occaſions calling me abroad, I looked upon the ſnake, which, though he ſeemed to be dead, and gave no ſigns of life upon the ſhaking of the receiver; yet, upon holding the glaſs a convenient diſtance from a moderate fire, he did in a ſhort time manifeſt himſelf to be alive by ſeveral tokens, and even by putting forth his forked tongue. In that condition I left him, and, by reaſon of ſeveral avocations, came not to look upon him again till the next day early in the afternoon; at which time he was grown paſt recovery, and his jaws, which were formerly ſhut, gaped exceeding wide, as if they been ſtretched open by ſome external violence.

TITLE III.

Of the phænomena afforded by frogs in an exhauſted receiver.

THE ſame conſiderations, that induced me to make ſeveral trials upon vipers, did alſo invite me to make ſeveral upon frogs; the ſucceſs of ſome of which the following notes will declare. Sept. 9, 1662.

EXPERIMENT. I.

WE took a large luſty frog, and having included her in a ſmall receiver, we drew out the air, and left her not very much ſwelled, and able to move her throat from time to time, though not ſo faſt, as when ſhe freely breathed from the exſuction of the air. She continued alive about two hours, that we took notice of, ſometimes removing from the one ſide of the receiver to the other; but ſhe ſwelled more than before, and did not appear by any motion of her throat, or thorax, to exerciſe reſpiration, but her head was not very much ſwelled, nor her mouth forced open. After ſhe had remained there ſomewhat above three hours, (for it was not three half hours) perceiving no ſign of life in her, we let in the air upon her, with which the formerly tumid body ſhrunk very much, but ſeemed not to have any other change wrought in it; and though we took her out of the receiver, yet in the free air itſelf, ſhe continued to appear ſtark dead. Neverthelеſs, to ſee the utmoſt of the experiment, having cauſed her to be laid upon the graſs in a garden all night, the next morning we found her perfectly alive again.

EXPERIMENT II.

June 29, 1660. ABOUT 11 of the clock in the forenoon, we put a frog into a ſmall receiver, containing about 15¼ ounce troy weight of water, out of which we had tolerably well drawn the air, (ſo that when we turned the cock under water, it ſucked in about 13¾ ounce of water:) the frog continued in it (the receiver all the while under water) lively enough until about 5 of the clock in the afternoon, when it expired. The frog at the firſt ſeemed not to be much altered by the exſuction of the air, but continued breathing both with her throat and lungs.

EXPERIMENT III.

Sept. 6, 1662. WE included into a pretty large receiver a couple of frogs newly taken, the one not above an inch long, and proportionally ſlender; the other very large and luſty. Whilſt the air was drawing out, the leſſer frog ſkipped up and down very lively, and, ſomewhat to our wonder, clambered up ſeveral times to the ſides of the receiver, inſomuch, that he ſometimes reſted himſelf againſt the ſide of the glaſs. When his body ſeemed to be perpendicular to the horizon, if not in a reclining poſture, he continued to ſkip up and down a while after the exſuction of the air; but within a quarter of an hour (meaſured by a minute watch) we perceived him to lie ſtark dead with his belly upwards. The other frog, that was very large and ſtrong, though he began to ſwell much upon the withdrawing of the air, and ſeemed to be diſtreſſed, by his frequently leaping up after the air was drawn out, which he did not before, yet being; as we ſaid, very luſty, he held out half an hour, at which time it was remarkable, that the receiver, though it had held out againſt the preſſure of the outward air, during, that ſpace of time, notwithſtanding, that a piece of it had been cracked out, and was mended, with a cloth deeped in cement, yet at the end of the half hour, the weight of the outward air ſuddenly beat it in, and thereby brought the impriſoned frog a reprieve, which hindered us from bringing the experiment to an iſſue.

EXPERIMENT IV.

Sept. 11. WE took a ſmall frog, and having conveyed her into a very ſmall portable receiver, we began to pump out the air. At firſt ſhe was lively enough, but when the air began to be conſiderably withdrawn, ſhe appeared to be very much diſquieted (leaping ſometimes after an odd manner, as it were, to get out of the uneaſy priſon, but yet not ſo, but, that after the operation was ended, and the reciver taken off, the frog was perfectly alive, and continued to appear ſo (if I am not miſtaken) near an hour, though the abdomen was very much, and the throat ſomewhat extended; this latter part having alſo left, that wonted panting motion, that is ſuppoſed to argue and accompany the reſpiration of frogs. At the end of about three quarters of an hour, after the removal of the receiver from the pump, the air was let in; whereupon the abdomen, which by that time was ſtrangely ſwelled, did not only ſubſide, but ſeemed to have a great cavity in it, as the throat alſo proportionably had; which cavities continued, the frog being gone paſt all recovery.

EXPERIMENT V.

A large frog was conveyed into a plated receiver, and the air being withdrawn, her body by degrees was diſtended; as appeared very notably, when by a caſual ſpringing of a leak, the air got in again, and made her look much more lank and hollow than ever. The receiver with the gage were kept under water near ſeven hours, becauſe I was obliged to ſtay long abroad; at the end of which coming home, I found the receiver ſtaunch, but the frog dead and exceedingly ſwelled: upon the letting in of the air, ſhe became more hollow and lank than ever. April 14.

N. B. I have purpoſely, both under this title, and ſome others, ſubjoined ſome trials, whoſe events are not altogether ſuch, as others recited under the ſame head, which would invite one to expect; but I purpoſely do it, not only to be true to the impartiality, I propoſed to myſelf, in writing theſe narratives, but to awaken the curious to conſider and obſerve, what variety of phænomena in ſuch trials may be attributed to the ſeaſon of the year, wherein they are made; and to ſtrength, bulk, age, peculiar conſtitutions, &c. that relate to the reſpective animal, on which the experiments are made; beſides what things may on other account be fit to be alſo conſidered.

TITLE. IV.

Of the phænomena afforded by a new kittened kitling in the exhauſted receiver.

BEING deſirous to try, whether animals, that had lately been accuſtomed to live, either without any, or without a full reſpiration, would not be more difficult or ſlowly killed by the want of the air, than others, which had been been longer uſed to a free reſpiration; we took a kitling, that had been kittened the day before, and put it into a very ſmall receiver (that we gueſſed to hold about a pint or leſs,) that it might be the ſooner exhauſted. As ſoon as the pump began to play, I took notice of the time, and found by a watch, that marks minutes and quarter minutes, within one minute or little more, after the air firſt began to be withdrawn, that the little animal, who in the mean time had gaſped for life, and had ſome violent convulſions, lay as dead, with his head downwards, and his tongue out; but upon letting in of the air, he did in a trice ſhew ſigns of life, and being taken out of the receiver quickly recovered: and to allow him the benefit of his good fortune, we ſent for a kitling of the ſame age and litter, which being put into the ſame receiver, quickly began, like the other, to have convulſions, after which he lay as dead; but obſerving very narrowly, I perceived ſome little motions, which made me conclude him alive; which I ſoon found I had cauſe to do. For though we continued pumping, and could not perceive, that the engine leaked more than in the former experiments; the kitling began to ſtir again, and after a while had ſtronger and more general convulſions than before; until at the end of full ſix minutes, after the exſuction of the air was begun, the animal ſeeming quite dead, the outward air was re-admitted into the receiver, which not reviving him as it had done the other, he was taken out of the veſſel, and lay with his mouth open, and his tongue lolling out, without any ſenſible breathing, and pulſation; until having ordered him to be pinched, the pain or ſome internal motion, produced by the external violence done to him, made him immediately give manifeſt ſigns of life, though there was yet no ſenſible motion of the heart, or the lungs; but afterwards gaping and fetching his breath in an odd manner, and with much ſtraining, as I have ſeen ſome fœtus's do, when cut out of the womb, he, little by little, within about a quarter of an hour recovered: wherefore thinking it ſevere to make him undergo the ſame meaſure again, we ſent for another, kittened at the ſame time, and incloſing that alſo in the receiver, obſerved, that divers violent convulſions, as it were gaſping for breath, into which he began to fall at the ſecond or third ſuck, ended in a ſeeming death, within about a minute and a half. But being made more diffident by the late experiments, I cauſed the pump to be plied, and the rather, becauſe I had a mind to obſerve, whether, when the air was from time to time drawn away, there would not, upon the opening of the ſtop-cock to let it out, appear ſome ſudden ſwelling, greater or leſs, of the body of the animal, by the ſpring and expanſion of ſome air (or aerial matter) included in the thorax, or the abdomen. Such an inflation (though not great) we thought we obſerved; but until farther trial, I dare not acquieſce in it. A while after, notwithſtanding our continuing to pump, the kitling gave manifeſt ſigns of life, which was not until it had endured divers convulſions, as great as thoſe of the firſt fit, if not greater. When 7 minutes from the beginning of the exhauſtion were compleated, we let in the air; upon which the little creature, that ſeemed ſtark dead before, made us ſuſpect, that he might recover; but though we took him out of the receiver, and put aqua vitæ into his mouth, yet he irrecoverably died in our hands.

THESE trials may deſerve to be proſecuted with farther ones, to be made not only with ſuch kittens, but with other very young animals of different kinds; for by what has been related, it appears, that thoſe animals continued 3 times longer in the exhauſted receiver, than other animals of that bigneſs would probably have done.

TITLE V.

Some trials about the air, uſually harboured and concealed in the pores of water, &c.

IT might aſſiſt us to make the more rational conjectures about the phænomena of divers of our experiments, if we knew (ſomething near) what quantity of aerial ſubſtance is uſually found in the liquors we employ about them, eſpecially in that moſt common of them, water. And therefore, though it be very difficult (if at all poſſible) to determine the proportion of the air, that lurks in water, with any thing of certainty, many circumſtances making it ſubject to vary very much; yet to make the eſtimate, I eaſily could, where none at all, that I know of, hath been hitherto made by any man, I conſidered, that it might afford us ſome light, if we diſcovered, at leaſt, what proportion as to bulk, the air latitant in a quantity of water would have to the liquor it came from, when the aerial particles ſhould be gathered together into one place. For, though about this union, and the ſpring that may be conſequent to it, ſome doubts may be ſuggeſted, which I have not now time to diſcuſs; yet I ſuppoſed, that, at leaſt, ſome diſcoveries would by this way be made, though not of the true proportion between the air and the water, yet about two or three particulars, in due time to be taken notice of.

TO find inſtruments, which would any way accommodate our purpoſe, proved a very difficult work; ſo that among other things, that we were fain to do, this was one, that to evince how little the air, latitant in water, did appear to leſſen the bulk of that water, if it were ſuffered to fly away in an open tube; we ſuffered it to eſcape in an exhauſted receiver, without any artifice to catch it; by which trial the water did not part with any thing of its bulk, that made a diminution ſenſibly to the eye. Wherefore we endeavoured to make this loſs viſible by ſome other trials, of which I can find but a few haſty memorials among my looſe entries.

A chemical pipe ſealed at one end, and 36 inches, or ſomewhat leſs, in length, was filled with water, and inverted into a glaſs veſſel, not two inches in diameter, but $\frac{1}{4}$ of an inch, or little more in depth. Theſe glaſſes being conveyed into a fit receiver, and the air being leiſurely pumped out, and ſomewhat ſlowly re-admitted, the numerous bubbles, that had aſcended, during the operation, conſtituted at the top, an aerial aggregate, mounting to $\frac{8}{10}$, wanting about 100 part of an inch.

PRESENTLY after, the tube (by and by to be deſcribed) was filled again with the ſame water, and inverted; and the water being drawn down to the ſurface of the veſſeled water, and the air let in again, the water was impelled up to the very top, within a tenth and half a tenth of an inch. These are two experiments.

THE tube for meaſuring the air latitant in water was 43 inches and $\frac{1}{2}$ above the ſurface of the ſtagnant water: the air collected out of the bubbles at the top of the water was the firſt time $\frac{1}{4}$ of an inch, and ſomewhat better; the ſecond time we eſtimated it but $\frac{1}{2}$ and $\frac{1}{16}$. The firſt time the water in the pipe was made to ſubſide full as low as the ſurface of the reſtagnant water; the ſecond time the loweſt, we made it ſubſide, ſeemed to be four or five inches above the ſurface of the water in the open veſſel.

MATTER of fact thus recited would afford divers difficulties worthy to be considered, which I have not leisure to discuss; especially, the odd thing, that happens to the aerial particles of water: for though, whilst they lay concealed in the water, they took up so little room in it, that it was insensible; and when they were permitted to escape out of the tube, the water was not manifestly diminished by their recess; yet when they were associated at the top of the tube, their aggregate did sometimes maintain a place, that was considerable enough in reference to the capacity of the whole tube; though I must here advertise, that this aggregate did, at the top of the tube, possess more room than its bulk did absolutely require; because it was somewhat defended from the pressure of the atmosphere, by the weight of the subjacent cylinder of water, which might be about three or four foot long.

QUERE, Whether any considerable proportion of bubbles will be afforded by the same liquor, if it be suffered to continue in the glass for some competent time, after it has been once, or oftner, freed from bubbles already?

QUERE, How far it may be worthy our consideration, whether, in common water, there may not be concealed air enough to be of use to such cold animals as fishes; and whether it may be separable from the water, that strains through their gills?

BUT though I was at first content to make use of this way of estimating the air concealed in water; yet, when I came where I could be a little better accommodated with glasses, I bethought myself of a small instrument, that would much better disclose the wonderful plenty of the aerial particles I designed to discover. The structure and use of this glass, may be easily enough understood by the recital of the first experiment, that was made with it, whereof take the following transcript.

WE provided a clear round glass, furnished with a pipe or stem of about nine inches in length, the globulous part of the glass being on the outside about three inches and half in diameter: the pipe of this glass was within an inch of the top, melted at the flame of a lamp, and drawn out for two or three inches as slender as a crow's quill, that the decrement of the water upon the recess of the air, harboured in its pores, might, if any should happen, be the more easily observed and estimated. Above this slender part of the pipe, the glass, as was before intimated, was of the same largeness, or near it, with the rest of the pipe, that the aerial bubbles, ascending through the slender part, might there find room to break, and so prevent the overflowing, or loss of any part of the water.

THIS vessel being not without difficulty and some industry filled, till the liquor reached to the top of the slender part, where not being uniformly enough drawn out, it was somewhat broader than elsewhere; we conveyed the glass, together with a pedestal for it to rest upon, into a tall receiver, and pumping out the air, there disclosed themselves numerous bubbles, ascending nimbly to the upper part of the glass, where they made a kind of froth, or foam; but by reason of the above-mentioned figuration of the vessel, they broke at the top of the slender part, and so never came to overflow.

THIS done, the pump was suffered to rest a-while, to give the aerial particles, lodged in the water, time to separate themselves, and emerge; which when they had done a pretty while, the pump was plied again, for fear some air should have stolen into so large a receiver. These vicissitudes of pumping and resting lasted for a considerable time, till at length the bubbles began to be very rare, and we weary of waiting any longer: soon after which, the external air was let into the receiver, and it appeared somewhat strange to the spectators, that notwithstanding so great a multitude of bubbles, as had escaped out of the water, I could not by attentively comparing the place, where the surface of the water rested at first, to which a mark had been affixed, with that where it now stood; I could not, I say, discern the difference to amount to above, if so much, as an hair's breadth; and the chief operator in the experiment professed, that, for his part, he could not perceive any difference at all.

THUS far for the narrative of the trial made by water: but, that was not the only liquor, into whose aerial particles I designed by our little instrument to inquire; and therefore filling a glass of the same shape, and much of the same bigness, with claret wine, and placing it upon a convenient pedestal, in a tall receiver, we caused some of the air to be pumped out; whereupon, in a short time there emerged, through the slender pipe, so very great a multitude of bubbles, that were darted, as it were, upwards, as did not a little, both please and surprize the beholders; but it forced us to go warily to work, for fear the glass should break, or the wine overflow. Wherefore we seasonably left of pumping, before the receiver was any thing near exhausted, and suffered the bubbles to get away as they could, till the present danger was over-passed; and then from time to time, we pumped a little more air out of the receiver, till we were weary, the withdrawing of a moderate quantity of air at a time sufficing, even at the latter end, to make the bubbles not only copiously, but very swiftly to ascend (by a minute watch) for above a quarter of an hour together.

THE little instrument, made use of about these trials, being designed to examine, among other things, the quantity of bubbles lurking in several liquors, is to be applied to spirit of wine and chemical oils, that are more subtile liquors than wine itself. And some circumstances of our trials made us think, that it might be worth examining, what kind of substance may be obtained by this way of handling aerial and spirituous corpuscles. But of the other uses of our instrument elsewhere.

TITLE VI.

Of some phænomena, afforded by shell-fishes in an exhausted receiver.

EXPERIMENT I.

AN oyster being put into a very small receiver, and kept in long enough to have successively killed three or four birds or beasts, &c. was not thereby killed, nor, for ought we could perceive, considerably disturbed; only at each suck we perceived, that the air contained between the two shells broke out at their commissure; as we concluded from the foam, which at those times came forth all round that commissure. About twenty four hours after, coming to see in what condition this oyster was, I found, that both this, and another, that had been put at the same time into the receiver, were alive; but how long afterwards they continued so, I did not observe.

EXPERIMENT II.

THAT same day we put a pretty large craw-fish into a pretty large receiver, and found, that though he had been injured by a fall before he was brought thither, yet he seemed not to be much incommoded by being included, till the air was in great measure pumped out; and then its former motion presently ceased, and he lay as dead; till, upon the letting in a little air into the receiver, he began forthwith to move afresh. And upon the withdrawing the air again, he presently, as before, became moveless. Having repeated this trial two or three times, we took him out of the receiver, where he appeared not to have suffered any harm.

EXPERIMENT III.

BUT I thought it not unlikely, that there may be some such inequality in the strength or vivacity of animals, as to such kind of experiments as ours, that it might be well worth while in several cases to re-iterate our trials. And on this occasion, I shall here add, that having put an oyster into a vial full of water, before we included it in the receiver, that through the liquor the motion of the bubbles, expected from the fish, might be the more pleasantly seen and considered; this oyster proved so strong, as to keep itself close shut, and repressed the eruption of the bubbles, that in the other did force open the shells from time to time; and kept in its own air as long as we had occasion to continue the trial.

EXPERIMENT IV.

MOREOVER a craw-fish, that was thought more vigorous, being substituted in the place of the former craw-fish, though once he seemed to lose his motion together with the air, yet afterwards he continued moving in the receiver, in spight of our pumping: whether, because there was some unperceived leaking, that hindered a sufficient exhaustion of the air; or because this particular animal was more strong, or vivid, than the other, we could not positively determine.

TITLE. VII.

Of the phænomena of a scale-fish in an exhausted receiver.

THE following experiment is far from being the first, that was made on a scale fish in our vacuum; but in regard, that in the receivers, wherein those trials were made, the external air could not be kept out near so long, and so well as in the vessel I am about to mention; I judged it well worth the pains to observe, what would happen to a fish in an exhausted vessel, where it should be kept for some hours together from all supply of fresh air. And therefore I made several trials to that purpose; whereof, that, which I think the most considerable, was registered as follows:

WE took a receiver, shaped almost like a bolt-head, containing by estimation near a pint, and the globulous part of it being almost half full of water, we put into it, at the orifice (which was pretty large) a small gudgeon, about three inches long, which, when it was in the water, swam nimbly up and down therein. Then having drawn out the air so well, that we guessed by a gage, that about nineteen parts of twenty, or more might be exhausted, we secured ourselves, that the regress of the air should not injure our experiment; about which we observed these particulars.

FIRST, The neck of the glass being very long, though there appeared great store of bubbles all about the fish; yet the rest of the water, notwithstanding the withdrawing of so much air, as has been mentioned, emitted no froth, and but few bubbles.

SECONDLY, The fish both at his mouth and gills did, for a great while, discharge such a quantity of bubbles as appeared strange, and for about half an hour or more (for much longer I had not opportunity to watch it;) when ever he rested a-while, new bubbles would adhere to many parts of his body (as if they were generated there) especially his fins and tail; so that he would appear almost beset with bubbles; and if, being excited to swim, he was made to shake them off, he would quickly, upon a little rest, be beset with new ones as before.

THIRDLY, Almost all the while he would gape and move his gills, as before he was included; though towards the end of the time that I watched, it often happened, that he neither took in, nor emitted any aerial particles, that I could perceive.

FOURTHLY, After a while he lay almost constantly with his belly upwards, and yet would in that posture swim briskly as before.

FIFTHLY, Nay, after a while he seemed to be more lively than at first putting in: whether by reason, that by discharge of so many bubbles, which by their distension, perhaps put him to pain, he found himself relieved, or for some other cause, I examine not.

HAVING occasion to go abroad, I returned about an hour and a half after he had been sealed up, and found him almost free from bubbles,

bubbles, and with his belly upwards, and ſeeming ſomewhat tumid, but yet lively as before. But an hour and a quarter after that, when riſing from dinner, I went to look upon him again, he ſeemed to be moveleſs, and ſomewhat ſtiff; yet, upon ſhaking the glaſs, obſerving ſome faint ſigns of life in him by ſome languid motions, he attempted to make when excited to them, I opened the receiver under water, to try, if that liquor and air would recover him; and the external water ruſhing in, till it had filled the vacant part of the ball, and the greateſt part of the ſtem too, the fiſh ſunk to the bottom of it, with a greater appearance than ever of being alive; in which ſtate, after he had continued a pretty while, I made a ſhift, by the help of the water he ſwam in, to get him through the pipe into a baſon of water, where he gave more manifeſt ſigns of life; but yet for ſome hours lay on one ſide or other, without being able to ſwim, or lie on his belly, which appeared very much ſhrunk in, as if ſomething during the time of its being ſealed up had been broken in his body, or his belly had been exceedingly diſtended, beyond reſtitution to its former tone.

ALL the while he continued in the baſon of water, though he moved his gills as before he had been ſealed up, yet I could not perceive, that he did, even in his new water, emit, as formerly, any bubbles, though two or three times I held him by the tail in the air, and put him into the water again; where at length he grew able to lie conſtantly upon his belly, which yet retained much of its former lankneſs; and though it be now about, or above twenty four hours, ſince he was firſt included, he continues yet alive.

(POSTSCRIPT. He lived in the baſon eight or ten days longer; though divers gudgeons ſince taken died there in much fewer days.)

TITLE VIII.

Of two animals included, with large wounds in the abdomen, in the pneumatical receiver.

EXPERIMENT I.

Sept. 12. A SMALL bird, having the abdomen opened almoſt from flank to flank, without injuring the guts, was put into a ſmall receiver, and the pump being ſet to work, continued for ſome little time without giving any ſigns of diſtreſs; but at the end of about a minute and a half from the begining of the exhauſtion, ſhe began to have convulſive motions in the wings: and though the convulſions were not univerſal, or did appear violent, as is uſual in other birds, from whom the air is withdrawn by the engine, yet at the end of two full minutes, letting in the air, and then taking off the receiver, we found the bird irrecoverable; notwithſtanding which, we did not find any notable alteration in the lungs, and found the heart, or, at leaſt, the auricles of it, to be yet beating, and ſo it continued for a while after.

EXPERIMENT II.

WE took alſo a pretty large frog, and having, without violating the lungs or the guts, made two ſuch inciſions in the abdomen, that the two curled bladders or lobes of the lungs came out almoſt totally at them, we ſuſpended the frog by the legs in a ſmall receiver; and after we had pumped out a good part of the air, the animal ſtruggled very much, and ſeemed to be much diſordered; and when the receiver was well exhauſted, ſhe lay ſtill for a while, as if ſhe had been dead, the abdomen and thigh very much ſwelled, as if ſome rarefied air, or vapour, forcibly diſtended them. But as, when the frog was put in, one of the lobes was almoſt full, and the other almoſt ſhrunk up; ſo they continued to appear, after the receiver had been exhauſted; but upon letting in of the air, not only the body ceaſed to be tumid, but the plump bladder appeared for a while, ſhrunk up as the other, and the receiver being removed, the frog preſently revived, and quickly began to fill the lobe with air. Sept. 12.

TITLE IX.

Of the motion of the ſeparated heart of a cold animal in the exhauſted receiver.

WITHOUT diſcuſſing the opinions of learned men about the connection and dependency of the motions of the blood, and beating of the heart, I thought it might give me a ſufficient inducement to make the following experiment, that ſeveral ſorts of animals would be preſently killed in our vacuum by the withdrawing of the air; and even the inſects mentioned in the formerly publiſhed digreſſion about reſpiration, though they alſo were not totally deprived of life by the abſence of the air, yet they were of viſible motion: wherefore, ſome good hint or other being to be hoped for from the diſcovering, whether or no a ſeparated heart, which is but a part of an animal, would continue its motion in our vacuum; we made ſome trials to that purpoſe, whoſe ſucceſs I find thus ſet down.

EXPERIMENT I.

THE heart of an eel being taken out, and laid upon a plate of tin in a ſmall receiver, when we perceived it to beat there, as it had done in the open air, we exhauſted the veſſel, and ſaw, that, though the heart grew very tumid, and here and there ſent forth little bubbles, yet it continued to beat as manifeſtly as before, and ſeemed to do ſo more ſwiftly; as we tried by numbering the pulſations it made in a minute, whilſt it was in the exhauſted receiver and when we had re-admitted the air, and alſo when we took it out of the glaſs, and ſuffered it to continue its motion in the open air. The heart of another eel, being likewiſe taken out, continued to beat in the emptied receiver, as the other had done.

EXPERIMENT II.

THE heart of another eel, after having been included in a receiver, firſt exhauſted, and then accurately

accurately secured from leaking, though it appeared very tumid, continued to beat there an hour; after which, looking upon it, and finding its motion very languid, and almost ceased, by breathing a little upon that part of the glass, where the heart was, it quickly regained motion, which I observed a while; and an hour after, finding it to seem almost quite gone, I was able to renew it by the application of a little more warmth. At the end of the third hour, coming to look at it once more, a bubble, that appeared to be placed between the auricle and the heart, seemed to have now and then a little trembling motion; but I found it so faint, that I could no more by warmth excite it, so as plainly to perceive the heart to move: wherefore I suffered the outward air to rush in, but could not discern, that thereby the heart regained any sensible motion, though assisted with the warmth of my breath and hands.

TITLE X.

A comparison of the times, wherein animals may be killed by drowning, or withdrawing of the air.

TO help myself and others to judge the better of some difficulties concerning respiration, I thought it might be useful, that we compared together the times, wherein animals may be killed by that want of respiration, which, in those that are drowned, is caused by the water that suffocates them, and that other want, which proceeds from withdrawing the ambient air. Of the latter of these, a sufficient number of instances is to be met with among our other experiments, and therefore, I shall now subjoin about the former the more trials, because this comparison hath not, that I know of, been yet thought on by any.

EXPERIMENT I.

Sept. 10. A green-finch, having his legs and wings tied to a weight, was gently let down into a glass-body filled with water; the time of its total immersion being marked: at the end of half a minute after that time, the strugglings of the bird seeming finished, he was nimbly drawn up again, but found quite dead.

EXPERIMENT II.

WHEREUPON a sparrow, that was very lusty and quarrelsome, was tied to the same weight, and let down after the same manner; but though he seemed to be under water more vigorous than the other bird, and continued struggling almost to the very end of half a minute, from the time of his being totally immersed, (during which stay under water, there ascended, from time to time, pretty large bubbles from his mouth) yet notwithstanding that as soon as ever the half minute was compleated, he was drawn up, we found him, to our wonder, irrecoverably gone.

EXPERIMENT III.

A small mouse, being held under water by the tail, emitted from time to time, divers aerial bubbles out of his mouth, and at last, as one of the spectators affirmed, he saw at one of his eyes: being taken out at the end of half a minute and some seconds, he yet retained some motions; but they proved but convulsions, which at last ended in death.

"BY what is related under the first title, it "does not appear, that water-fowl, at least, "that ducks could, in our receivers, endure the "want of air much longer than other birds: "but now to shew, that the contrivance of "nature is not insignificant, as to the enabling "them to continue much longer under water, "without fresh air, than the land birds above-"mentioned, it will not be amiss to subjoin "the two following experiments."

EXPERIMENT IV.

WE took the duck mentioned in the first title, and so tied a considerable weight of lead to her body, as it did not hinder her respiration, and yet would be sure to keep her down under water; which we had found, that a small weight would not do by reason of her strength, nor yet a great weight, if tied only to her feet, in such a middle-sized tube as ours was, because of the height of her neck and beak. With the above-mentioned clog, the duck was put into a tub full of clear water, under whose surface she continued about a minute by my watch, quietly enough, but afterwards began to appear for a while much disturbed; which fit being over, our not perceiving any motion in her made us, at the end of the second minute, take her out of the water, to see in what condition she was, and finding her in a good one, we had allowed her some breathing time to recruit her self with fresh air, we let her down again into the tub, which in the mean time had been filled with fresh water, least the other, which had been troubled with the steams and foulness of the duck's body, might either hasten her death by its being infected with them, or hinder our discerning what should happen, by its being opacated by them.

THE bird being thus under water, did after a while, begin, and from time to time continue, to emit divers bubbles at her beak. There also came out at her nostrils, divers real bubbles from time to time; and when the animal had continued about two minutes, or better under water, she began to struggle very much, and to endeavour either to emerge, or change postures; the latter of which she had liberty to do, but not the former. After four minutes, the bubbles came much more sparingly from her: then also she began to gape from time to time, (which we had not observed her to do before,) but without emitting bubbles; and so she continued gaping until near the end of the sixth minute, at which time all her motions, some of which were judged convulsive, and others, that had been excited by our rouzing her with a forceps, appeared to cease, and her head to hang carelesly down, as if she was quite dead. Notwithstanding which, we thought fit for greater security, to continue her under water a full minute longer, and then finding no signs of life, we took her out, and being hung by the heels, and gently preſſed in convenient places, ſhe was made to void a pretty quantity of water, of which, whether any had been received into the lungs themſelves, we had not time and opportunity to examine. But all the means, that were to recover the bird to life, proving ineffectual, we concluded, ſhe had been dead a full minute before we removed her out of the water: ſo that, to ſum up the event of our experiment, even this water bird was not able to live in cold water, without taking in freſh air, above ſix minutes; which is but $\frac{1}{10}$ of an hour.

EXPERIMENT V.

THE duckling mentioned in the firſt title, and ſecond experiment, having a competent weight tied to her legs, was let down into a tub of water, which reached not above an inch or two higher than her beak: during the moſt part of her continuance, there came out ſtore of bubbles at her noſtrils; but there ſeemed to come out more and greater from a certain place in her head, almoſt equidiſtant from her eyes, but ſomewhat leſs remote from her neck than they. Whilſt ſhe was kept in this condition, ſhe ſeemed frequently to endeavour to dive lower under the water, and after much ſtruggling, and frequent gaping, ſhe had divers convulſive motions, and then let her head fall down backward, with her throat upwards. To which moveleſs poſture ſhe was reduced at the end of the third minute, if not a little ſooner; but a while after there appeared a manifeſt, but tremultous motion in the two parts of her bill, which continued for ſome time, but afforded no circumſtances, whereby we could be ſure, that they were not convulſive motions: but theſe alſo ceaſing upon the end of the fourth minute, the bird was taken out and found irrecoverable.

EXPERIMENT VI.

A viper, that was kept ſo many hours in an exhauſted receiver, till it was concluded to be ſtark dead, and to have been ſo for a good while, was nevertheleſs reſolutely hindered by me from being thrown away, till I had tried, what could be done by keeping it all night in a glaſs-body upon a warm digeſtive furnace. Whereupon this viper was found the next morning, not only to be revived, but to be very lively, ſo as to invite me to make with her, without ſeeking for another, the following experiment.

WE put her into a tall glaſs-body, fitted with a cork to the orifice of it, and depreſſed with weight, ſo that ſhe could come at no air. In this caſe we obſerved her from time to time; and after ſhe had been ducked a-while, ſhe lay with very little motion for a conſiderable ſpace of time. At an hour and a quarter ſhe often put out her black tongue: at near four hours ſhe appeared much alive, and, as I remember, about that time alſo put out her tongue, ſwimming all this while, as far as we obſerved, above the bottom of the water. At the end of about ſeven hours or more, ſhe ſeemed yet to have ſome life in her, her poſture being manifeſtly changed in the glaſs, from what it was a-while before; unleſs that might proceed from ſome difference made in her body, as to gravity and levity. Not long after, ſhe appeared quite dead, her head and tail hanging down moveleſly, and directly towards the bottom of the veſſel, whilſt the middle of the body floated as much as the above-mentioned cork would permit it.

HASTE maketh me pretermit the mention of divers things ſuggeſted by what hath been delivered upon the preſent title. But this one thing would be taken notice of, that, though ſome of the above-mentioned animals ſeem, by the relations we have given of them, to have been a little ſooner deſtroyed by drowning, than any we have mentioned were by our engine, that is no ſure proof, that ſuffocation does kill animals faſter than the deprivation of air, they are expoſed to in our engine. For in drowning, that which deſtroys is applied to its full vigour at the firſt, and all at once; whereas, our receivers being made for ſeveral purpoſes, the deprivation of the air, that they make, cannot be made all at once, but the air muſt be pumped out by degrees; ſo that till the laſt the receiver will be but partly emptied. For confirmation of which, I have this to allege, that, having in the preſence of ſome virtuoſi provided for the nonce a very ſmall receiver, wherein yet a mouſe could live ſometime, if the air were left in it, we were able to evacuate it at one ſuck, and by that advantage we were enabled, to the wonder of the beholders, to kill the animal in leſs than half a minute.

THE

THE CONTINUATION OF THE EXPERIMENTS CONCERNING RESPIRATION.

Printed first in the PHILOSOPHICAL TRANSACTIONS, N° 63, for *September* the 12th, 1670.

A PREFACE concerning these EXPERIMENTS.

THOUGH, to shun prolixity, the preface, which the author had made to all he wrote about respiration, have been purposely omitted; yet there are some few points so necessary to be taken notice of, that it is thought unfit to leave them wholly untouched. For, the following experiments being not at first written for the press, and thrown by for many years, till they were very hastily gathered together, and in some places supplied with others, little less hastily annexed, to make some necessary supplies, the reader must not expect in such a casual tract, (which the author confesses to be one of the most imperfect, and immethodical of all his compostures) any thing but novelty and truth, and an earnest desire to be serviceable in an inquiry so important to mankind, to the curious in general, and especially to physicians, who, by the encouraging mention they have made of his former endeavours in this kind, have invited him to add these many new experiments to those few, they had hitherto exercised their wits upon; and, to leave them the more freedom to do so, he purposely forbore to confirm, or confute any hypothesis, or so much as propose any of his own; declaring it to be his aim, not to espouse, or make a party, but to communicate to the curious some matters of fact, that are new; and in an historical way impartially delivered. No more of preface is now to be added, but that it is thought fit, for prevention of ambiguity, to give this advertisement, touching the ground of the title of *Vacuum Boylianum*, to be met with in these experiments; that, as learned men, both English and foreigners, in their writings, have familiarly, for distinction-sake, employed the titles of *Machina Boyliana*, and *Experimenta Boyliana*; so the author, that writ these, for the most part in haste, and for his own memory, did for dispatch-sake, call the absence of the air procured in his receivers, our vacuum; whence by analogy was framed the *Vacuum Boylianum*, which he therefore thinks the less improper, because, to call it vacuum absolutely, would be judged by many a declaring himself a vacuist, who does not yet own the being either of their opinion, or a downright plenist; or else he must be troublesome to the reader and himself, by frequently explaining, what sort of vacuum he understands; whereas he declares once for all, that by the *Vacuum Boylianum*, he means such a vacuity or absence of common air, as is wont to be effected or produced in the operations of the *Machina Boyliana*.

TITLE XI.

Of the accidents, that happened to animals in air, brought to a considerable degree, but not near the utmost one, of rarefaction.

IN the generality of our pneumatical experiments upon animals, it suited with our purposes, to rarefy the air as much, and for the most part as fast, as we could: but I had other trials in design, wherein an extraordinary degree of rarefaction, but yet not near the highest, to which the air might be brought by our engine, seemed likeliest to conduce to my inquiries, and particularly seemed hopeful to afford some light, in reference to those diseases and distempers, that are thought primarily to

 affect

affect the reſpiratory organs; or, to depend upon ſomething amiſs in reſpiration.

WHEREFORE having gages, by the help of which ſuch experiments might be much better performed, than elſe they could, I attempted ſeveral of them; ſome of whoſe ſucceſſes I find in the following memorials.

EXPERIMENT I.

Aug. 16. A linnet being put into a receiver, capable to hold about four half pints of water, the glaſs was well cloſed with cement and a cover, but none of the air was drawn out with the engine, or otherwiſe. And though no new air was let in, nor any change made in the impriſoned air; yet the bird continued there three hours, without any apparent approach to death; and though it ſeemed ſomewhat ſick, yet being afterwards taken out, it recovered, and lived ſeveral hours.

EXPERIMENT II.

Aug. 18. FROM the above-mentioned receiver about half the air was drawn out, a linnet being then in the glaſs, and in that rarified air, (which appeared by a gage to continue in that ſtate) the bird lived an hour and near a quarter, before it ſeemed in danger of death; after which, the air being let in without taking off the receiver, ſhe manifeſtly recovered, and leaped againſt the ſide of the glaſs: being taken out into the open air, ſhe flew out of my hand to a pretty diſtance.

EXPERIMENT III.

Sept. 9. WE conveyed into a receiver, capable to hold four half pints of water, a lark, together with the gage, by the help whereof we pumped out of the receiver, three quarters of the air, that was in it before: then heedfully obſerving the bird, we perceived it to pant very much, ſo that a learned phyſician (from whom I yet diſſented) judged thoſe beatings to be convulſive: having continued thus for a little above a minute and a half, the bird fell into a true convulſive motion, that caſt it upon the back. And although we made great haſte to let in the air; yet, before the expiration of the ſecond minute, and conſequently, in leſs than half a minute from the time immediately preceding the convulſion, the lark was gone paſt all recovery, though divers means were uſed to effect it.

EXPERIMENT IV.

Sept. 9. PRESENTLY after we put into the ſame receiver a green-finch, and having withdrawn the air, until it appeared by the gage there remained but half, we preſently began to obſerve the bird, and took notice, that, within a minute after, ſhe appeared to be very ſick, and ſhaking her head, threw againſt the inſide of the glaſs a certain ſubſtance, which I took to be vomit, and which afterwards appeared ſo: upon this evacuation the bird ſeemed to recover, and continue pretty well (but not without panting) until about the end of the fourth minute, at which growing very ſick, ſhe vomited again (ſhaking her head as at firſt,) but much more unqueſtionable than before, and ſoon after, eat up again a little of her vomit; at which time (whether, that contributed to her recovery or no) ſhe very much recovered. And though ſhe had in all three fits of vomiting, yet for the laſt ſeven or eight minutes, that we kept her in the receiver, ſhe ſeemed to be much more lively than was expected; which may in part be attributed to a little air, that by accident got in, though it were immediately pumped out again. At the end of a full quarter of an hour from the firſt exhauſtion of the receiver, the bird appearing not likely to die in a great while, and the engine being needed for other uſes, we took out the bird, and thereby put a period to the experiment.

EXPERIMENT V.

I now thought it fit to try, whether, though a viper would not hold out very many hours in air, brought to as high a rarefaction, as we could bring it by our engine, yet to that cold and vivacious animal, a very ſmall proportion of air, in compariſon of what was neceſſary to hot animals, would not ſuffice to keep it alive for a conſiderable time: the narration of the experiment I find regiſtered as follows.

A viper lately bought of the perſon, that at this ſeaſon uſes to take new ones, almoſt from day to day, was included together with a gage in a portable receiver, capable to hold about three pints and half of water. This veſſel being exhauſted, and ſecured againſt the regreſs of the air, the impriſoned animal was obſerved from time to time; and obſerved not only to be alive, but nimbly to put out, and to draw back, its tongue about 36 hours after it was ſhut up; for which reaſon we continued the veſſel longer in the ſame ſhady place; where at the end of 60 hours, looking upon her, as I was going to bed, ſhe appeared very dull and faint, and not likely to live much longer: and the next morning being by ſome occaſions carried abroad, and coming to look upon the glaſs preſently after dinner, I found her ſtark dead, with her mouth opened to a ſtrange wideneſs; wherefore ſuffering water to be impelled by the outward air into the cavity of the receiver, to obſerve how far that veſſel was then emptied of air, we found by the water, that was driven in, and afterwards poured out again, and meaſured, that 4 parts of 5, or rather 5 of 6 of the veſſeled air (if I may ſo call that, which was ſhut up in the receiver) had been pumped out; ſo that in an air ſo rarified as to expand itſelf to 5 or 6 times its former and uſual dimenſions, our viper was able to live 60 hours, that we are ſure of, and perhaps might a pretty while longer. April 12.

A digreſſive experiment concerning reſpiration upon very high mountains.

TO illuſtrate what I have taken notice of in the printed experiments about the unfitneſs for reſpiration, obſerved by the learned *Acoſta* in the high mountains of *Pariacaca*, I ſhall here add, what I have had the curioſity and occaſion to learn from divers travellers, whom I purposely consulted about these matters; whereof you will easily believe, that not many of them have had opportunity to give accounts. Meeting with an ecclesiastical person, that had visited those high mountains of *Armenia*, (on one of which, because of their height, the tradition of the natives will needs have the ark to have rested ;) I asked him, whether those mountains are as really so high as is given out, and whether at the top of that he visited, he found any difficulty of breathing. To the first part of which question he answered; that they were really exceeding high (which he might well judge of, having been upon some of the most famous both in *Europe*, *Asia* and *Africa* ;) and that he could not come to the top, because of the unpassable snows: and to the second part he replied, that whilst he was in the upper part of the mountain, he plainly perceived, that he was reduced to fetch his breath much oftener than he was wont, and than he did before he ascended the hill, and after he came down from it. And upon my inquiring, whether or no that difficulty of breathing might not be accidental, or peculiar to him, he told me, that he himself having expressed some wonder to find himself so short-winded, the people told him, that it was no more than happened to them, when they were so high above the plain; it being a common observation among them. And I was the more inclined, both to make inquiry about these matters, and to believe what he said, because what he related of their being covered with snow, and of an odd temperature of air, I had learned before from a traveller of another nation than this person, and a stranger to him.

THE same churchman, being asked by me, whether he had not in some part of *Europe* made the like observation (of the difficulty of breathing) told me, that he had done it upon the top of a mountain in the country of *Cevennes*, in or near the province of *Languedoc*; which may serve to confirm what I am about to relate from the mouth of a learned traveller, that was upon the top of one of the Pyreneans, that is not very remote from the mountains we speak of.

THIS gentleman, who was a person curious and intelligent, being brother-in-law to one of the chief lords of those parts, was by him invited, about the beginning of September, to visit a neighbouring mountain, that is at least one of the highest of the Pyreneans, which is commonly called *Pic de Midi*, upon whose top, where a tent was spread for them, they stayed many hours. His answers to the other questions I asked him, are elsewhere related: all that concerns this place being, that I find this set down among my adversaria; viz. I also inquired of him, whether they found the air at the top as fit for respiration as common air, which he told me they did not, but were fain to breath shorter, and oftener than usual; and because I suspected, that might come from their motion, I asked, whether they observed it to cease, when they came down to the bottom of the hill, which he told me they plainly did; besides that they stayed many hours at the top, too long to continue out of breath.

BUT that I may not here conceal any thing, that may conduce to the discovery of the truth in the matter under consideration, I shall here add, that I did sometimes think it worth further inquiry, whether the sickness, if not also the difficulty of breathing, that some have been obnoxious to, in the uppermost parts of Pariacacha, and perhaps, some other high mountains, may not be imputed, not so precisely to the thinness and rarity of the air, in places so remote from the lowermost part of the atmosphere, as to include certain steams of a peculiar nature, which, in some places, the air may be imbued with? In favour of which suspicion, I remember, that inquiring once of an intelligent man, who had lived several years in the Island of *Teneriff*, whether he had been at the top of the *Pic* of that name, and what he had there taken notice of about the air? he answered me, that he had attempted to go up to the top of the mountain, but, that though some of the company were able to do so, he and some others, before they had reached so high, grew so sick upon the operation they felt of the sharp air, and sulphurous exhalations which infected it, that they were fain to stay behind their companions, he having already found this effect of those piercing steams upon his face, which, when he made me this relation, was of a fair complexion, that the skin began to be of a pale yellow, and even his hair to be discoloured.

TITLE XII.

Of the observations produced in an animal, in changes as to rarity and density made in the self-same air.

IN the experiments hitherto recited, the animals, that were recovered from a gasping condition, have been so, by letting in fresh air upon them, and not the same, that had been withdrawn from them. Wherefore I thought it very requisite to try, whether the same portion of air, without being renewed, would, by being expanded much beyond its usual degree, and reduced to it, serve to bring an animal to death's door, and revive him again; since by the success of such a trial it would notably appear, that the bare change of the consistence of the air, as to rarity and density, may suffice to produce the abovementioned effect.

BUT to devise a way to put this experiment in practice appeared no easy matter; since it required a receiver, that should be transparent, and be capable of changing its bulk, without suffering any air to get in or out.

To surmount these difficulties, the first thing I thought on was, to take a fine limber and clear bladder of a sheep or hog, made more transparent by being anointed with oil, which was done on the outside, that the smell of it might less offend the animal to be included. Then we clipped off as much of the bladder at the neck, as was judged absolutely necessary to make an orifice capable of letting in a mouſe; that ſort of animals being, by reaſon of their ſmallneſs, the fitteſt of thoſe furniſhed with lungs and hot blood, we could procure. And whereas it ſeemed very difficult, when the neck of the bladder was cut off, to make up ſo large an orifice without wrinkles, at which the rarified air may eſcape; to obviate this inconvenience, we provided a round ſtick ſomewhat leſs than the orifice, that, the wood being laid over with a cloſe and yielding cement, (for pitch, or the like common ſtuff will not always ſerve the turn) we might be able to tie the bladder faſt and cloſe enough upon the thus fitted ſtopple.

AND now to reduce theſe things to practice, and by their help make our deſigned experiment, we included a mouſe into a receiver made according to this way, leaving in the bladder as much air, as we thought might ſuffice him for a long time, as the experiment was to laſt. Then putting this limber or extenſible receiver, if I may ſo call it, into an ordinary one of glaſs, and placing this engine near a window, that we may ſee thro' both of them; the air was by degrees pumped out of the external receiver, (as for diſtinction ſake I ſhall call it) and thereupon the air included in the bladder did proportionably expand itſelf, and ſo diſtend the external receiver, till being arrived at a degree of rarefaction, which rendered it unfit for the included mouſe's reſpiration, I perceived, though with ſome difficulty, in this animal, the ſigns of his being in great danger of ſudden death. Whereupon the outward air being haſtily let into the external receiver, compreſſed the ſwelled bladder to its former dimenſions, and thereby the included air to its former denſity, by which means the fainting mouſe was quickly revived. Having given him ſome convenient time of reſpite, the experiment was reiterated with the like ſucceſs, and we doubted not, but the third trial we made would have ended as the two former did; but that, whilſt we were conſidering of the ſickneſs of the mouſe, which, by reaſon of ſome opacity that could ſcarce be avoided in the wrinkled bladder, was not as to its degree ſo eaſily taken notice of, it grew irrecoverable by the ſubſequent condenſation of the air.

N.B. The confirmation of this by further experiments will properly fall under another title.

TITLE. XIII.

Of an unſucceſsful attempt to prevent the neceſſity of reſpiration by the production, or growth, of animals in our vacuum.

HAVING had frequent occaſions to obſerve, how quickly thoſe animals, whoſe blood is actually warm, did expire in our vacuum; and that even thoſe animals, with lungs, whoſe blood was actually cold, were not able to live any conſiderable time there; I thought it very well worth while, and yet extremely difficult, to try, whether there might not be ſome ways yet unpracticed, either to make ſuch animals as nature endows with lungs, live without reſpiration, or at leaſt, to bring ſuch inſects, and other animals, as can already live without air, to move alſo without it in our vacuum.

THEREFORE conſidering with my ſelf what happens to infants, and other young animals, in the womb, and even after they come from thence, if they continue to be wrapped up in the ſecundines; though as ſoon as they are brought into the free air, they may be preſently killed by being kept from breathing: conſidering alſo, what I elſewhere relate of the ſlow expiration of a very young kitling in our vacuum, together with the long want of reſpiration, which cuſtom enables ſome divers to endure: conſidering theſe things, I ſay, though I know, that ſomewhat may be objected to ſhew, that theſe inſtances are not altogether full to my purpoſe; yet they, among other things, invited me to think, that the leaſt unlikely projects, that occurred to my barren invention, would be theſe that follow.

FIRST, I thought fit to try, whether the ſeeds of reſpiring animals might be either hatched, or otherwiſe brought to produce young ones, in our vacuum. For, if that could be compaſſed, I ſhould obtain my end.

NEXT, in caſe of my failing in the former attempt, and that, which is to be after a few lines propoſed, I thought fit to try, whether at leaſt I could not bring the eggs of inſects to hatch or be animated; or aurelias, as they call them, that were already alive, turn according to the courſe of nature, into winged inſects, as flies, or butter-fiſhes: of which trials, and thoſe of the former ſort, the account properly belongs to another place, where I relate the ſucceſs of theſe and other attempts to produce plants and animals in our vacuum.

BUT thirdly, conſidering, that nature has ſo ordered it, that frogs, though when they are grown big enough to deſerve that name, they be amphibious animals, endowed with lungs; yet before they attain to that pitch, they live wholly in the water like fiſhes; I thought it the moſt expeditious, and leaſt improbable attempt we could make, to try, whether or no this animal, being as a fiſh brought to live, either in our vacuum, or at leaſt in highly rarefied air, would not continue to do ſo, after its lungs ſhould be perfectly formed. Wherefore, though I foreſaw, and foretold the difficulty, that would be met with in the proſecution of this experiment, namely, that the aerial bubbles, that would be diſcloſed in ſuch ſoft bodies upon the withdrawing of the preſſure of the ambient, would ſo violate the ſlight texture of thoſe tender animals, as to hinder them from living long, or moving freely; yet I thought it very fit to attempt the trial, whereof I find this account among my adverſaria.

EXPERIMENT I.

WE took a good company of tadpoles, and put them with a convenient quantity of water into a portable receiver of a round figure, and obſerved, that at the firſt exſuction of the air they did riſe to the top of the water, though moſt of them ſubſided again, till the next exſuction

ſuction raiſed them. They ſeemed by their active and wrigling motion to be very diſcompoſed. The receiver being exhauſted, they continued reſtleſs, moving all of them in the top of the water; and though ſome of them ſeemed to endeavour to go to the bottom, and dived ſome part of the way, eſpecially with their heads, yet they were immediately buoyed up again. Within an hour a or little more, they were all moveleſs, and lay floating on the water: wherefore I opened the receiver, upon which the air ruſhed in, and almoſt all of them (which were many) preſently ſunk to the bottom, but none of them recovered to life.

EXPERIMENT II.

A little after theſe, we included a leſſer number of tadpoles in a ſmaller glaſs, which was alſo exhauſted with the like circumſtances with the former. And when I found the other tadpoles to be dead, I haſted to theſe, which did not, except perhaps one, give any ſign of life, but upon letting in the air, theſe having not been long kept from it, ſome few of them did recover, and ſwam up and down lively enough for ſome time; though after a while they alſo died.

EXPERIMENT III.

SOME years after I repeated the ſame experiment in a portable receiver of a convenient kind; and though after the exhauſtion was perfected, the tadpoles did for a while move briſkly enough on the top of the water, none of them appearing able to dive or ſwim under water, yet coming to look on them at the end of an hour, they ſeemed to be all of them quite dead, yet continued floating. And though within half an hour after that, I let in the air upon them, yet all the effect of it was, that the moſt of them immediately ſunk to the bottom, as the reſt of them did a very little while after; none of them, that I could obſerve, recovering any vital motion.

EXPERIMENT IV.

THERE remains an experiment, which I often judged as well more hopeful as more noble, if I could procure an opportunity to bring my deſign to a trial, which I have found it very difficult to do; nevertheleſs I was able to do it once, though not fully as I deſired, yet not altogether without ſucceſs.

WE procured then, and with much ado, ſome of thoſe odd inſects, which I elſewhere deſcribe, whereof gnats have by ſome ingenious men been obſerved to be generated about the end of Auguſt, or beginning of September. Theſe for ſome weeks live all together in the water, as tadpoles do, ſwimming up and down therein, till they are ripe for a tranſmigration into flies: which it ſelf is ſo great a rarity in nature, as makes theſe little creatures recompence to our curioſity the trouble, they often give our faces and hands. Suppoſing then, that if I could get ſome of theſe, and include them, being of thoſe inſects they call aquatilia, and ſo minute as they are, they may live a great while in the receiver without air, and in the mean while attain the period, which, according to nature's courſe, is wont to turn them into flies, which might come forth winged creatures into a medium not furniſhed with common air, as others of their kind enjoy; ſuppoſing, I ſay, that theſe inſects would afford me ſome information about theſe particulars, having upon much watching met with four or five of them after a ſhower of rain, that dropped from a houſe into a veſſel laid on purpoſe for it, we included them with ſome of their water into a ſmall glaſs receiver, which being very exactly cloſed, we kept in a ſouth-window, where theſe little creatures continued to ſwim up and down for ſome few days, without ſeeming to be much incommodated by ſo unuſual an habitation; and at the end of that time, and much about the ſame day, they diveſted the habit they had, whilſt they lived as fiſhes, and appeared with their exuviæ or caſt-coats under their feet, ſhewing themſelves to be perfect gnats, that ſtood without ſinking upon the ſurface of the water, and diſcovered themſelves to be alive by their motion, when they were excited to it: but I could not perceive them to fly in that thin medium; to which inability whether the viſcoſity of the water might contribute, I know not, though they lived a pretty while, till hunger or cold deſtroyed them. Something in this experiment may deſerve ſerious reflections; which I cannot ſpare time to offer at.

A digreſſive experiment, concerning the expanſion of blood, and other animal juices.

FOR ſome purpoſes, relating partly to reſpiration, and partly to other enquiries, I thought fit to endeavour to obtain, what information could be procured, of the conſiſtence and diſpoſition to expand itſelf of blood, and other animal liquors; in purſuance of which the enſuing trials, among others, were undertaken.

THE warm blood of a lamb or a ſheep, being taken as it was haſtily brought from the butcher's, where the fibres had been broken, to hinder the coagulation, was in a wide mouthed glaſs put into a receiver, made ready for it; and the pump being early ſet on work, the air was diligently drawn out; but the operation was not always, eſpecially at firſt, ſo early manifeſt, as the ſpirituouſneſs of the liquor made ſome expect: yet this hindered not, but, after a long expectation, the more ſubtle parts of the blood would begin to force their way through the more clammy ones, and ſeem to boil in large cluſters, ſome as big as great beans or nutmegs; and ſometimes, to the wonder of the by-ſtanding phyſicians, the blood was ſo volatile, and the expanſion ſo vehement, that it boiled over the containing glaſs; of which, when it was put in, it did not, by our eſtimate, fill above a quarter. Having alſo included ſome milk warm from the cow, in a cylindrical veſſel of about four or five inches high, though the operator were induced to pump a great while before any intumeſcence appeared in the milk, yet afterwards, when the external air was fully withdrawn, the white liquor began to boil in a way, that was not ſo eaſy to deſcribe, as pleaſant to behold: and this it did for a pretty while with ſo much impetuoſity, that it threw up ſeveral parts of itſelf out of the wide mouthed glaſs, that contained it (and could have contained as much more) though there were not above two or three ounces of the liquor.

A yet greater diſpoſition to intumeſcence, we thought, we obſerved in the gall, which was but ſuitable to the viſcoſity of the texture.

NOTE, that the two foregoing experiments were made with an eye caſt upon the enquiry, that I thought might be made; whether, and how far the deſtructive operation of our engine upon the included animal, might be imputed to this, that upon the withdrawing of the air, beſides the removal of what the air's preſence contributes to life, the little bubbles generated upon the abſence of the air in the blood, juices, and ſoft parts of the body, may by their vaſt number, and there conſpiring diſtenſion, variouſly ſtrengthen in ſome places, and ſtretch in others, the veſſels, eſpecially the ſmaller ones, that convey the blood and nouriſhment; and ſo by choaking up ſome paſſages, and vitiating the figure of others, diſturb or hinder the due circulation of the blood? not to mention the pains, that ſuch diſtenſions may cauſe in ſome nerves, and membranous parts, which by iritating ſome of them into convulſions, may haſten the death of animals, and deſtroy them ſooner, by occaſion of that irritation, than they would be deſtroyed by the bare abſence or loſs of what the air is neceſſary to ſupply them with. And to ſhew, how this production of bubbles reaches, even to very minute parts of the body, I ſhall add on this occaſion, hoping, that I have not prevented myſelf or any other, what may ſeem ſomewhat ſtrange, what I once obſerved in a viper, furiouſly tortured in our exhauſted receiver, namely, that it had manifeſtly a conſpicuous bubble moving to and fro in the wateriſh humour of one of its eyes.

Another digreſſive experiment belonging to the ſame title.

To ſhew, that not only the blood and liquors, but alſo the other ſoft parts, even in cold animals, have aerial particles latitant in them; we took the livers and heart of an eel, as alſo the head and body of another fiſh of the ſame kind, cut aſunder croſs ways ſomewhat beneath the heart; and putting them into a receiver, upon the withdrawing of the air we perceived, that the liver did manifeſtly ſwell every way, and that both the upper and lower parts did ſo likewiſe; and at the place where the diviſion had been made, there came out in each portion of the fiſh divers bubbles, ſeveral of which ſeemed to come from the *medulla ſpinalis*, or the cavity of the back-bone, or the adjoining parts; and the external air being let in both the portions of the eel preſently ſhrunk, ſome of the ſkins ſeeming to be grown empty or flaccid in each of them.

TITLE XIV.

Of the power of aſſuefaction to enable animals to hold out in air, by rarefaction made unfit for reſpiration.

"THE power of aſſuefaction in other "caſes, made me think it very well "worth trying, what it would do in reſpiration; "and the rather, becauſe I preſumed, it might "prove an experiment of good uſe, if we "ſhould diſcover, that by a gradual accuſto-"mance an animal may be brought to live, "either in a much thinner air, or much lon-"ger in the ſame air, than at firſt he could. "But in regard, that to make ſuch a trial per-"ſpicuouſly enough, the opacity of the blad-"der made uſe of in the former title was like "to be an impediment, I deviſed another way "to obviate that inconvenience, which may, I "hope, be competently underſtood, by the "heedful peruſal of the following trials."

EXPERIMENT I.

WE included in a round vial with a wide neck, (the whole glaſs being capable of containing about 8 ounces of water) a young and ſmall mouſe, and then tied ſtrongly upon the upper part of the glaſs's neck a fine thin bladder, out of which the air had been carefully expreſſed, and then conveyed this phantaſtical veſſel into a middle ſized receiver, in which we alſo placed a mercurial gage (adjuſted by our elſewhere mentioned ſtandard:) this done, the air was by degrees pumped out, until it appeared by the gage, that there remained but a fourth part in the external receiver (as for diſtinction ſake I call it) whereupon the air in the internal receiver expanding itſelf, appeared to have blown the bladder almoſt half full, and the mouſe ſeeming very ill at eaſe by his leaping, and otherwiſe endeavouring to paſs out at the neck of his uneaſy priſon; we did, for fear the over thin air would diſpatch him, let the air flow into the external receiver, whereby the bladder being compreſſed, and the air in the vial reduced to its former denſity, the little animal quickly recovered.

EXPERIMENT II.

A while after, without removing the bladder, the experiment was repeated, and the air, by the help of the gage was reduced to its former degree of rarefaction, and the mouſe, after ſome fruitleſs endeavours to get out of the glaſs, was kept in that thin air for full 4 minutes; at the end of which he appeared ſo ſick, that, to prevent his dying immediately, we removed the external, and took out the internal receiver: whereupon, though he recovered, yet it was not without much difficulty, being unable to ſtand any longer upon his feet, and for a great while after continued manifeſtly trembling.

EXPERIMENT III.

BUT having ſuffered him to reſt a reaſonable ſpace of time, preſuming, that aſſuefaction had

had accuſtomed him to greater hardſhips; we conveyed him again into the external receiver, and having brought the air to the former degree of expanſion, we were able to keep him there for a full quarter of an hour; though the external receiver did not at all conſiderably leak; as appeared, both by the mercurial gage, and by the continuing diſtenſion of the bladder. And it is worth noting, that, till near the latter end of the quarter of an hour, not only the animal did ſcarce at all appear diſtreſſed, remaining ſtill very quiet; but, which is more, whereas when he was put in, the trembling formerly mentioned were yet upon him, and continued ſo for ſome time; yet afterwards, in ſpite of the expanſion of the air he was then in, they left him early enough. And when the internal receiver was taken out, he did not only recover from his fainting fit ſooner than before; but eſcaped thoſe ſubſequent tremblings we have mentioned.

Experiment IV.

Encouraged by this ſucceſs, after we allowed him ſome time to recollect his ſtrength, we reconveyed him and the odd veſſel, wherein he was included, into the former receiver, and pumped out the air, till the mercury in the gage was not only drawn down as low as formerly; but near half an inch lower, that there the air might be yet further expanded, than hitherto it had been. And though this did at firſt ſeem to diſcompoſe our little beaſt; yet after a-while he grew very quiet, and continued ſo for a full quarter of an hour: when being deſirous to try what operation a farther rarefaction of the air would have upon him, we cauſed three exſuctions more to be made by the pump, before we diſcovered him to be in manifeſt danger (at which time the bladder appeared much fuller than before) but then we were obliged to let the air into the outward receiver; whereupon the mouſe was more ſpeedily revived than one would have ſuſpected.

And theſe trials of the power of aſſuefaction ſeemed the more conſiderable, becauſe the air, in which the mouſe had all this while lived, had been clogged and infected with the excrementitious effluviums of his body; for it was the ſame all along, we having purpoſely forborn to take off the bladder, whoſe regular intumeſcencies and ſhrinkings ſufficiently manifeſted, that the veſſel, whereof it was a part, did not leak.

POSTSCRIPT.

"Though the ſucceſs of the recited " experiments is very promiſing; yet a " ſubſequent trial or two, whoſe particulari- " ties are ſlipped out of my memory, oblige " me in point of candour, to declare, that, " for further ſatisfaction, the trials of the " power of accuſtomance, in reference to air " unfit for reſpiration, ought to be both reite- " rated, and to be made in differing ſorts of " animals."

TITLE XV.

Some experiments ſhewing, that air, become unfit for reſpiration, may retain its wonted preſſure.

Experiment I.

We took a mouſe of an ordinary ſize, having (not without ſome difficulty) conveyed him into an oval glaſs, fitted with a ſomewhat long and conſiderably broad neck, which we had provided, that it might be wide enough to admit a mouſe in ſpight of his ſtruggling. We conveyed in after him a mercurial gage, in which we had diligently obſerved, and marked the ſtation of the mercury, and which was ſo faſtened to a wire, reaching to the bottom of the oval glaſs, that the gage, remaining in the neck, was not in danger to be broken by the motions of the mouſe in the oval part: the upper part of the long neck of the glaſs was, notwithſtanding the wideneſs of it, hermetically ſealed by the help of a lamp and a pair of bellows, that we might be ſure, that the impriſoned animal ſhould breath no other air, than that which filled the receiver at the time when it was nipped up. This done, the mouſe was watched from time to time; and though by reaſon of the largeneſs of the veſſel, in compariſon of ſo ſmall an animal, he ſeemed to me rather drooping, than very near death at the end of the ſecond hour; yet coming to look upon him about half an hour after, he was judged by the ſpectators quite dead, notwithſtanding our ſhaking of the veſſel to rouze him up. This made me caſt my eyes upon the gage, wherein I could not perceive any ſenſible change of the mercury's ſtation. But being unwilling to give over the mouſe, without trying what freſh air would do to recover him, I cauſed the ſealed part of the glaſs to be broken off, and, notwithſtanding that his continuing to appear dead increaſed the confidence of thoſe that thought him ſo, I obtained after a while ſome faint tokens of life; though I am not ſure, that they would have continued in a veſſel, where the air was ſo clogged and infected, if it had not been, that freſh air was frequently blown in by a pair of bellows, whoſe noſe was inſerted into the neck of the glaſs. This freſh air ſeemed evidently, though but ſlowly, to revive the gaſping animal, whom I would not, nor could not, conveniently take out of the glaſs, till he had gained ſtrength enough to make uſe of its legs; after which, without breaking of the glaſs (which I was loath to looſe, having then no other of the kind) we took him out, and found him quickly able to go up and down. After which ſervice, and another trial we had with him, which belongs not to this place, we ſet him at liberty to ſhift for himſelf.

Experiment II.

Such an experiment as the former we made with like ſucceſs upon a ſmall bird, included

cluded with a gage in a receiver, holding about a quart of water. The bird in about half an hour appeared to be ſick and drooping, and the faintneſs and difficulty of breathing increaſed for about two hours and a half after that, at which time the animal died, the gage being not ſenſibly altered, unleſs perhaps the mercury appeared to be impelled up a little thort higher than it was when put in; which yet might well enough proceed from ſome accidental cauſe.

EXPERIMENT III.

To ſatisfy ſome curious perſons, that it is not want of coldneſs, but ſomething elſe in the included air, that makes it deſtroy the birds, that are pent up in it, and by the hot exhalations, that ſteam from their bodies, may be ſuppoſed to overwarm it, we made the following experiment.

In a glaſs-vial, capacious enough to hold about three quarts of water, we not only included, but for greater accuracy, hermetically ſealed up a ſmall bird, and found, that in a few minutes he began to be ſick and pant; which ſymptoms I ſuffered to continue and increaſe againſt the mind of a learned by-ſtander, (who thought the animal would not hold out ſo long,) till they had laſted juſt half an hour: at which time, having provided a veſſel of water with ſal armoniack, newly put into it, to refrigerate it, (according to the way I elſewhere publiſhed;) and the liquor thus made exceeding cold, ſomewhat to the wonder of thoſe that felt it; the vial, with the ſick bird, was immerſed in it, and kept there in that condition for ſix minutes; and yet it did not appear, in the judgment of the by-ſtanders, that the great refrigeration, that muſt be this way way procured to the impriſoned air, did ſenſibly revive, or refreſh the drooping animal, who manifeſtly continued to pant exceedingly as before, and, as ſome affirmed, more; ſo that this remedy proving ineffectual, the vial was removed out of the water, and the bird ſometime after did, as I foretold, make many ſtrains to vomit (though ſhe brought up little) followed by evacuations downward, before ſhe quite expired, which ſhe did within a minute or two of a juſt hour, after the beginning of her impriſonment.

If I had been able (which I was not) to procure more birds, I would willingly have proſecuted this experiment by ſeveral other, not unhopeful, trials which for want of ſubjects I was fain to leave only deſigned.

TITLE XVI.

Of the uſe of the air, to elevate the ſteams of bodies.

In the digreſſion about reſpiration, annexed to the 41ſt of our Phyſico-mechanical Experiments formerly publiſhed, it is propoſed as one of the conſiderable uſes of the air in reſpiration, that, being drawn into the lungs, it ſerves to carry off with it, when it is breathed out again, the recrementitious ſteams, that are ſeparated from the maſs of blood in its paſſage through the lungs; from which fuliginous excrements if the blood were not continually freed by the help of the air, after nature had been accuſtomed to that way of diſcharging them, their ſtay in the body might have very great and deſtructive operations on it.

For the illuſtration of this uſe of the air, I ſhall now ſubjoin the following experiment.

We made by diſtillation a blood-red liquor, which chiefly conſiſted of ſuch ſaline and ſpirituous particles, as may be obtained from the maſs of blood in human bodies: this liquor is of ſuch a nature, that if a glaſs vial, about half filled with it, be kept well ſtopped, the red liquor will reſt as quietly as any ordinary one, without ſending up any ſmoke or viſible exhalation; but if the vial be unſtopped ſo, that the external air be permitted to come in, and touch the ſurface of the liquor, within a quarter of a minute or leſs, there will, upon this contact, be elevated a copious white ſmoke, which will not only fill the upper part of the glaſs, but plentifully paſs out into the open air, till the vial be again ſtopped.

My purpoſe in this tract to forbear ſidings in controverſies keeps me from taking notice of the ſpeculations ſuggeſted by ſome of the phænomena of this liquor; which yet I thought I might lawfully mention, as far as I have done it, becauſe it but adventures upon giving one of the uſes rather of the air, than immediately of reſpiration it ſelf; and is brought but to illuſtrate what I have not found denied by any, though conſidered by very few; namely, the office of the air to carry off in expiration the fuliginous ſteams of the lungs. For in our experiment we manifeſtly ſee, that the very contact of the air may give the corpuſcles of moiſt bodies a peculiar volatility, or facility to emerge in the form of ſteams. I know, there are ſome corroſive ſpirits, as in nitre and ſalt, ſimple, or compounded of them, that, when they are very ſtrong, emit for a while manifeſt fumes; but the difference of thoſe liquors, and their inferiority to our red ſpirit, in the capacity of ſmoking liquors, might eaſily enough be manifeſted, if it were judged proper in this place, where it may ſuffice, to take notice of theſe two things. The one is, that when the vial has lain ſtopped and quiet a competent time, the upper half of it will appear deſtitute of fumes, of which the air, it ſeems, will imbibe, and conſtantly retain but a certain moderate quantity; which may give ſome light towards the reaſon, why the ſame air, which will be quite clogged with ſteams, will not long ſerve for reſpiration, which requires frequent ſupplies of freſh air. The other is, that if the unſtopped vial were placed in our vacuum, it would not emit any viſible ſteams at all, nor ſo much as to appear in the upper part of the glaſs itſelf that held the liquor; whereas, when the air was by degrees reſtored at the ſtopcock, without moving the receiver itſelf, to avoid injuring its cloſeneſs, the returning air would preſently raiſe the fumes, firſt into the vacant part of the vial, whence they would aſcend into the capacity of the receiver; and likewiſe, when the air, that was requiſite to ſupport them, was pumped out, they alſo accompanied it, as their unpleaſant ſmell evinced, and the red ſpirit, though it remained unſtopped, emitted no more fumes till the new air was let in.

One may compare with this liquor another ſmoking one, mentioned in the 29th of the firſt publiſhed pneumatical experiments, where an experiment is related of it, that has ſomething in common with this, and may ſo far ſerve to confirm what is now delivered, as this alſo has ſome things additional to that: beſides that, that liquor being made with ingredients corroſive, and of a bad name among chemiſts themſelves, the fumes, that proceed from it, may fright many from daring to meddle with it whereas; this our red ſpirit has been found potently medicinal for ſome diſtempers of the lungs, by a doctor of phyſick, whom I deſired to try it. The other phænomena of this liquor I ſhall not ſtay to deſcribe, as not belonging to this place; and the liquor itſelf, with very little variation, I have in the Hiſtory of Colours communicated.

TITLE XVII.

Of the long continuance of a ſlow-worm and a leach alive, in the vacuum made by our engine.

In the often cited digreſſion about reſpiration, there is mention made of the great vivaciouſneſs of houſe-ſnails, as they call them, and how little operation the withdrawing of the air had upon them in compariſon of what it is wont to have on other animals. I ſhall now add by way of confirmation, that I made trial upon ordinary white ſnails, without ſhells, whereof two of differing ſizes (the biggeſt about an inch and a half, and the other about an inch in length) were included in a ſmall portable receiver, which being carefully exhauſted, and ſecured againſt the return of the air, was attentively conſidered by me, preſently after it was removed from the engine; whereby it was eaſy to diſcern, that both the ſnails thruſt out and retracted their horns (as they are commonly called) at pleaſure, though their bodies had in the ſofter places pretty ſtore of newly generated bubbles ſticking to them: but though they did not loſe their motion near ſo ſoon, as other animals were in our vacuum wont to do; yet coming to look on them after ſome hours, they appeared moveleſs and very tumid, and at the end of twelve hours, the inward parts of their bodies ſeemed to be almoſt vaniſhed, and they ſeemed to be but a couple of ſmall full-blown bladders; and on the letting in of the air they immediately ſo ſhrunk, as if the bladders having been pricked, the receding air had left behind it nothing but ſkins; nor did either of the ſnails afterwards, though kept many hours, give any ſigns of life.

Upon a ſuppoſition, that the cold and clammy conſtitution of ſnails might be a main cauſe of their being able to endure the abſence of the air ſo well, I thought it worth trial, whether effs and leaches might not yet be more able to continue in our vacuum than a ſnail; and accordingly ſome experiments were made purſuant to that curioſity; the moſt fully regiſtered whereof are theſe, that follow.

Experiment I.

We included in a receiver, whoſe globular part was about the bigneſs of a large orange, one of that ſort of animals, that they vulgarly call effs: having withdrawn, but not ſollicitouſly, the air, and ſecured the veſſel againſt the unpermitted return of it, we kept him there about eight and forty hours, during all which time he continued alive, but appeared ſomewhat ſwelled in his belly; his under-chap moving the very firſt night, but not the day and night following. By opening the receiver at length under water, we perceived, that about half the air had been drawn out. As ſoon as the water was impelled into the glaſs, the animal, that was before dull and torpid, ſeemed, by very nimble and extravagant motions to be ſtrangely revived.

Experiment II.

We took a leach, that was of a moderate bigneſs, or ſomewhat ſhort of it; and having included it together with ſome water in a portable receiver, that was gueſſed to be capable of holding about 10 or 12 ounces of that liquor, the air was pumped out after the uſual manner, and the receiver being removed to a lightſome place, we obſerved, as we expected, that the leach keeping himſelf under water, there emerged from divers parts of her body ſtore of bubbles, ſome of them in a diſperſed way, but others in rows or files, if I may ſo ſpeak, that ſeemed to come from determinate points. Though this production of bubbles laſted a pretty while, yet the leach did not ſeem to be very much diſcompoſed by her preſent condition. This done, we diſpoſed of the receiver, which was well ſecured from the ingreſs of the outward air, into a quiet place, where we daily viſited it once at leaſt, or oftner, as there was occaſion; and found the leach ſomewhat faſtened by her tail to that part of the glaſs, that was under water, and ſometimes wandering about that part, which was quite above water; and ſtill, when we endeavoured to excite her, ſhe quickly manifeſted herſelf to be alive: and indeed (which will be thought ſtrange) appeared ſo lively after the full expiration of five natural days, that expecting ſomething might have happened to the receiver, and thereupon reſolving to try how ſtanch it had continued, I opened it under water, by which means the outward air impelled in ſo much of that liquor, that I was ſatisfied, the receiver was immediately before as well exhauſted, as others are wont to be in our pneumatical experiments.

TITLE. XVIII.

Of what happened to ſome creeping inſects in our vacuum.

Notwithstanding the great variety of reptiles, that nature does almoſt every where produce; yet the inconvenient time,

time and place, wherein the following trials were made, supplied me with so few, that about those animals I find among my adversaria, no more than the ensuing notes.

EXPERIMENT I.

We took five or six caterpillars of the same sort; but I could not tell, to what ultimate species the writers about insects referred them. These being put into a separable receiver of a moderate size, had the air drawn away from them, and carefully kept from returning. But notwithstanding this deprivation of air, I found them about an hour after moving to and fro in the receiver; and even above two hours after that, I could, by shaking the vessel, excite in them some motions, that I did not suspect to be convulsive: but looking upon them again some time before I was to go to bed (which may be was about 10 hours after they were first included) they seemed to be quite dead; and though the air were forthwith restored to them, they continued to appeared so, until I went to bed; yet, for reasons elsewhere expressed, I thought fit to try, whether time might not at length recover them, and leaving them all night in the receiver, I found the next day, that three, if not four of them, were perfectly alive.

EXPERIMENT II.

We took from an hedge a branch, that had a large cobweb of caterpillars in it; and having divided it into two parts, we put them into like receivers, and in one of them shut up the caterpillars together with the air, which from the other was exhausted. The event was, *that in that, which had the air, the little and difficultly visible insects, after a small time,* appeared to move up and down as before, and so continued to do for a day or two; after which, other occasions made the experiment to be neglected; whereas that glass, whence the air had been drawn out, and continued kept out, shewed after a very little while no motion, that we could perceive. But to try, whether caterpillars may continue so far alive in our vacuum all the winter, as the next spring or summer, to proceed in the transmigration to a butterfly, is a trial, that we have but begun, and therefore must not pretend to say any thing about its event.

TITLE XIX.

Of the phænomena suggested by winged insects in our vacuum.

When our Physico-mechanical Experiments were dispatched to the press, the inconvenient season of the year, and the difficulty of making the receivers, I then employed, to keep out the air for any long time, hindered me from then publishing above a trial or two of what will happen to winged insects in our vacuum. But afterwards being provided of more commodious vessels, I thought fit at several times, to repair that omission by various attempts, whereof the chief ensue.

EXPERIMENT I.

There were taken four middle sized flesh-flies, which having their heads cut off, were inclosed in a portable receiver, furnished with a pretty large pipe, and a bubble at the end. As soon as the receivers was exhausted, those flies lost their motion, which was not brisk before; an hour or two after, I approached them to the fire, which, restored not their motion to them (but as to one of them, I suspect it had a languid motion for a while:) wherefore I let in the air upon them, after which, in a very short time (though not immediately) they began one after another to move their legs, and one or two of them to walk; and having kept them all night in a warm place, when I sent one the next morning to try, if they would manifest any motion, he told me, that for a while they did, though when I afterwards rose myself, I could not perceive any motion in them. Nov. 12, about 8 a clock at night.

EXPERIMENT II.

About noon we closed up divers ordinary flies, and a bee or wasp; all which, when the air was fully withdrawn, lay as dead, save that for a few minutes some of them had convulsive motions in their legs. They continued in this state 48 hours, after which, the air was let in upon them, and that not producing any signs of life in them, they were laid in the meridian sun, but not any of them seemed in any degree to recover. Sept. 11.

EXPERIMENT III.

We put a great flesh-fly into a very small portable receiver, where, at first, it appeared to be very brisk and lively, but as soon as the air was drawn out, fell on his back, and seemed to have convulsive motions in her feet and proboscis; from whence she presently recovered upon the letting in of the air, which being drawn out again, she lay as dead: but a while after, (within a quarter, or half an hour) I perceived, that upon shaking the receiver, she stirred up and down, but faintly. This was done pretty late yesternight, since whence I had not occasion to look on the glass, till this night after supper, when I found the fly not (whilst I stayed to endeavour it) to be recovered either by warmth, or letting in the air. A while after this note was written, this fly recovered; and being next morning sealed up again in that glass, and kept 48 hours, though over the chimney, died for good and all. Dec. 11.

EXPERIMENT IV.

We took a large grass-hopper, whose body, besides the horns and limbs, was about an inch in length, and of a great thickness in proportion to that length: this we conveyed into a portable receiver of an oval form, and capable of holding, by our guess, about a pint of water and more; and having afterwards pumped out the air, till by the gage it appeared to have been pretty well drawn out, we took care, no air

air should re-enter to disturb the experiment. The success whereof was this: first, though, before the exhaustion of the air was begun, the grass-hopper was stirring, and lively, and continued so for a while after the beginning of the operation; yet when the air began to be considerably rarified, he appeared to be very ill at ease, and seemed to sweat out of the abdomen many little drops of liquor, which being united, trickled down the glass like a little stream, which made at the bottom a small pool of clear liquor, amounting to near a quarter of a spoonful, and by that time the receiver was ready to be taken off, the grass-hopper was fallen upon his back and lay as dead. Secondly, though having a little after laid the glass in a south window, on which the sun then shone, I perceived some slow motions in the thorax, as if he strained to fetch breath; yet I was not sure they were not convulsive motions, and whatever they were, they lasted but a while, and then the animal appeared to be quite dead, and to continue so for three hours from the removal of the receiver. Thirdly, that time being expired, the glass was opened, and the air let in upon him; notwithstanding which there appeared no sign at all of life; but imagining there might be some time requisite to recover him out of so deep a swoon, I let the glass rest in a convenient posture, that the water, that came from him might not endanger him, for a quarter or half an hour; and though I then perceived no signs of life, yet being desirous to pursue the trial yet further, I caused him to be carried into a sun-shiny place, where the beams of a declining sun presently began to make him stir his limbs, and, in a short time brought him perfectly to life again.

EXPERIMENT V.

April 15. WE took one of those shining beetles they call rose-flies, and having included it in a very small round receiver, which we exhausted; and though he that attended the engine, affirmed, it struggled much whilst the air was withdrawing, yet presently after I could perceive but little motion, and part of that seemed almost convulsive; and afterward going abroad, and not returning to look on the glass till about six hours after, the fly seemed quite dead, and discovered not any motion upon that of the glass. And within about an hour after, though I let the air rush in, yet no sign of life ensued, neither immediately, nor for a pretty while after. So that suspecting the fly to be really dead, and yet not resolutely concluding it, though I would then wait no longer, yet three or four hours after (viz. about 10 of the clock at night) I returned to the receiver, and found the beetle lively enough. Whereupon I caused the glass to be again exhausted, and secured from the ingress of the air, during which time the animal seemed to be much disquieted by what was done to it, but did not lose its motion before I went to bed, which was soon after.

EXPERIMENT VI.

ABOUT butterflies, I remember, I made several trials, most of which chanced to be lost; but thus much I very well remember, that having observed them not only to live, but to move longer than was expected, I chose to include divers of them in receivers somewhat large, especially, that I might see, whether in so thin a medium some or other of them, by the help of their large wings, would be able to fly. But though, whilst the air continued in the glasses, they flew actively, as well as freely, up and down; and though after the exhaustion of the air, they continued to live, and were not moveless; nay, though at the bottom of the receiver they would even move their wings, and a little flutter, yet I could not perceive any of them to fly, by which I mean, perform any progressive motion, supported by the medium only. And by frequently inverting the receiver (which I took care should be pretty long, to let them fall from one extream to the other,) they would fall like dead animals without displaying their wings, though just as they came to touch the bottom, some of them would sometimes seem to make some use of them, but not enough to sustain themselves, or to keep their falls from being rude enough.

TITLE XX.

Of the necessity of air to the motion of such small creatures, as ants, and even mites themselves.

" IN the experiments hitherto mentioned, " the animals, on which the trials have been " made, were divers of them of a moderate " bulk; and others of them, though small, yet " not of the least sizes, that nature afforded us. " Wherefore I thought fit to annex the following experiments, wherein I designed to " examine, whether even those minute sorts of " animals, whose bulk is thought the most " contemptible, have not, as well as the greater, " need of the air, if not to make them live, yet " at least to enable them to move."

A pretty number of ants were included in a small portable receiver exhausted yesterday about noon: between six and seven in the afternoon, they seemed to be all quite dead, and the rather, because, though they were very lively just before they were sealed up, running briskly up and down the bubble they were in; yet they grew almost moveless as soon as the air was exhausted; and a little while after appeared more so: though I then suspected more than I since did, that they were much inconvenienced by some small glutinous substance, that seemed to have got into the small receiver from the vapours of the cement. When I looked on them at the time lately mentioned, I opened the glass, whereupon the air rushed in; but no sign of life appeared for a great while in any of the ants: but looking upon them this morning about 9 a clock, I found many of them alive, and moving to and fro.

" IT is said by naturalists upon the authority " of *Aristotle*, that the animal, the Greeks call " ἄκαρι, is the minutest of living creatures. But " those of this sort being very hard, if at all, " to be met with here, I thought fit to make " some experiments upon the least of the terrestrial animals I could procure, and try, whe- " ther

"ther or no mites themselves, which are reputed but living points, and not to be taken notice of by the naked eye to be living, but by motions, which even an attentive one can scarce discover, stand in need of the air; especially, because, in case they do, it may suggest to us some odd reflections upon the strange subtilty and minuteness of the aerial particles, which must be capable of flowing in, and passing out, at the invisible, and almost in-imaginable small pores, and other cavities of the parts of an animal, whose entire body is reputed but a physical point."

We conveyed then a pretty number of mites, together with the mouldy cheese they were bred in to nourish them, into three or four portable receivers (which were all of them very small,) not much differing in size. From all of these, save one, we withdrew the air; and then, making use of our peculiar contrivance to hinder its return, we took them one after another from the engine, and laid them by, for further observation. That one, which I took notice that we had reserved, and, in which, to observe the difference, we thought fit to leave the air, was sealed at a lamp-furnace, after the usual manner of nipping up glasses there. This done, there remained nothing but to observe the event of our trials, which afforded us the ensuing phænomena.

1. Those mites, that were inclosed in the small glass, that never came near the engine, continued alive, and able to walk up and down for a full week, after they had been put in; and possibly would have continued much longer, if the glass had not been accidentally broken.

2. As soon as ever one of the receivers was removed from the engine, I looked with great attention upon it; and though just before the withdrawing the air, the mites were seen to move up and down in it; yet, within a few minutes after, the receiver was applied to the engine, I could discern in them no life at all, nor was any perceived by some younger eyes than mine, whereunto I exposed them. Nay, by the help of a double convex-glass (that was so set in a frame, as to serve me as a microscope on such occasions) I was not able to see any of them stir up and down. Nor was any motion taken notice of in the other small receiver of like bigness and shape with mine, by them that had exhausted it of air. And my occasions not permitting me to attend the observation any longer in the place where it was made, I took the receiver, I had so attentively considered myself, along with me in the coach; and having occasion to make some stay about an hour after, I looked upon it attentively again, but could not perceive any of the mites to stir; and the like unsuccessful observation I made, when I had a conveniency two or three hours after that. And the place I did it in, being one, where I thought myself, as it were at home, I first let in the air, to try if the mites were not quite dead; and though neither upon its rushing in, nor during my stay there, I could perceive any of them to stir; yet I left the receiver unstopped, as it was in the window, upon a suspicion, that the air might not be able to produce its operation upon them in a short time.

3. And therefore passing by the same place about two or three days after, I called in to look upon my receiver, and found a number of my little animals revived, as an attentive eye might easily perceive by the motion of certain little white specks, when it was helped to observe it by little marks, I made on the outside of the glass (which was purposely chosen thin and clear) near this or that mite, with a diamond; by the approach to, or recess from, which marks, the progressive motion became (perhaps within a minute) plainly discoverable; especially, if we used the following expedient, (which I found the best of those I tried,) namely, that when the eye perceived little white specks, that looked like mites, the receiver should be so turned and returned, that the bellies and feet of those little creatures were uppermost; notwithstanding which, they would not easily drop down, but continue their motion; which specks being made upon the concave surface of the thin glass itself (to which you may approach your eye as much as you please) are thereby rendered much more easily visible. But, this being only intimated upon the by, I proceed to take notice, that in the newly mentioned receiver, the mites did, by stirring up and down, continue to appear alive for two or three days after, if not longer. I should not, I confess, have thought it ridiculous to suspect, that the mites, which at first lost their motion, did at last really die; and that those, I after saw stirring up and down, were others newly generated in the included mouldy cheese: but I was not apt to think this suspicion probable; not only because of the extreme difficulty of making any living creature to be generated in *Vacuo Boyliano*, but because it did not seem agreeable to what I elsewhere noted, about the way and time of the propagation of mites, whose eggs I have divers times observed with pleasure, that at a season of the year, that was not favourable (for these things happened in a cold *March*,) newly generated mites should in two or three days grow up to their just bigness, which several of those we observed, seemed to have attained.

4. But, because it doth not by the third phænomena appear, whether or no, in case our mites had been kept in a moveless state for a much considerabler time, than three or four hours, they would have been recoverable by the admission of the air; I shall add, to satisfy that doubt, that one of the portable receivers above-mentioned, being exhausted, and carefully secured from the regress of the air, was kept from monday morning to thursday morning: after all which time, our attentive eyes being unable to discover any signs of life among the included mites, the air was let in upon them, and after no long time, had such an operation upon them, that both I, and others, could plainly see them creep up and down in the glasses again.

SOME

SOME

CONSIDERATIONS

TOUCHING THE

USEFULNESS

OF EXPERIMENTAL

NATURAL PHILOSOPHY.

Propofed in a familiar Difcourfe to a Friend, by way of Invitation to the ftudy of it.

The Second Tome, containing the latter Section of the Second Part.

The PUBLISHER to the READER.

WHEREAS the preface of the noble author to this fecond tome of the ufefulnefs of experimental philofophy, was written with defign it fhould come forth a year or two before the laft, it is fit, that fomething be now added about the prefent publication.

FIRST, if inquiry be made, why the effays, that now come abroad, are not accompanied with thofe others, that according to the forts of the titles, fhould precede fome of them ; he reprefents, that it was not thought fit, that thofe, that are now publifhed, having no neceffary dependance on the reft, and being fufficiently intelligible without them, fhould ftay for difcourfes, that are not at prefent ready, and perhaps will not fuddenly be fo; partly, in regard they confift of no fmall number of loofe papers, which by reafon of fome, yet infuperable, obftacles (of which want of health is none of the leaft) he cannot conveniently feek out, range, and compleat; and partly, becaufe he cannot, in the place where he is now detained, be mafter of divers uncommon minerals, and fome chymical productions, whofe defcriptions through hafte he omitted, becaufe he had them at hand in the place, where thofe effays were written, and prefumed, he could at leifure fill up thofe vacancies he left for fuch defcriptions.

SECONDLY, as to the effays themfelves, which, for the reafons juft now mentioned, come not abroad with the reft, though the excellent author hath of late years conftantly refufed to promife any thing to the publick, yet, that the reader may the better judge of the fcope and defign of the whole treatife, he will not deny him an intimation of what fubjects thofe effays relate unto, by telling him, that one of them treateth of the ufefulnefs of chymiftry (not to phyfick, but) to the empire of man over the inferior works of nature: another, of the advantages, that a naturalift's country may derive from his curiofity: another, of the mutual affiftance, that the fpeculative and practical part of phyfiology may afford each other: after which comes a difcourfe, containing inducements to hope for much greater things from experimental philofophy, than men have hitherto obtained.

LASTLY, as to what the author taketh notice of, about the coincidents of fome experiments, that may be mentioned as well by others as by him; it is very poffible, that the fame things may, by the fame, or other ways, come to the knowledge of different perfons. Befides, that I have heard him mention with fome complaint, that, when divers years fince, he writ feveral difcourfes (whereof fome belonged to the ufefulnefs of experimental philofophy,) for the ufe of a private friend, not for the prefs, he was not fo fhy, as had been requifite, of fhewing divers experiments, and of imparting others in difcourfe, to inquifitive men, whether Englifh or Foreigners, that came to vifit him; divers of which things he afterwards found in print, fometimes indeed with, but for the moft part without, mention of his name. So, that fometimes his unwillingnefs to difoblige fuch writers, and to contend about fuch matters, made him either wholly omit fome of the particulars he afterwards intended to publifh, or even to crofs out feveral paffages, that he had already written, where he would, without much inconvenience (for, that did not always happen) either quite leave them out, or fubftitute others (though lefs proper) in their ftead. He added alfo, that fometimes obferving his notions and experiments to be afcribed to other writers, and fomewhat wondring at it, he found indeed fuch writers to have mentioned fuch things, but in editions, that came abroad after the publication of our author's writings; from whence

whence such things might, with the greater likelihood be presumed to have been borrowed, both because some of the writers had conversed with him, and he could not find them in the first edition of such books. But these unfair proceedings being the faults but of a few, he said, he was far from imputing them to the generality of those, that have mentioned, (which divers of those have very civilly done,) his experiments, or writings in theirs.

THE particulars being thus taken notice of, the curious reader ought not to be any longer detained from conversing with the author himself in this instructive treatise. Farewel.

The PREAMBLE.

I HAVE, in the preface, and body of the former, and already published part of this treatise, taken notice of so many of the things, that concern the whole work in general, that I presume it will not here be necessary to detain the reader with any other particulars, than those, that will be offered by way of answers to some questions, that are like to be asked about the publication of this present tome.

AND in the first place, if it be demanded, why this latter part did not more closely follow the former, I have this to answer; that the papers it consisted of, chanced to be so unfortunately disposed of, during the late publick confusions, that for a great while I was not the master of them, and in the mean while was, sometimes upon one occasion, and sometimes upon another, engaged to venture abroad the History of Colours, the History of Cold (with the preliminary and additional tracts) Hydrostatical Paradoxes, and the Origin of Forms and Qualities; the publication of which treatises, besides that of some anonymous papers, as it took up much of the time I had to spare for the press; so it may, I suppose, keep it from being thought strange, that I did not trouble myself and others with this book also. And indeed, this having been (as the scope and divers passages of it sufficiently intimate) one of the first I wrote to the gentleman I call *Pyrophilus*, I had occasion, whilst it was out of the way, to make use of so many of the experiments and observations, that belonged to it, that fearing I had thereby too much robbed, and disfigured it, to leave it any way fit for publick view, I had the greater temptation to neglect the looking after it.

BUT if it be further demanded, why then, since it was not ready to come out more early, I did not condemn it not to come out at all? I have two things to return by way of answer.

THE first is, that some eminent virtuosi, to whom I owe a peculiar respect, were pleased to challenge the edition of this tome, as if I had made myself a debtor to the publick for the second part of this work, by having suffered what I wrote to a private friend to be divulged in the first. Especially since the publick had given that so very favourable an entertainment; as, besides other things, the early reprinting of it manifested.

THE other part of my answer, and that, which made the former consideration prevalent, is, that I was overcome, either by the reasons, or by the authority, of those ingenious persons, that were pleased to think, that this work would not prove unserviceable to mankind, to whose good, both as a man, and Christian, I have been long ambitious to contribute, as well upon the account of the great author and divine redeemer of men, as of that common nature, whereof all men partake. What the utilities of this work were conceived to be, the reader will find disclosed at the end of this preface. To which I will therefore refer him for an account of them; and now only take notice, that as to one of the scruples I had against the publication, namely, that I had plundered this present treatise of divers particulars, wherewith I had accommodated some of my other writing; I could not well reject this answer, that in so many years as had passed since the writings of this book, I had not been so negligent a commercer with the works of nature and art, as not to be able to make some amends for what I had taken away, and easily substitute other experiments and observations, to supply the vacancies, left by those I had transferred to other discourses.

AND as to another of my scruples, about venturing abroad this tome, namely, that it must come forth so late, if it should come forth at all, it was answered, that it could scarce come forth more seasonably to recommend the whole design of the Royal Society, whose generous aims being to promote the knowledge of nature, and make it useful to human life. This treatise may procure them some number of assistants, in a work, whose vastness and difficulty will need very many, if men's curiosity and industry can by this treatise (or any to the like purpose) be well excited by a conviction of the real and wide disparity betwixt true natural philosophy, and that of the peripatetick schools; and that in cultivating the former, they will not meet with a field, that will afford them nothing, but (the wonted production of the latter) the thorns and thistles of acute indeed, but useless, and oftentimes troublesome, subtilties; but, that they may expect a soil, that may by a due culture be brought to afford them both curious flowers to gratify curiosity, and delight their senses, and excellent fruits, and other substantial productions, to answer the necessities, and furnish the accommodations of human life.

AND I will not deny, that I have had the fortune to be looked upon, as not the unfittest person in the world to offer something in this kind; for those, that are meer scholars, though never so learned and critical, are not wont to be be acquainted enough with nature and trades, to be able to suggest those instances, that are the most proper to manifest that, which men are to be convinced of. The meer chymists, besides that their curiosity is wont to be too much confined to let them be fittest for such a work, have the ill fortune to be distrusted by the generality of men, not credulous, which is a great unhappiness in this case, because, that though their experiments were never so true (as divers of them are) yet skill in their art being requisite to make them, men's diffidence of the proposers, joined with the difficulty of examining the things, will not allow them, either to believe what is proposed, or to try it. And as for the new philosophers (as they call them) though, if they were to write but for philosophical readers, I know several of them, that would questionless do it rarely well; yet the generality of those readers, to whom we would give good impressions of the study of nature, being such as will probably be more wrought upon by the variety of examples, and easy experiments, than by the deepest notions, and the neatest hypotheses, such a treatise for the kind, as that which follows, containing many practices of artifices and other particulars, that are either of easy trial, or immediate use, may perhaps by that variety gratify, and perswade a greater number of differing sorts of readers, than a far more learned and elaborate piece, that might be welcomer to more intelligent and philosophical perusers.

If it be asked by some, that know me, whence it comes, that the second part of the usefulness of experimental philosophy being written (as very credible persons, that saw it can witness) about the year 1658, there may be met with in the following treatise some experiments of my own, that they know were since made, and some (though few) citations out of books published since that time? If, I say, this be asked, the answer is intimated a little above; for having transferred to other tracts many passages, that belonged to those I now publish, I was obliged to repair the injury I had done them, by supplying them with such particulars, as offered themselves to my memory, when I hastily reviewed this tome, without scrupulously minding the times, when the particulars inserted did first occur. And if this advertisement be applied to some other of my writings, that either the importunity of friends, or some unwelcome accidents, engaged me to publish out of their due time, and not in their intended order; it may keep men from thinking, that when I first wrote them, I had read over, or at least seen, (which indeed I neither did nor could) every book of a recenter date, of which upon occasion I mention a passage or two, and those perhaps, as they are cited by other authors, we being here in *England* but slenderly, and very slowly, furnished with modern foreign books.

All these inserted passages the reader should find included in parenthesis, as the printers call these marks (), by which he will yet be able to distinguish several of them, though I now find, that some others, by the negligence of the transcribers, or of the press, or of both, have been omitted; which advertisement I fear may have need to be extended to some other printed tracts of mine, wherein parenthesis are to be met with.

Bating these few additional passages, the ensuing book comes forth, without taking notice of what changes or discoveries have happened in the common-wealth of letters, since the time it was written in. On which account, if some few of those many particulars delivered there should chance to be co-incident with what some other man hath written, I would neither on the one side be thought a plaguary myself, nor on the other side deny any man, to whom it may be due, the honour of the earliest publication; though, to shun needless controversies, I am somewhat shy of naming this or that person, as the first proposer or inventor of an experiment, which, especially if the persons or things be not considerable, is often difficult enough to discover: witness the contests, that have been, and yet continue, about the first inventors of common weather-glasses, the ascension of water in slender pipes, the glass drops that fly in pieces, the measuring of time by a pendulum, and, which is more strange, the art of printing itself. If it be asked, why I did not forbear to make use of some practices of tradesmen, and other known, and perhaps seemingly trivial, experiments; these things may be replied.

I. That since on divers occasions it was requisite, that my discourse should tend rather to convince, than barely to inform my reader, it was proper, that I should employ at least some instances, whose truth was generally enough known, or easy to be known, by making enquiry among artificers, even by such as out of laziness, or want of skill, or accommodation, cannot conveniently make themselves the trials.

II. But yet I have taken care, that these should not be the only, nor yet the most numerous instances, I make use of; it being in this tome, as well as in my other physiological writings, my main business, to take all just occasions, to contribute as much, as without indiscretion I can, to the history of nature and arts.

III. As to the practices and observations of tradesmen, the two considerations already alledged may both of them be extended to the giving of an account of the mention I make of them. Of the truth of divers of the experiments I alledge of theirs, one may be easily satisfied, by inquiring of artificers about it; and the particular, or more circumstantial accounts I give of some of their experiments, I was induced to set down by my desire to contribute toward an experimental history. For I have found by long and unwelcome experience, that very few tradesmen will, and can give a man a clear and full account of their own practices; partly out of envy, partly out of want of skill to deliver a relation intelligibly enough, and partly (to which I may add, chiefly) because they omit generally, to express either at all, or at least, clearly some important circumstance, which becauſe long uſe hath made very familiar to them, they preſume alſo to be known to others; and yet the omiſſion of ſuch circumſtances doth often render the accounts they give of ſuch practices, ſo dark and ſo defective, that, if their experiments be any thing intricate or difficult (for if they be ſimple and eaſy, they are not ſo liable to produce miſtakes) I ſeldom think my ſelf ſure of their truth, and that I ſufficiently comprehend them, till I have either tried them at home, or cauſed the artificers to make them in my preſence.

THEY that have given themſelves the trouble of endeavouring to make the experiments of tradeſmen, to be met with in the writings of *Cardan*, *Weckar*, and *Baptiſta Porta*, for inſtance; and have thereby diſcovered (what is not uſually obvious upon a tranſient reading) how lamely and darkly, not to add unintelligibly, ſeveral things are written, will probably afford me their aſſent, having found upon trial the inſtructions of ſuch learned and ingenious men to be often obſcure and inſufficient for practice.

BUT here I muſt give the reader notice, that as mechanical arts for the moſt part advance from time to time towards perfection; ſo the practices of artificers may vary in differing times, as well as in differing places, as I have often had occaſion to obſerve. And therefore I would neither have him condemn other writers or relators, for delivering accounts of the experiments of craftſmen differing from thoſe I have given; nor condemn me, for having contented my ſelf to ſet down ſuch practices faithfully, as I learned them from the beſt artificers (eſpecially thoſe of *London*) I had opportunity to converſe with.

BUT here perhaps it will be demanded by way of objection, whether I do not injure tradeſmen, by diſcovering ſo plainly thoſe things, which our laws call the myſteries of their arts. To a queſtion, that may perhaps by ſome be clamorouſly preſſed, not only upon me, but much more upon ſome ingenious men of our nation, whoſe pens have been more bold than mine in diſcloſing craftſmens ſecrets, it will be requiſite to return ſeveral things by way of anſwer; but that ſuch readers, as are not troubled with the ſcruple, may not be ſo with the apology, they will find this printed in another character, ſo that, if they pleaſe, they may paſs it over unread.

In the preſent edition, theſe paragraphs are included in crotchets.

[FIRST then, it may be repreſented, that I never divulge all the ſecrets and practices neceſſary to the exerciſe of any one trade, contenting myſelf to deliver here and there, upon occaſion, ſome few particular experiments, that make for my preſent purpoſe: ſo that, for much more than I allow my ſelf to do, I can plead the example, not only of other writers, that have publiſhed books to teach the whole myſtery of this, or that trade, as the prieſt *Antonio Neri* hath diligently done in his Italian *Arte Vetraria*, and ſome Engliſh, as well as foreign, virtuoſi have done on other ſubjects; but alſo ſome of the artificers themſelves, as the famous gold-ſmith and jeweller *Benvenuto Cellini* in his much eſteemed Italian tracts of the lapidaries and goldſmiths trades. Thus alſo the famous mineraliſt *Georgius Agricola* publiſhed in Latin a whole volume of the more practical part of mineralogy, wherein he largely and particularly deſcribes experiments, tools, and other things, that belong to the callings of mine men. To which I might add divers other treatiſes, ſome of them French, others Italian, (which, though I could not procure them, I have ſeen among curious collections of books) that have been publiſhed about ſeveral arts by the artificers themſelves. And it is notorious, that in Engliſh, as well as in divers foreign languages, we have books of the arts of gunnery, diſtillation, painting, gardening, &c. divulged by perſons, that profeſſed thoſe callings.

SECONDLY, it is not the cuſtom of tradeſmen to buy books, eſpecially ſuch as are not intended for ſuch readers, and treat (for the moſt part) of things, either beyond their reach, or wherein they ſeem not likely to be concerned; and as for gentlemen and ſcholars, though ſome of them may, to ſatisfy their curioſity, make a few trials, yet their doing ſo will ſcarce in the leaſt be prejudicial to tradeſmen. Since, to omit other arguments, it will not be worth while for a virtuoſi to be at the charge and trouble of buying tools, and procuring other neceſſary accommodations to ſell a few productions of his ſkill, though he ſhould not ſcruple to deſcend to ſuch a practice. For if he make but a ſmall number of experiments, their effects will coſt him more than the like may be bought for of thoſe, that make them in great quantities, and whom their trade obligeth to be ſollicitous to buy their inſtruments and materials at the beſt hand, and ſell them to the beſt profit. Beſides that moſt of the works of artificers are chiefly recommended to the more curious ſort of buyers by a certain politeneſs, and other ornaments, compriſed by many under the name of finiſhing; which require either an inſtructed and dexterous hand, or at leaſt ſome little peculiar directions, which I did not always think my ſelf obliged to mention, in a treatiſe deſigned to aſſiſt my friend to become a philoſopher, not a tradeſman, and publiſhed to help the reader to gain knowledge not to get money.

THIRDLY, to publiſh an experiment or two, or in ſome caſes, a much greater number belonging to a trade is not ſufficient to rob a tradeſman of his profeſſion. For beſides that moſt trades conſiſt of ſeveral parts, and are each of them made up of divers practices, (that commonly are more than a few) thoſe numerous mechanical arts, that are called handicrafts, require (as their very name argueth) a manual dexterity, not to be learned from books, but to be obtained by imitation and uſe. And to theſe conſiderations I ſhall add this more important one, that mechanical profeſſions are wont to be, as it were, made up of two parts, which, for diſtinction ſake, I take leave to call the art and the craft; by the former whereof I mean the ſkill of making ſuch or ſuch things, which are the genuine productions of the art, (as when a taylor maketh a suit, or a cloak,) and by the latter I mean the result of those informations and experiments, by which the artificer learns to make the utmost profit, that he can, of the productions of his art. And this oeconomical prudence is a thing very distinct from the art itself, and yet is often the most beneficial thing to the artificer, informing him how to chuse his materials, and estimate their goodness and worth; in what places, and at what times, the best and cheapest are to be had; where, and when, and to what persons the things may be most profitably vended. In short, the craft is that, which teacheth him how both to buy his materials and tools, and to sell what he makes with them, to the most advantage.

FOURTHLY, it may often prove more advantageous than prejudicial to tradesmen themselves, that many of their practices should be known to experimental philosophers. This I suppose, that I have sufficiently proved in some, and especially in * one of the following essays.

YET I shall now represent, that though some little inconvenience may happen to some tradesmen by the disclosing some of their experiments to practical naturalists, yet, that may be more than compensated, partly, by what may be contributed to the perfecting of such experiments themselves, and, partly by the diffused knowledge and sagacity of philosophers, and by those new inventions, which may probably be expected from such persons, especially if they be furnished with variety of hints from the practices already in use. For these inventions of ingenious heads do, when once grown into request, set many mechanical hands a work, and supply tradesmen with new means of getting a livelihood, or even enriching themselves. As to the discipline subordinated to the pure mathematicks, this is very evident; for those speculative sciences have (though not immediately) produced their trades, that make quadrants, sectors, astrolabes, globes, maps, lutes, vials, organs, and other geometrical, astronomical, geographical, and musical instruments; and not to instance those many trades, that subsist by making such things as mechanicians, proceeding upon geometrical propositions, have been the authors of; we know, that whether the excellent *Galileo* was, or was not, the first finder out of telescopes, yet he improved them so much, and by his discoveries in the heavens did so recommend their usefulness to the curious, that many artificers in divers parts of *Europe* have thought fit to take up the trade of making prospective glasses. And since his death, several others have had profitable work laid out for them, by the newer directions of some English gentlemen, deeply skilled in dioptricks, and happy at mechanical contrivances; insomuch, that now we have several shops, that furnish not only our own virtuosi, but those of foreign countries, with excellent microscopes and telescopes, of which latter sort I lately bought one (but I confess the only one, that the maker of it, or any man, that I hear of, hath perfected of that bigness) which is of threescore foot in length, and which the ingenious artist, that made it, Mr. *Reeves*, prized constantly at no less than an hundred pounds English money. I know not, whether or no I should add, that possibly some particular experiments of mine have not been hitherto unprofitable to several tradesmen. But this I may safely affirm, that a great deal of money hath been gained by tradesmen, both in *England* and elsewhere, upon the account of the scarlet dye, invented in our time by *Cornelius Drebble*, who was not bred a dyer, nor other tradesman. And, that we daily see the shops of clockmakers and watchmakers more and more furnished with those useful instruments, pendulum clocks, as they are now called, which, but very few years ago, were brought into request by that most ingenious gentleman, who discovered the new planet about Saturn.]

I have handled the subject of the foregoing arguments much more particularly, than I would have done, had not my pen been drawn on by a hope, that the things I have represented, may furnish apologies to many inquisitive men, who may be thereby enboldened to carry philosophical materials from the shops to the schools, and divulge the experiments of artificers, both to the improvement of trades themselves, and to the great inriching of the history of arts and nature.

IF it be further demanded, whether I have furnished these essays with the chiefest things I could have afforded them, I must confess, that I have not; for though I had, lying by me, several experiments and observations, less inconsiderable than many of those I have made use of, which would have been pertinent enough to the subjects here treated of; yet I purposely forbore to imploy them in these tracts, because I would not defraud those others, to which they were more proper, and some of them necessary. For I freely declare, that my design in this present tome was not to furnish it as well as I could, but to preserve, as in a repository, several scattered experiments and remarks, which I could best spare from the other treatises I had designed, which might otherwise probably be lost: but yet I shall not deny, that I did not carelessly draw up some of the following tracts, but, that I endeavoured to write them in such methods, that they might contain several distinct heads, and those as comprehensive as I could easily make them, that both the young and hopeful gentleman, I call *Pyrophilus*, and I myself, might conveniently refer such other practices and experiments (especially those of tradesmen) as should hereafter occur to us, and appear to belong to those heads. And I did the less despair of his giving a kind reception to these discourses, because I could expect so little assistance in my undertaking, having never met with any book, great or small, written upon the subject I was to treat of.

IF hereupon it be objected, that by my own confession, divers of the particulars admitted

* The Essay here meant is that, which treats of the utility of the naturalists insight into trades.

mitted into this book, are but ſlight, and ſome of them already known; I ſhall repreſent, that as ſome of the experiments ſpoken of are but ſlight, ſo there are others, that poſſibly diſcerning readers will not think to be altogether ſuch; and that it was fit (for reaſons already mentioned in this very preface) that I ſhould not forbear to employ, as proofs to convince others, things either known, or eaſy to be made ſo, eſpecially, ſince I commonly uſe them to ſome purpoſe, or other, whereto they have not been applied; and my deſign in the publication of theſe trifles being chiefly to invite the generality of readers, though of different inclinations, qualities, &c. to addict themſelves to the ſtudy of experimental philoſophy. The variety and eaſineſs I have aimed at in the experiments I have ſet down, may, for ought I know, be more proper, than if I had confined myſelf to the mention of a few choice and elaborate experiments, which ſome readers would think impertinent to their ſtudies, and others judge too difficult for them to put in practice. It appeared not unfit, that a book, whoſe title was like to procure it very different ſorts of readers, ſhould be for the moſt part written in a popular way; divers perſons, eſpecially thoſe of a higher quality, by a trifle, that hath the luck to gratify their curioſity, may be more ſucceſsfully invited to reliſh and eſteem experimental learning, than by a deep notion, or a weighty experiment. And there are others, that will eaſier be brought to value and try experiments, by meeting with ſome few, though but ſlight ones, that happened to ſuit with their humour or calling, or to accommodate them on ſome particular occaſions, than they would by many others, much more luciferous, or otherwiſe important. And though it were to be wiſhed, that men's kindneſs to practical philoſophy were grounded on the beſt motives; yet this treatiſe will not altogether miſs the aim of its publication, if even upon the fore-mentioned ſlighter accounts, it engages readers to make, as well as reliſh, experiments; for the pleaſantneſs, variety, uſefulneſs, and other indearing qualities of ſuch an imployment will probably invite moſt of them to a further progreſs, whereby many uſeful phænomena and obſervations are like to accrue to what is already known of the hiſtory of nature and arts. And if this ſhall come to paſs, it will keep him from complaining of labour loſt, who in venturing upon ſuch a work, as now comes forth, was knowingly to poſtpone the appetite of fame to the deſire of doing ſome ſervice to mankind; to which end he takes one of the directeſt ways to be the contributing ſomewhat to the advancement of experimental philoſophy.

It remains, that I add ſomething more, which poſſibly may not a little befriend both theſe laſt mentioned anſwers, and ſeveral others contained in this preface; for, when all the former demands occurred to my thoughts, as likely to made, ſome by one ſort of readers, and ſome by another, thoſe virtuoſi, that were ſollicitous for the publication of theſe papers, were not backward to urge the utilities, which they fancied would thence accrue to the publick. And I cannot very well deny, that, as meanly as I think of a treatiſe, to whoſe tome I did not, till the ſecond edition, (when I could conceal it no longer) let my name be prefixed; yet ſuch a work as this for kind, well performed, may be a very uſeful one. And even of this following book, ſuch as it is, it was ſuggeſted, that the uſes would not prove deſpicable, in regard, that beſide thoſe, that are common to it with the formerly publiſhed tome, ſuch as the improvement of the minds of men, and (eſpecially) the aſſiſting them to underſtand the works of God, and thereby engage them to admire, praiſe and thank him for them: beſides theſe (I ſay) there may be other uſes of the following tome, which, to avoid increaſing a prolixity, that I fear is already too great, I ſhall rather name than diſcourſe of, contenting myſelf briefly to intimate, that it was conceived, the peculiar uſes of this preſent tome might be ſuch as theſe.

I. It may afford materials for the hiſtory of nature, which that it may the more plentifully do, I have purpoſely, on ſeveral occaſions, added a greater number of inſtances, than were abſolutely neceſſary, for the making out of what I intended to declare or prove.

II. It may afford ſome inſtructions, advices, and hints to promote the practical or operative part of natural philoſophy in divers particulars, wherein men have been either not able, or not ſollicitous to aſſiſt the curious.

III. It may enable gentlemen and ſcholars to converſe with tradeſmen, and benefit themſelves (and perhaps the tradeſmen too) by that converſation; or, at leaſt, it will qualify them to aſk queſtions of men, that converſe with things; and ſometimes to exchange experiments with them.

IV. It may ſerve to beget a confederacy, and an union between parts of learning, whoſe poſſeſſors have hitherto kept their reſpective ſkills ſtrangers to one another; and by that means may bring great variety of obſervations and experiments of differing kinds into the notice of one man, or of the ſame perſons; which how advantageous it may prove towards the increaſe of knowledge, our illuſtrious Verulam has ſomewhere taught us.

V. It may contribute to the reſcuing natural philoſophy from that unhappy imputation of barrenneſs, which it has ſo long lain under, and which has been, and ſtill is, ſo prejudicial to it. And to effect this reſcue, it will in ſome meaſure enable thoſe, that deſire it, to employ thoſe practical arguments, that are proper to convince many, that are not to be convinced by any other ſort of proofs.

VI. And which is the main of all, it may ſerve by poſitive conſiderations, and directions, to rouſe up the generality of thoſe, that are any thing inquiſitive, and both loudly excite, and ſomewhat aſſiſt, the curioſity of mankind; from which alone may be expected a greater progreſs in uſeful learning, and conſequently greater advantages to men, than in the preſent ſtate of human affairs will be eaſily imagined.

OF THE USEFULNESS OF EXPERIMENTAL PHILOSOPHY.

The SECOND PART. The SECOND SECTION.

Of its USEFULNESS to the Empire of MAN over inferior Creatures.

ESSAY I.

Containing some general considerations about the means, whereby Experimental Philosophy may become useful to human life.

HITHERTO, my dear *Pyrophilus*, I have attempted to satisfy you of the usefulness of experimental natural philosophy to physick: it follows, that I proceed to endeavour to shew you, that it may be also very serviceable to husbandry, in all its subordinate parts, and to those other professions, that serve to provide men with food and rayment, or do otherwise minister to the necessities or accommodations of life; as the trades of brewing, baking, fishing, fowling, building, and the rest not needful here to be enumerated. For though the human body, in respect of the rational soul, (which is the inventress and seat of sciences) be one of the corporeal things, over which the empire of knowledge is to be established; yet taking man as a creature made up of body and soul, the advancement of his empire seems to consist more properly in the inlargement of his power over the other creatures: physick seeming rather to defend him against revolts and insurrections at home, than to increase his power, and extend the limits of his empire abroad.

BUT, *Pyrophilus*, I hope, you do not expect, that I should now insist on each, or so much as on any of the above-mentioned trades, by whose intervention it is, that man exercises his dominion over external bodies. For such a work would require little less than an age, and much more than a volume; and besides (that it is vastly disproportionate, both to my slender stock of mechanical skill, and to the little leisure I have to conclude this section in) I could not acquaint you with all that I could pertinently enough deliver about these matters, without too much defrauding some other treatises, that I design you: and therefore, I hope you will be content, if, in the remaining part of this tract, I do not only present you a not despicable number of considerations proper to manifest that, and to intimate, how experimental philosophy may be of great use to the promoting of mechanical arts and trades, but illustrate and confirm all, or most of those considerations by particular instances, derived from observations and experience.

THIS I shall, God assisting, endeavour to do in the following essays. But before I descend to particulars, it will be expedient in this place to premise some general considerations relating to the influence of experimental philosophy upon trades; and two or three advertisements, that concern the ensuing discourses.

SECTION I.

FIRST then, to make it probably, that a true insight into natural philosophy may be capable of affording some reformation, or other kind of improvement to trades, I shall desire to consider, that being, for the generality of them, conversant about some few particular productions of nature, such men as are thoroughly skilled in her general laws, and acquainted with a vast number of her productions, and versed in the ways of applying nature and art jointly to several purposes, according to the several exigencies of things; such sagacious persons (I say) will, in all likelihood, be able, some way or other, to meliorate the inventions of illiterate tradesmen. As the husbandman's skill, for instance, consisting chiefly in the observations of the nature of a few plants and animals, their relation to such and such soils and kinds of culture, and the operations of stars and meteors upon them, which are subjects, that properly enough fall within the cognizance of the naturalist, it cannot seem improbable, that he, that has seriously and industriously inquired into the nature of generation, nutrition, and accretion, both in plants and animals, and knows how to vary an useful experiment, when once found out, so as to remedy the inconveniencies, or supply the deficiencies, or improve the advantagiousness, or translate and apply the use of it, and, in sum, he that can knowingly and dexterously manage, what his own or other men's observations have afforded him, will be able to cultivate the ordinary husbandman's skill with a as much improvement, as that confused skill enables the husbandman to cultivate his ground.

SECTION II.

To carry on the foregoing considerations a little farther, I will add, that it may as well conduce much to the manifesting, how much trades are subordinate to natural philosophy, as to the improvement of trades themselves, that it be attentively considered, what things each particular trade is, as it were, made up of. As for example, the chief things in the refiner's trade are, to know the ways of making, and the operations of aqua fortis upon silver, gold, and copper; to know how to purge that menstruum, that it may dissolve no gold, nor precipitate any of the silver it dissolves; to know what proportion there ought to be dissolved in it; to know with what quantity of water to weaken the solution, and how long copper-plates need lie in it, to precipitate all the silver out of it; to know how lead is to be colliquated with them, and what proportion of it is necessary and sufficient to carry off with it (when it is blown off upon the test) the baser metals; to know how to make cupples of several sorts and sizes, and upon them to draw off the lead or antimony from the silver or gold, and discern when the metal is sufficiently refined; to know what proportion of gold and silver is requisite for the making of water-gold as they call it, (because it is separated from silver by aqua fortis, which dissolves this metal, and leaves the other in a fine powder;) these things, to which many others are subservient, belong to the refiner's trade, which, though understood by few, seems to be a very narrow and simple trade, in comparison of a hundred others, whose operations are far more numerous and complicated. Now if all trades were judiciously resolved (if I may so speak) into the several parts they consist of, it would, I question not, manifestly appear, that the most, if not all of them, are in many particulars but corollaries duduced from some particular physical observations, or but applications of them to the uses of human life.

And if this be so, you will not, I presume think it unlikely, that by a farther discovery of the nature of those particular bodies wherewith the trade is conversant, and a solid knowledge of those laws of nature, and those operations of bodies upon one another, which it employs; some, if not most, of those parts, whereof the trade may be conceived to be made up, may be reformed or bettered; which is enough to make the philosopher an improver of the trade, which he may become upon such unobvious accounts, that perhaps it may not unreasonably be hoped, that even the chymist's charcoal may be made, by a good naturalist, equivalent to an excellent compost for land. For if it be true, as well as it is probable, not only that the food of those animals (as oxen, sheep, &c.) which the husbandman deals with, springs out of the ground; but that the plants, which afford them this food, are themselves nourished by a certain vegetative salt they find in the ground; and, that this salt being by frequent seminations exhausted, the soil grows barren, until either by the air, or the steams of the subterraneous parts, or the spontaneous maturation of the saline rudiments contained in the ground, or by adventitious manure, or by all or divers of these together, it be re-impregnated, with a new vital saltness: if these things be true, I say, then those chymical experiments, that conduce to discover to us, what kind of salt that is, and to what other salts it is allied or opposite, as it is to several acid ones, may probably afford very useful directions to the husbandman, towards the meliorating of his land, both for corn, trees, grass, and consequently cattle. And having had the curiosity * to distil some earths, some dungs, and some seeds, and observe the salts abounding in the liquors yielding by them, (of which we have elsewhere occasion to speak) we found cause to wish, that experiments of that nature, in relation to the improvement of husbandry, might be industriously prosecuted by naturalists. He that has observed those many particulars in husbandry, which might invite that great naturalist Sir *F. Bacon* † (who yet mentions very few of them) to pronounce, that nitre is, as it were, the life of vegetables; he that observes how conducive that fertilizing dung of pigeons is, both to make earth fruitful to the husbandman, and to impregnate it with nitrous salt for the salt-petre-man; and he that knows, that most fat earths, so defended from the rain and sun, that the one may not draw up, nor the other wash down the embryonated saltness of them, will after a time abound in nitrous salt, if they are not permitted to spend any in producing of vegetables; such a one, I say, will perchance be apt to think, that enquiries into the nature of salt-petre may be of great concernment to husbandry. And to give you, *Pyrophilus*, some inducements to expect, that chemistry may be very useful in such kind of enquiries, I shall here mention to you a couple of my experiments relating to nitre.

The first is that, whereby I endeavoured to give an inquisitive person hopes, that materials, which seemed unlikely, might, by due changes, and without much art, be turned into salt-petre. The experiment was this. I caused some earth to be digged up just underneath the clay-floor of a pigeon-house; such earths being believed to abound the most with nitre, that needs only to have its particles brought together and united to compose salt-petre: a pretty quantity of this earth being put into a retort, and distilled with a good fire *ex arena*, afforded me, though little or no oil, yet a pretty quantity of a reddish liquor, which, instead of being, as others would have expected, of an acid nature like spirit of nitre, was fit for my purpose, by strongly participating

* *Verulam* hist. v. & mort. p. 237. Certissimum est quamcunque terram, licèt puram, neque nitrosis admixtam, ita accumulatam & lectam, ut immunis sit solis, neque emittat aliquid vegetabile, colligere etiam satis copiosè nitrum.

† Nat hist. cent. 5. exp. 444.

pating of the nature of volatile salts; as appeared, not only in that I could, without rectifying it, turn syrup of violets with it immediately green, and precipitate a solution of sublimate into a milky substance; but because there came over, with the spirit into the lower part of the receiver, a salt in a dry form, which not only was in taste not unlike the other volatile salts, but was so far from being of an acid nature, that with an acid menstruum it readily fell to hiss, and made an ebullition. So that it seems (which in an enquiry about nitre is very considerable,) that a salt, very repugnant to acids, may, by the operation of the earth and air, be so altered, as afterwards by a slight management to afford salt-petre, whose spirit is highly acid. But of this experiment I may hereafter make farther mention.

THE other, (which we elsewhere have occasion more particularly to take notice of with reflections on it) is briefly this. We took potashes, which you know contain but the salt of burnt vegetables; and on those, first dissolved in a little fair water, we dropped aqua-fortis (whose saline part consists indeed of little else than the spirits of nitre,) till all ebullition and hissing betwixt it and the resolved pot-ashes were perfectly ceased; and having filtrated this liquor, and set it in an open vessel in a gentle heat to evaporate, it did within two or three days after, (and sometimes, for we made it more than once, even in a few hours) being removed to a cold place, afford us very pure crystals of salt-petre, as both their shape, and flashing (on live coals) into a blue halituous flame, informed us. And since I have had occasion to mention the use of salt-petre in husbandry, I shall not forbear to add, that the knowledge, which the naturalist, as a discerning chemist, may give the husbandman of the natures and distinctions of saline bodies, may be of no mean use to him, by assisting him to discern and observe the considerablest differences of the various saltnesses to be found in soils, and what sort of saltness each particular seed or plant most affects. For by this means, not only many grounds might be made useful, which are thought barren, only by reason of our not knowing for what plants the saltness predominant in them may be proper; but the same ground may yield much frequenter crops than commonly it doth, when it is successively sowed only with one sort of seed, by the due alteration of plants delighting in the several sorts of salts to be met with in that ground; which oftentimes, by being impoverished, or rather freed from one sort of salt, doth but the more plentifully feed those plants, that delight in another: which, in some places we have observed, that husbandmen seem to have taken notice of already, by sowing (in fields too remote from their dwellings to have compost brought to them) turnips, to fit the ground for wheat, and serve for a manure; though in this method some other circumstances may possibly concur with the nature of turnip-seed, to the preparation of the ground for wheat. And I am prone to think, that there is scarce any ground or soil, except perhaps mere sand, that might not, even without much culture, be made fertile, or at least kept from being altogether barren, if we were on the one hand skilled in the ways of discerning the nature of the ground; and on the other hand acquainted with, and provided of, all the variety of seeds and plants, that nature has, though not all in one country, afforded us. For there are divers soils, which here in *England*, or in other regions, are, as useless, left quite uncultivated; which seeds or plants, that abound in other countries, and would probably be made to grow in these, would make serviceable to the husbandman. Many steep and abrupt portions of ground (some of them very large) exposed to the southern sun are left altogether waste, not only in *England*, but in divers hot climates, where the planting of grapes for wine is not yet in use; though such pieces of land in *France* and *Italy*, and, as I have observed, even in the *Rhetian Alps*, nourish excellent vineyards.

I know an ancient and landed gentleman, who communicated to me upon his own knowledge an experienced way of making wheat grow and prosper well on mere clay, where there was no grain at all did thrive; which though I have not hitherto had opportunity to try, yet upon the credit of a person so sober and qualified, I scruple not to mention it here, because the art consisting mainly in the imbibition of the seed for a determinate time in a certain expressed oil, that is not dear; it may make it probable, that without altering the whole soil by manures, a slight, but convenient change made in the seed it self may serve to make them fit for one another. And (to add, that upon the by) to shew, that the particular dispositions of some sorts of seeds may enable them to make the ground they are sowed in, much more productive, than it would otherwise be, I shall relate to you, that being not long since in the company of a learned and curious traveller, I saw, among some rarities of a quite other nature, an ear or two of corn, not much unlike our common wheat; at which being somewhat surprized, I asked him, what peculiarity had procured that grain admission among such rarities? to which he replied, that in the warmer region, where he begged it of a virtuoso, one of those grains would afford so vast a multitude, as he was almost ashamed to name, and I am more than almost afraid to repeat: but before I went out of the house, an English gentleman, that had a more than usual curiosity for such kind of trials, assured me, that having obtained some grains of that corn, and carefully sowed it in some land of his own, not far from the place we were in, he had out of a single grain several hundreds; though not near so many of them, as the other traveller, who yet was a very sober and judicious man, related to have been produced in a better climate and soil. Of this strangely prolifick wheat, the gentleman readily granted me a promise of a sufficient quantity to make a trial; whereof, when I shall have received it from a servant of mine in the country, you may command the success. And this brought into my mind what

I read in the learned Jesuit * *Acosta*, who affirms, that in divers parts of *America*, where it is known, that our European wheat prospers not, the Indian (or, as many English have stiled it, Virginian) wheat, they call *Maiz*, does so wonderfully thrive, that although the stalk bear often more than one cluster, and the grain be big; yet in some clusters he has reckoned seven hundred grains: to which he adds, that it is not strange in those countries to gather three hundred faneques, or measures, for one sown. Which passages, especially the former, speak of an increase, that seems so little credible, that I should on that account forbear to mention it, were it not, that in *Europe*, and even in *England*, I my self have reckoned such a multitude of grains upon one of the very numerous ears produced by the same single grain, that I found myself very inclinable to absolve *Acosta*, and continue to look upon him as one of the best writers of the natural history of *America*.

We now proceed to take notice, that in some Eastern countries, a sort of rice (a grain that makes the chief and most usual food of the natives over almost all those parts) prospers very well upon land so drenched with waters, that seeds-men, to scatter the rice, do rather wade than walk. But this itself (which, for the main, was confirmed to me by eye-witnesses) is less strange, and does less illustriously confirm what I was proposing, than what the inquisitive Jesuit *Martinius* affirms to be the practice of some (as well great as small) countries in *China*, where, in divers places, that are all the year under water, and would by our European husbandmen be thought capable of no other use, than that of ponds or lakes, the Chinenses cast a certain seed so well appropriated to the place, that is to receive it, that though it falls not immediately on the land but on the water, so that one would think they were not about to sow a field, but bait a pond for fishes, yet this seed, being adapted to the soil it meets with at the bottom of the water, does so well prosper and shoot up to the top, that in its proper season the surface of the water looks as fresh and verdant, as a fruitful meadow, and yields as rich a crop. But for fear of digressing, I shall, *Pyrophilus*, proceed to tell you, that perhaps also chemistry, especially in conjunction with hydrostaticks, may prove serviceable to the ingenious husbandman, by assisting him to discover the kinds and degrees of saltnesses, that are in several other bodies that he much deals with. I remember I have met with things surprizing enough, in examining some sorts of earths by distillation, and by several chemical instruments of discovery; but though I have likewise had the curiosity to distil dungs and grain, and fruits, and some other subjects, wherewith the husbandman is conversant, to observe what kinds of saline and other liquors, and in what proportion, and of what strength, they could afford me; yet not having any notes by me of the particular trials, I shall content my self to have given you this hint of a new sort of experiments in husbandry; and shall only add, as to salts, that since the fertilizing power of dungs seems to reside in the salino-sulphureous part of them, (and the like I have by chemical trials found in lime;) a practical insight into the differences and differing operations of salts (about which I elsewhere entertain you) may probably very much assist the husbandman to examine the several dungs, and other composts (the knowledge of which is of great moment in his art) and to multiply, compound, and apply them skilfully.

And as chemistry, that is conversant about fire, so even hydrostaticks and hydraulicks, that teach us to make engines and contrivances for the lifting up, and for the conveying of water, may in divers places be of no small use to the husbandman. For not to mention what is done in some more known parts of the East, of the like nature with what I am going to mention, *Martinius* informs us, that in one province of *China* (whose name I remember not) they are so curious to water their fields of rice, that they have upon the river excellent mills so made, as that great quantities of water are continually raised in buckets, or other convenient vessels, fastened to vast wheels driven by the stream; which watering-mills (to add that notable instance upon the by) are not, as our European mills are wont to be, fixed to one place, but built upon vessels, with which they may remove the mills, how great soever, from place to place, as occasion requires. Nor is this eastern way of raising water by wheels, so as that it may be conveyed by convenient channels to places many foot higher than the river, or other receptacle of the water, that is to be distributed, the only way, whereby the hydraulist and mechanician may assist the husbandman; since he may considerably do it by the art of libellation, or conducting of water upon the ground. For the improvement, that may be made of land by water, in soils fit for that way of culture, may be far more considerable, than is yet wont to be taken notice of; as indeed this husbandry itself is in many countries both elsewhere, and in *England*, as yet unpractised. I have had some lands of my own much bettered by being skilfully overflowed; so that when I observed the difference, the tenant, though shy of acknowledging the utmost advantage, confessed to me, that he thought it yielded him double the former income. And a gentleman of quality of my acquaintance, whose improvements I went lately to view, shewed me a scope of ground, which at his first coming to that wild place, four or five years ago, was boggish, and which yet he had turned into a good dry soil, by only trenching it here and there with shallow trenches of not a foot deep, and overflowing it, by the means of those trenches, and conveniently placed dams, as evenly as he could, five, six, or seven times a year, betwixt the beginning of October, and about the middle of April, with the water of a neighbouring spring, which was no way enriched by land-floods, arising but in a very barren and uncultivated place, far from the

* Lib. IV. cap. 16. as he is published by *Purchas*.

the neighbourhood of grounds capable of enriching it; and yet this spring drained away, if I may so speak, that ancient hydropical distemper of the land, and turned it, as I found by trial, into a good compact soil, on which store of mowers were (when I saw it) employed in making of hay, which this meadow yeilded plentifully enough to be worth twenty times its former value. Nor is this the single considerable instance we have met with, of the improvement that may be made of divers kinds of land, only by skilfully overflowing them with common waters.

BUT, *Pyrophilus*, I may hereafter have so many occasions to mention particulars relating to agriculture, that I should presently dismiss them in this essay, were it not, that I am, by my having named husbandry to you, put in mind to employ it as an instance to confirm this observation, that the more comprehensive a trade is, the more likely it is, that it will be capable of being meliorated by natural philosophy. For such trades, as are of great extent, are obliged to deal with a considerable number of nature's productions, and to make use of divers of her operations; and consequently must comprehend the more particulars, wherein the manufacture or profession may be reformed, and otherwise advantaged by a knowing and dexterous naturalist. Thus the husbandman's corn makes it fit for him to have a competent skill in the whole art of tillage, the keeping of cattle great and small, the ordering of dairies, of wood, of flax and hemp, of hops, of the kitchen-garden, of an orchard, of bees, &c. besides that the particular productions of some of these, as honey, cyder, &c. require some skill, and are capable of much improvement; so that among so great a variety of things, wherewith the husbandman has to deal, it can scarce be otherwise, than that there will be several things, wherein the naturalist's higher and more reaching knowledge and experience will be serviceable to him. And whereas in the preservation both of cattle from diseases, and of the fruits of the earth from putrefaction, lieth one of the most beneficial and difficult parts of the husbandman's skill, he may therein be much assisted by an expert naturalist; who not only, by being able to accelerate putrefaction in divers bodies, may teach the husbandman to furnish himself with great variety of composts and manures, to relieve and enrich his ground with whatever peculiar sort of salt he observes to be deficient; but also may teach him how to preserve many of his seeds, and flowers, and fruits, beyond their wonted duration: as I know some persons, to whom I recommended methods of this kind, that use to preserve quinces, for instance, a great part of the year, by a strong liquor, or pickle, made of nothing bur water, and what (for the most part refuse stuff) may be easily obtained from the quinces themselves. This way presented us fruit at almost the year's end; and a while since I could have shewn you (and, for ought I know, can do so yet) cherries well shaped, and succulent enough, of above a year old, preserved without salt or sugar, by being kept in a spirit of wine fitted for that use, and fully impregnated, before their immersion, with the tincture of the skins of other cherries of the same kind. The vast benefit, that the Hollanders derive from the best way of salting or pickling of herrings, and the advantageous use, that is made by others, of so powdering beef, and ordering other flesh, that it will last good to the *Indies*, and is sometimes brought uncorrupted into these parts again; may persuade us of the benefit, that may accrue to the husbandman, by the discovery of the ways of keeping the productions of the earth from corruption; especially if his skill be extended to weak wines, cyder, perry, and other liquors, which are wont to be made in great quantities, and yet apt to decay at home, and unfit to be transported far abroad. And the use of sugar to strengthen vinous liquors, and make them durable; and, without the help of salt or any sharp thing, to preserve great variety of fruits, and of the juices of herbs, may encourage us to think, that there may be very differing ways (and some of them seemingly opposite) to make many things outlast their natural periods of duration.

BUT my trials and observations (whether about the conserving of fruits, flowers, and flesh, or of other things of this sort) belonging more properly to another discourse (of the preservation of bodies) I shall now mention no more of them, but pass on to tell you, that very much prejudice, which often happens to the poor husbandman (and sometimes even to his utter ruin) by those, either stubborn, or contagious diseases, (such as the rot in sheep, and the glanders in horses,) that make havock of his cattle, may in great measure be prevented by the instructions of a knowing naturalist, especially if he be an expert physician too. For, as many diseases, so many cures are analogous in men and beasts; and the remedies prove frequently more succesful in these than in them, as well for divers other reasons, as because the bodies of many brutes are more able to bear the operation of strong remedies; and yet the unaccustomedness of almost all of them to physick makes them more relievable, than men by any (not improper) remedies. I will not now relate, that I have in some countries found medicines, that have been usefully tried against diseases in men, cried up for their efficacy against their analogous ones in horses; nor with what difference in the dose these may be purged by several of the same catharticks, especially aloes, that are employed for the purgation of human bodies. I shall rather inform you, that as in these salt is, you know, reputed a great resister of corruption, and an enemy to worms, (with a sort of which the livers and neighbouring vessels of sheep have been observed to be infested;) so by the bare use of Spanish salt, which each sheep, being first made to bleed a little under the eye, was made to take down a small handful, two or three times, with some days of interval, without being suffered for some hours to drink any thing after it: by this remedy, I say, given at the time of the year when there is danger, that the sheep will begin to be blotched, many flocks have for divers years been preserved by a rich intelligent gentleman of my acquaintance, that is a great sheep-master, and has thereby (and that also lately) preserved his flocks in a moist country, when most of his neighbours lost theirs. I might here mention to you, *Pyrophilus*, the virtues of crude antimony, to cure the foulness of blood, and even the leprosy in swine; of quick-silver to cure the worms in horses; of *Palmarius*'s famous remedy, which he solemnly affirms to be a constant one against the bitings of a mad dog in cattle, and of a more parable one for men also, whose success I almost admired in a near relation of yours and mine; of the use of the antimonial cup for several sicknesses in horses and sheep, which (if I misremember not) was successfully tried by one, to whom I recommended it; and of another antimonial medicine, which (though much commended to me by a virtuoso, that took it himself) a gentleman of my acquaitance, resident in the country, who prepares it, assures me, that he uses it with strange success to fatten his horses, (made lean by occasion of sickness) with whom yet it works not, either as an emetick, or a purge. And I could here present you divers other receipts much prized for their having (as well as the newly mentioned remedies) frequently been found effectual against the same diseases both in human bodies and in brutes, if I did not think it less proper to make in this place a veterinarian excursion, than to tell you, that, if you have any curiosity for them, you may command them.

I might add, if I had leisure, some reasons, why I despair not, that in time the husbandman may, by the assistance of the naturalist, be able to advance his profession by a therapeutical part, which may extend not only to the animal productions of the ground, and to the vegetable ones; but (in a large acception of the term) to the distempers of the ground itself. For if the causes of the barrenness of soils in general, and of their indisposition to cherish particular plants or animals, were by the philosopher's sagacity discovered, I see not why many of those defects may not be removed by rational applications, and proper ways of cure; as well as we see inconveniences remedied in many other inanimate bodies, without excepting the close and stubborn metalline ones themselves.

And perhaps also, that by a way of management suggested by the knowledge of causes the barrenness of a soil may be cured, or its fertility much promoted by methods, that do nothing near so much require cost as skill. Some ingenious husbandmen have of late proclaimed themselves much satisfied with a way of corrcteing two of the barrenest sorts of land, not by rich manures or other costly cultures, but by skilfully mixing the sand and clay themselves in a due proportion, according to the use the husbandman designs to make of it. And whereas one of the best modern writers of agriculture reports, as he may, for a strange thing, that he had seen seven or eight and thirty ears of barley, that sprung from one grain; I remember, that an ingenious gentleman, to satisfy some curious persons what might be done in that kind, sowed corn upon a piece of land, very near the place of my abode, which prospered so strangely, that one root, that I took particular notice of, though perhaps not the fruitfullest in the field, produced sixty and odd ears of corn; and yet, which was the strangest, this wonderful increase depended upon a philosophical observation; nothing extraordinary having been done, either to the land, or so much as to the seed; as I had opportunity to know, both by the informations of observing men, and by the confession of the gentleman himself, who was pleased to make choice of me to intrust his secret with, that, in case he died before me, the publick might not lose it. Upon which account he also confided to me another specimen of his skill. He once presented your excellent mother a company of several sorts of choice apples, among which there was one sort excellently tasted, but very small; the following year he presented her another basket of the like fruit, but finding no small ones among them, she took occasion to ask him, what was become of the tree, that produced those delicious little apples, that made part of his former present? to which he replied, that he had brought several of its productions among the other fruits, she was looking on; and thereupon shewed her some, that came from the same tree, and appeared by the peculiar relish to be of the same sort, though exceedingly differing in bulk, that neither your mother, nor I, had any suspicion, that the same tree bore them. Upon which occasion he readily gratified my curiosity by acquainting me with his way, which depended almost only upon a physical observation; all that he added being not any rich compost, but some despised leaves of a very cheap and common vegetable. But husbandry is too large a subject for me to prosecute in this place, and therefore I shall here dismiss it.

SECTION III.

THE next thing I shall observe to you, *Pyrophilus*, is, that it is not only to the trades, that minister to the necessities of mankind, but to those also, that serve for man's accommodation or delight, that experimental philosophy may bring improvements; for these arts also do, for the most part, consist in the knowledge and application of some of nature's productions and courses, whose being referred to the accommodation or delight of men, rather than to any other purpose, does produce nothing, that is truly physical in the things so referred, which thereby acquire only such a kind of respect to man, as that which the metaphysicians call an extrinsical denomination; and we see that the same things, without varying their nature, are serviceable to men in very differing capacities: as wine serves one, that is dry, to quench his thirst, serves a faitning person to revive his spirits, and the drunkard to inebriate him; the same spirit of wine, that serves the physician to make tinctures and extracts for the recovery of health, may serve the ladies to dissolve benjamin into a tincted liquor, that diluted with fair water may be used as a cosmetick, which I have received many thanks for; and the same spirit skilfully employed upon ingredients, to be named to you ere long, is of excellent use for making divers fine varnishes made with rectified spirit of wine; nay, the newly mentioned solution of benjamin may it self be applied to all those differing uses; for of it self it is a pretty and odoriferous varnish, and I have used it (though not often, for want of opportunity) with very good success against a sort of tetters, which I caused frequently to be bathed with it. What happy applications knowledge and skill may make even of unpromising things, to the furnishing men with delights, is methinks very evident in musical instruments, as lutes, viols, &c. For who would think (if experience did not assure us of it) that with a few pieces of wood joined together, and the guts of cats or lambs wreathed or twisted into strings, the skilful musician, by the help of mathematicks and exercise, should be able to charm the ear with the greatest, as well as most innocent delights, the sense belonging to the organ is capable of, and which sometimes does not only please, but ravish the transported hearers. But though, *Pyrophilus*, as I was lately saying, physicks may not only be very improving to those arts and professions, that serve to provide man with the necessaries or accommodations of life, but also to those, that serve chiefly to furnish him with pleasures and delights; as might be instanced in experiments of colouring, perfuming, making sweet-meats of all sorts, embellishing the face with cosmeticks, and divers others of the like voluptuous nature: and though I may elsewhere have occasion, when I come to treat of colours, odors, tastes, and other qualities, to acquaint you with some receipts and experiments of this kind; yet now I do not only want leisure to mention them, but am desirous, that natural philosophy should engage you to court her, rather by her gratifying and enamouring your reason, than by her bribing and inveigling your senses.

SECTION IV.

THOUGH what has been represented about the usefulness of experimental philosophy to trades does chiefly belong to those, wherein nature's productions are employed to human uses, by those operations, wherein nature her self, rather than the artificer, seems to have the chief hand, as the trades of brewing, baking, gardening, tanning, &c. yet I would not exclude those very trades, wherein the artificer seems to be the main agent, and in whose ultimate productions the chief thing, that is wont to be considered, is the adventitious shape or form, which the artificer, as an intelligent and voluntary agent, does, by the help of his tools, give the matter he works on, as in the trades of the smith, the mason, the cutler (when distinct from that of the sword-maker,) the watch-maker, and other handicrafts. For though these consist rather in the manual dexterity of men, than the skilful ordering of the productions of nature, by their material operations upon one another; yet to many, if not all, even of these, the naturalist may some way or other be a benefactor.

For there are divers of these manual trades, that, especially as they are exercised in cities and greater towns, consist of several parts, and have need of several other trades to prepare materials for them, and dispose them to receive the last form, which the artificer is to give them, to fit them for sale. And we may, in many cases, observe, that though this artificer, that gives the matter this last form, does it chiefly with his hands and his tools; yet those other tradesmen, to whom he is beholden for his materials, do some or other of them, to prepare and qualify them for his use, need some observations of the conditions of the body they deal with, or must imploy some physical operations, wherein they may be much assisted by the knowing naturalist, who may also teach the manual operator himself, how to make choice of his materials, and examine the goodness of those, that subordinate workmen shall bring him. Thus, though stone-cutting be a trade, that seems to consist almost wholly in giving, with proper tools, to marble, free-stone, and other materials, the shape, which the artificer designs; yet, if I had leisure, I could easily shew you, that even in this trade, there are many particulars, wherein experimental philosophy might be helpful to the artificer. For ways, hitherto unused, may be found out (as I have partly tried) to examine the nature and goodness of the marble, alabaster, and other stones, which the mechanicks deal with. A competent knowledge of the sap, that is to be found in stones imployed for building, is of so much importance, that the experienced master workmen have confessed to me, that the same sort of stone, and taken out of the same quarry, if digged at one season, will moulder away in a very few winters; whereas digged at another season, it will brave the weather for very many years, not to say, ages: (but of my observations of this kind, more elsewhere.) The cements also, and stoppings (as they call them) which are of good use in this trade, may be easily bettered by the naturalist, that is versed in such mixtures. And I remember, I had occasion to teach a fine cement for the rejoining of the broken limbs of statues to their bodies, to an inquisitive artificer, who, by such like helps, did in other cases, so well counterfeit marble with a cement, that even where there was occasion to fill up great cavities with it, the work would pass for entire; the additaments being not distinguished from the natural marble. Want of curiosity also keeps our stone-cutters here in *England* unacquainted with the ways of working upon porphyry, which they will not undertake either to polish or to cut. Nor is *England* the only country, where the art of working upon porphyry (which appears to have been in great use amongst the *Romans*) is unknown, though at *Rome* there are some few, that do with great gain exercise it. And though I know not precisely, what it is they employ, imploy, yet I presume, it may be powder of emery: for with that, and water, and steel-saws, I have here in *England* caused a porphyre-stone to be cut. And the mention of porphyre puts me in mind of telling you, that by an art I have, white marble may be so stained, and that durably, with spots great or small, and red or brown, as it pleaseth the artificer, as I may hereafter have occasion more fully to relate. It would be too long to discourse to you here of artificial marble, and divers other things, that stone-cutters affirm to belong to their trade, wherein you will scarce doubt, but that it may be capable of improvement. Wherefore I shall only add, that whereas this profession does much require very good steel-tools, and they must have these from smiths, and others that deal in iron, if these men's trade were bettered by the naturalist, they might be able to afford the stone-cutter the better tempered tools: and that even the smith's craft, though it seem to be merely a manual art, is yet capable of much melioration by the knowledge of nature, were not difficult to manifest, if it were proper here to insist on the proofs of it; yet thus much I shall here take notice of, to confirm this fourth observation, that not only the philosopher may, as a mineralist, and a mechanician, improve the ways of making iron and steel, before they come to the smith's hand, but likewise may devise better expedients, than are among us in use, for the ordering of iron and steel, when it comes to be formed into weapons and tools. The sword-blades, and other arms, that are made at *Damasco*, are very famous every where, and (as far as some trials have informed us) justly for their excellency in cutting even iron. And yet it seems to be only the skill of the artificers in ordering it, that gives the swords, and other instruments made at *Damasco*, so great a preheminence above others. For though the goodness of them have been presumed to proceed from that of the iron-mines, and steel, peculiar to the region of that city; yet the judicious *Bellonius* *, having made particular enquiry at his being there, informs us otherwise, and tells us, that iron and steel, being brought thither from other parts, (the country having no mines of it) receives there from the skill of the workmen its temper and perfection. And I see not, why I may not reasonably suppose, that in the tempering of steel, it is not only the goodness of the metal, and the determinate degree of heat, though these be the only things artificers are wont to look after, that give the best temper; but that much may depend upon the nature of the liquors, or other bodies, wherein the hot steal is plunged, and upon other ways of ordering it, if those be skilfully chosen and imployed. I have had a graver so well tempered, (but by whom I know not) that all the known ways used by me and others, (who wondered, as well as I, at the unsuccessfulness of our endeavours,) could not deprive it of its temper, as they would have done any gravers, that we make here; and it was afterwards affirmed to me, that it was made of steel tempered at *Damasco*.

I may elsewhere tell you, *Pyrophilus*, both of a way I have tried, of hardening gravers, without quenching them in any liquor or tallow, or any other unctuous body; and that having perswaded an ingenious artificer to try an unpractised way of tempering gravers, he soon after brought me one to see the goodness of it, which, by being plunged in a certain cheap mixture, (wherewith I may hereafter acquaint you) had been hardened and tempered at once: which though most artificers would think scarce possible, yet upon the authority of trial I shall venture to deliver, what some may think as strange, namely, that though ignition and extinction in cold water, be the common and known way to harden steel gravers, yet by that way, only observing precisely a nick of time, steel may be made strangely soft. But of this more elsewhere. I shall now add, that having enquired of one of the curiousest, and most observing makers of steel-tools, whether he did not find a difference in the employing of pump-water, or river-water, in giving them their temper, he satisfied me, that he did so; and observed the former to be fitter for some sorts of tools, and the latter for others. There may be divers other particulars, wherein iron and steel may be improved by the naturalist. The first may be this; that the metal be rendered so soft, as to be, by the help of strong moulds, put into shapes. This an eminent and credible artificer assured me, he had often seen his master do to iron, with considerable profit. Or else it may be made fusible like an other metal, as I remember I have (sometimes, with a certain flux-powder, which I composed, if I much forget not, of tartar, sulphur, and arsenick) made it run, even with a charcoal fire, into a mass exceeding hard, and very polishable. A third way may be this; that it be so ordered, as to be preserved very long from rust, which an ancient virtuoso, who had purchased the secret of a rare artist for a great prince, and used to shew his friend's steel so prepared, assured me, was done chiefly by tempering it in water well impregnated with the bark of a certain tree. In a word, there may be divers other ways, whereby iron or steel themselves, or their trades, that imploy them, may be meliorated; and to add, that on this occasion, there are many and very differing accounts, upon which a trade or profession may be benefited by the Experimental Philosopher: for he may either find out variety of materials, wherewith to perform the things desired by the tradesman, or he may render those materials, that are already in use, better conditioned; or he may discover and reform the unheeded errors and mistakes to be met with in the trade; or he may devise more easy and compendious ways of producing the effect that is required; or he may improve some of the auxiliary trades, of which the trade spoken of has need or use; or may instruct the artificer to choose, and examine, and preserve his materials and tools, better than is usual, or can make

* P. Bellonius observat. Lib. II. cap. 93.

make the ultimate productions of his trade sooner, or cheaper, or easier, or better conditioned, or applicable to more uses, or more durable, than they are commonly made. Nor are these all the particulars, that might here be enumerated to the same purpose, if this fourth consideration had not detained us too long already.

SECTION. V.

THE naturalist may increase the power and goods of mankind upon the account of trades, not only by meliorating those, that are already found out, but by introducing new ones, partly such as are in an absolute sense newly invented, and partly such as are unknown in those places, into which he brings them into request. For it were injurious both to nature and to man, to imagine, that the riches of the one, and the industry of the other, are so exhausted, but, that they be brought to afford new kinds of employments to the hands of tradesmen, if philosophical heads were studiously employed to make discoveries of them. And here I consider, that in many cases, a trade differs from an experiment, not so much in the nature of the thing, as in its having had the luck to be applied to human uses, or by a company of artificers made their business, in order to their profit; which are things extrinsical, and accidental to the experiment itself. To illustrate this by an example, the flashing explosion made by a mixture of nitre, brimstone, and charcoal, whilst it past not farther than the laboratory of the monk, to whom the invention is imputed, was but an experiment: but when once the great (though unhappy) use, that might be made of it, was taken notice of, and mechanical people resolved to make it their profession and business, to make improvements and applications of it; this single experiment gave birth to more than one trade; as namely, those of powder-makers, founders of ordnance, gunners (both for artillery and mortar-pieces,) gunsmiths; under which name are comprized several sorts of artificers, as the makers of muskets, small pistols, common barrels, screwed barrels, and other varieties not here to be insisted on.

THE discovery of the magnetical needles property to respect the poles, has given occasion to the art of making sea-compasses, as they call them, which in *London* is grown to be a particular and distinct trade. And divers other examples may be given to the same purpose; especially where mechanical tools and contrivances co-operate, with the discovery of nature's production. So that oftentimes a very few mathematical speculations, or as few physical observations, being promoted by the contrivance of instruments, and the practice of handicrafts men, are turned into trades; as we see, that a few dioptrical theories lighting into mechanical hands, have introduced into the world the manufactures of spectacle-makers, and of the makers of those excellent engines, telescopes and microscopes.

THE observing, that though quick-silver will amalgame with gold (and thereby seem to be destroyed,) which made *Pliny* think it an enemy to metals,) yet it may be separated from the gold again, without diminution of that noble metal) has brought forth the trade of guilders, whose art consists chiefly in mixing, by the help of a competent heat, good gold with five, six, or seven times its weight of quick-silver, until the mixture come of such a consistence, that they may spread it as they please upon the silver or copper to be gilt. For having by this means overlaid it evenly with gold, they can easily with fire force away the mercury; and, with a liquor impregnated with nitre, verdigrease, sal armoniack, and other saline bodies, which they call a colourish, restore its lustre to the remaining gold, which they after make bright by polishing.

THE almost obvious and trivial observation made by some sagacious person, (whoever it was) that a spring was a physical, continual, and durable power or force, and the corollary he thence deduced, "that this force, skilfully "applied, might be equivalent to the weights, "that were thought necessary to move the "wheels of clocks:" these reflections, I say, joined with a mechanical contrivance, produced those useful little engines, watches, that now afford a plentiful livelihood to so many dexterous artificers; which though custom has made familiar to us, yet were unknown to the ancients, and highly prized and admired in *China* itself, when first (in the last century) brought thither. The discovery of the virtue of aqua fortis, to dissolve silver and copper without working upon gold, added to the observation, that lead melted with either of the two noble metals, and then forced from them by fire, will carry away with it any of the baser metals, that may have been mixed with them; these two particulars, I say, have begot in latter ages the art of the refiners we now have.

MEN's having observed the operations of some lixiviums, clays, and a few other familiar things upon the juice of the sugar-cane, has not only occasioned the adding of the culture of those reeds to the other parts of husbandry, left us by the antients; but has produced the several trades of sugar-boilers, or makers of sugar, refiners of sugar, and confectioners: not to mention the great addition the concreted juice of the sugar-cane brings to the apothecaries profession, upon the score of syrups, conserves, electuaries, and other saccharine medicines. Nay, a very slight manual contrivance, or operation, if it light fortunately, may supply men with a trade, as in the art of printing. To which I shall only add, that in *China*, and some other Eastern parts, the lucky trial, that some made to bore very small holes through *Porcellane* or *China* cups, and employ very slender wire instead of thread or silk, has given being to the vulgar trade of those people, that go up and down in those countries, as tinkers do with us, getting their livelihood by sewing together the pieces of cracked or broken *Porcel-*

lane vessels; as I have been informed by more than one credible person, that lived in the East, and had experience of the use of cups so mended, though filled with liquors as hot, as they are wont in the East to drink their coffee and tea.

The mention freshly made of *China* brings into my mind, that whereas the knowledge of some gums and liquors in that country afforded those useful, as well as most beautiful, varnishes, which we call by the name of the kingdom, that supplies us with them, and which do both there, and in *Japan*, employ multitudes of tradesmen; I am credibly informed, that the art of making the like varnished wares is now begun to be a trade at *Paris*, and I doubt not but it will before long be so in *London* too. For though some accounts, that were given me by virtuosi of that varnish, were such, that the trials of them did very ill answer expectation; yet having read in *Linschoten*'s voyages, that in *China* and *Japan* they make this excellent varnish of gum lacca, I found by some trials, that I was able to imitate one of the best sorts of it, by dissolving the gum in high rectified spirit of wine,* and then giving it a colour, and laying it on in such a manner, as I may have before long a fitter occasion to inform you.

And without much impropriety, I might alledge the art of cultivating and gathering sugar-canes, and of ordering their juice, as a recent instance of the transplanting of arts and manufactures. For, as I am informed by very credible relations, there are not very many years effluxed, since, in our memory, a foreigner accidentally bringing some sugar-canes, as rarities, from *Brasil* into *Europe*, and happening to touch at the *Barbadoes*, an English planter, that was curious, obtained from him a few of them, together with some hints of the way of cultivating and and using them. Which, by the curiosity and industry of the English colony there, were in a short time so well improved, that, that small island became, and is still, the cheif store-house, that furnishes, not only *England*, but *Europe*, with sugars. And this instance I the rather mention, because it is also a very notable one, to shew, how many hands the introduction of one physico-mechanical art may set on work; since I have had particular opportunity to learn by enquiry, that the negroes, or, as they call them, blacks, living as slaves upon that spot of ground, and employed almost totally about the planting of sugar-canes, and making of sugar, amount at least to between five and twenty and thirty thousand persons. And, that you may see how lucriferous in that place this recent art of making sugar is, not only to private men, but to the publick; I shall add, that by divers intelligent and sober persons interested in the *Barbadoes*, (and partly by other ways) I have been informed, that there is, one year with another, from that little island, which is reckoned to be short of thirty miles in length, (and so I found it, by measuring it on one of the fairest and recentest maps,) shipped off for *England*, especially, ten thousand ton of sugar, each ton being estimated at two thousand pound weight, which amounts to twenty millions of pounds of that commodity; which though it may seem scarce credible, yet one of the antient magistrates of that island, lately assured me, that some years it affords a much greater quantity.

I shall not fortify what I have hitherto discoursed with particulars, that will elsewhere more properly fall in; it being sufficient for my present purpose, that the instances already mentioned may render it probable, that the experimental philosopher may not only improve trades, but multiply them, till I have occasion in the last essay of this book, to make it out more fully. Nor do I despair, that among other ways, whereby trades will be increased, one may be the retrieving some of those, that were anciently practised, and since lost; of which we have a catalogue in the learned *Pancirollus*. For as it is the skilful diver's work, not only to gather pearls and coral, that grew at the bottom of the sea, and still lay concealed there; but also to recover shipwrecked goods, that lay buried in the seas, that swallowed them up: so it is the work of the experimental philosopher, not only to dive into the deep recesses of nature, and thence fetch up her hidden riches; but to recover to the use of man those lost inventions, that have been swallowed up by the injuries of time, and lain buried in oblivion. This I do not say, altogether groundlesly, though for some reasons I here decline mentioning the things, that induced me to say.

SECTION VI.

To what has been hitherto said I shall venture to add, not only, that the sagacious philosopher may better most of the trades, that are already in use, and add to the number of mechanical employments; but that I am apt to think it might, without much hyperbole, be affirmed, that there is not any one profession or condition of men (perhaps scarce any single person of mankind) that may not be some way or other advantaged or accommodated, if all the truths discoverable by natural philosophy, and the applications, that might be made of them, were known to the persons concerned in them. So that besides those discoveries, that are compiled or formed into trades, there are, and may be, found a multitude of loose particulars, whereby the naturalist may much gratify and assist men, according to the exigency of particular occasions. The nature of the thing will scarce permit me to illustrate so unlikely an assertion, without employing instances in themselves trifling, if not despicable; of which I will therefore give you but a few, because, if they were not pertinent to my present purpose, they would be fitter to divert, than inform you.

I had, not long since, the honour to be known to a very great court-lady, who was much troubled, that having frequent occasion to write letters, she could scarce handle a pen, without

* See the Appendix to Essay V.

without blacking her fingers with ink. I ſmilingly undertook to make her write without ink, which I my ſelf was formerly wont to do, by firſt preparing my paper with a powder made of copperas, ſlightly calcined upon a fire-ſhovel, till it grow friable, and galls, and gum-arabick finely pulverized, and exquiſitely incorporated with the vitriol in a certain proportion; which though a few trials will better teach than rules, (becauſe, according to the goodneſs and calcination of the vitriol, the proportion of the other ingredients muſt ſometimes be varied,) yet to aſſiſt you in your firſt gueſſes, I ſhall tell you, that, for the moſt part, I uſed my ſelf three parts of calcined vitriol, two parts of galls, and one part of gum-arabick, and mixed them not before I was ready to employ them; for this powder being with a hare's foot, or any other convenient thing, carefully rubbed into the paper, and the looſer duſt ſtruck off, doth, without diſcolouring it, ſo fill its pores with an inky mixture, that as ſoon as it is written upon with a clean pen, dipped in water, beer, or ſuch other liquors, the aqueous part of the liquor diſſolving the vitriolate ſalt, and the adhering particles of the galls, makes a legible blackneſs immediately diſcover itſelf on the paper. This mention of writing brings into my mind, that ſeveral times having had occaſion to make a word or two, that was but lately written, look as if it had been written long before, I performed it, by lightly moiſtening the words I would have to look old, with oil of tartar per deliquium, allayed with more or leſs fair water, according as I deſired the ink ſhould appear leſs or more decayed: which experiments may be often uſeful in manuſcripts, to keep the recent interlineations, or other additions, from betraying themſelves by their freſhneſs not to have been written at the ſame time with the reſt of the manuſcript.

And the deſign I had in making uſe of the lately mentioned powder of galls and copperas puts me in mind of another way of writing without ink, (and too without danger of blacking one's fingers or linnen,) which I remember I have practiſed ſometimes with one powder, and ſometimes with another. For conſidering, that common ſilver being rubbed upon bodies, whoſe ſurfaces are a little rough, and even upon coloured cloth, the metal would leave a blackneſs on it, it was eaſy to conclude, that if the ſurface of the white paper were aſperated by a multitude of irregular grains of a powder as white as it, would retain a blackneſs, wherever a blunt ſilver bodkin ſhould be drawn over the grating particles: and accordingly I found, that either exquiſitely calcined hartshorn, or clean tobacco-pipes, or (which is better than that) mutton-bones (taken between the knuckles, and) burnt to a perfect whiteneſs, being finely powdered and ſearſed, and well rubbed upon paper, would make it fit to be written upon with the point of a ſilver table-book pin, or bodkin of ſilver (which metal is not abſolutely neceſſary in this caſe,) as well as that, which is called mathematical paper, (if the being prepared with one, or other of theſe powders, do not make it the ſame.)

And now I am upon the mention of ſuch preparations of paper, I remember, that I was once in a place, where I could get no white leaves, to ſupply a fine table-book, that I had much uſe for; nor could I hear of any tradeſman in the whole country, that knew the way of making ſo much as ordinary table-books: wherefore I bethought my ſelf of trying to make ſomething by way of ſuccedaneum, which ſucceeded at the firſt attempt. And though there may be better ways to make white table-books, yet perhaps you will find none more ſimple and eaſy; the two only ingredients we had in it, being to be had at every apothecaries ſhop. I only take ceruſs, rubbed to very fine powder, (which is done in a trice) and temper it up with fair water glutted with clear gum-arabick. With this mixture (being brought to the conſiſtence of a ſomewhat thick ſalve) I rub over the paper I prepare, putting on more or leſs, according as I would have it laſt; and having ſuffered it to dry (which it will quickly do) it may, if there be occaſion, be preſently uſed with the point of a ſilver-pin, which will make the letters appear very conſpicuous upon a mixture, that does not at all impair the whiteneſs of the paper; and what was thus written I could, with ſpittle or water, blot out three or four times ſucceſſively without ſpoiling the paper. Which queſtionleſs had been much better prepared, if divers couches of the mixture had been laid on, and ſuffered each to dry, and if afterwards the paper had been ſmoothed by being ſcraped with a knife, and poliſhed.

A very ingenious artificer, who had contrived an inſtrument uſeful to others, and profitable to himſelf, whereof an abſolute neceſſary part was a glaſs filled with fair water, and exactly ſtopped, complained to me, that though his inſtrument did exceeding well in all but froſty weather, yet then it was apt to be ſpoiled by the freezing of the included liquor, which too often broke the glaſs. Whereupon I taught him to remedy it, by ſubſtituting, inſtead of water, good ſpirit of wine, which has not in our climate been obſerved to freeze; or rather, (becauſe in his bigger glaſſes, that liquor would be chargeable) either ſea-water ſtrengthened with a little ſalt, or elſe common ſpring-water with a twentieth, or at moſt a tenth part of ſalt diſſolved in it. For though this brine look, if well made, as clear as common water, yet I have not obſerved, that the ſharpeſt of our Engliſh winters would make it freeze.

To a perſon of quality, that was very curious of the way of writing ſecretly, I undertook to teach an eaſy way (which after I knew it, I found alſo in an old printed book) of ſending a written meſſage, without putting it into the power of the bearer to betray it; which I could eaſily have performed my ſelf, if the meſſage were to be delivered in a ſhort time, and not too far off, by writing on his back, or other convenient part of his body, with a clean pen dipped in my own urine, (there being ſome urines, with which I have found, to my wonder, that the experiment would not ſucceed.) For if he, that receives the meſſage, rubs but a

little

little of the black substance remaining of paper after it is burnt, those sable parts adhering to those other of the liquor, that lurk yet in the pores of the skin (whence, if the messenger went fast, and very fast, the sweat would probably dislodge them) do denigrate all that was written, and make it legible enough, sometimes, as I have tried, after many hours.

I remember too, that intending one summer to make some abode at a house I had in the country, I sent for from *London*, among other things, a quantity of damask table-linnen, with which he that sent it me, inconsiderately packed up a great pot of a certain confection, which, for some purposes, I had caused to be made of the pulp of sloes, which, by agitation of the horse it was carried on, being brought to ferment, and run out of the broken pot, stained all the new damask from the top to the bottom. At which an old domestick of mine (whom you remember very well) seeming much troubled, because he had sent for it, to convince him, that experimental philosophy was not altogether useless, I steeped the stained linnen, for some convenient hours, in new milk; and afterwards causing it to be throughly and diligently washed in the like liquor, the damask came forth unstained, and almost as white as it. What urine, if duly and long enough employed, may do to take stains, even of ink, out of linnen, is but to be hinted in this place; where I might add, that with strong spirit of salt, wherewith I moistened, as often as was needful, the spotted places (first wetted with fair water) I have out of new linnen taken spots of ink (especially fresh ones) of very differing sizes, without leaving (after the linnen was well washed out in fair water) any of those yellow stains which many call iron-moles.

Some ingenious persons, that deal much in lixiviums and brines, complaining the other day, that besides that they could not sometimes easily come at an egg, to try, by its sinking or floating, the strength of the saline liquors they would examine, there needed a good quantity of liquor to make such a trial in, I allowed their complaint to be just, and the rather, because I observe, for nicer estimates of the strength of liquors, the trial by eggs is uncertain enough, in regard, that even the same egg will, as I have found, by being kept, grow lighter, whence stale eggs have usually a great cavity (that seems filled only with air) at the bigger end: and I told them, to omit the more artificial, but more difficult, ways of examining such liquors, I sometimes used a way, whereby I could try the strength of the lixiviums made with chemical salts, though I had not above a thimbleful of the liquor, and this with a body, that will not easily waste like an egg, and therefore may be kept. For I substituted, instead of the egg, a small piece of amber, about the bigness of a pea, which in a very strong solution of lixiviate salt, will, as I let them see, swim on the top, but sink in a weak one. And as you may take a piece of amber, less or bigger than a pea, as best fits your occasions, and need not be at all scrupulous about the figure, (provided the amber be once well ducked in the liquor;) so it is some convenience, that two pieces of amber, whereof the one is far more reddish, and the other paler, will be, as far as I have tried, of somewhat differing specifick gravities, so that the one will float in some liquors, wherein the other will sink.

I remember I was once in a country, where I had a great mind to try some things with Dantzick vitriol, or some other blue copperas, but, by reason of the wars, could not possibly procure any, though there were in that country a place, where green vitriol was made by the help of iron: wherefore getting some of that liquor, which the rain had washed from the copperas stones, I did, by putting into it a convenient quantity of copper, reduced into small parts, make the newly mentioned liquor, serve for a menstruum to work upon the metal, and by exhaling the solution to a due consistence, I obtained the blue venereal vitriol I desired. And the like, I doubt not, may be done with such of those common green vitriols made of iron, wherein the saline part is not too much satiated with the martial.

An ingenious and well known person, that is a great dealer in cyder, coming to visit me, and expressing a great desire to be able to make some, that would be stronger, and thereby likelier to keep longer than the ordinary way, I extempore directed him to an unusual course, for which he afterwards came to give me solemn thanks. The way was to take the strained juice of apples, and in ten or twelve gallons thereof to steep for 24 hours (more or less) about two bushels of the same kind of apples grosly bruised: the apples being lightly expressed, the infusion was (with fresh) repeated once more, (care being to be taken, that the infusion be not made too strong and thick, which may hinder the seasonable clarification of the liquor.)

It was not perhaps difficult to mend this prescription; but I give you the account of it, as I received it from him, because he assured me, that none of his many trials had furnished him with cyder so well bodied, and so much applauded. The cautions, that belong to this practice, and the various applications, that may be made of this way of making vinous liquors of fruits, without additions (so much as of water,) by infusion, and the varyings of the experiment according to particular cases, I must not here stay to mention.

It was not long since, that accidentally rummaging in a dark place, where I had not of a long time been, and where unknown to me some chemical glasses, negligently stopped, and not written on, had been put; one of them falling down made two or three great stains in the conspicuousest part of a new suit I had then on; and would have obliged me to leave it off, but that judging by the nature of the stain, that it was made with some acid spirit, I tried, by smelling to them, whether among the other bottles, one or other had not some urinous, or otherlike spirit; and lighting on a liquor, which, though I know not what it was, I guessed by the stink, to abound

abound with volatile salt, I bathed the stained parts well with it, and in a trice, restored them to their former colour. And, by a like way, I have presently remedied the discolorations made by some sharper and fretting liquors, of died garments of other sorts and materials, which those blemishes would else have rendered altogether unfit for wearing.

ANOTHER time, discoursing with a statesman of the ways whereby well-meaning persons may be injured and defamed, I undertook, that out of a parchment-writing, with his hand annexed, I would take out all, that was written above his name, without spoiling or disfiguring the parchment, on which I would afterward write what I pleased, and whereby I might make people believe, that he had acknowledged under his hand such things, as never came into his thoughts. And to satisfy him of the possibility of this, I did in a few minutes take off from the parchment all, that was written on it, without defacing the parchment. Some attempt to free paper from what is written upon it, with aqua fortis, but, that by discolouring the paper, makes men apt to suspect some intended deceit. And for the true way of performing such an effect, and divers others of the like nature, which I have sometimes for curiosity prosperously experimented, I think it much fitter to be concealed than communicated; because if such secrets should fall into the hands of persons inclined to mis-apply them, they might very much disturb human society. And therefore it is better men should want the light afforded them by such experiments, than be brought into the danger of such mischiefs, as they may be made to suffer by the mis-imployment of such discoveries.

I remember, that not long since, a virtuoso happening to have made a solution of gold, wherewith he thought to make aurum fulminans, thought he had cause to suspect, that it had been enbased with copper, and therefore would not be so fit for his work; whereupon I considered with my self, that a good urinous spirit being imployed instead of the usual menstruum (oil of tartar,) as it would precipitate gold out of aqua regis, so it would readily dissolve copper, I conjectured, that by the affusion of such a liquor I might both discover, whether the solution (whose colour did not at all accuse it) contained any copper, and if it did free the gold in great part from the baser metal: and indeed I found, that after the urinous spirit had precipitated the gold into a fine calx, the supernatant liquor was highly tinged with blue, that betrayed the alloy of copper, that did not before appear.

I hope you think, *Pyrophilus*, that it is because these instances are more pertinent to my design, than many others (that might have been substituted) in themselves more valuable, that I have mentioned such inconsiderable ones; and I shall not repent the naming of such instances, if they have let you see, that even mean experiments are not to be despised, but, that the meanest may be sometimes, not only useful, but more proper to convince strangers to natural philosophy of the manifold uses of it, than experiments of a higher and abstruser nature. For as in a shipwrack, it may more advantange the distressed pilot to know the supporting nature of a bladder filled with wind, though otherwise but a despicable and airy thing, than to know the abstrusest properties of the magnetick needle; so, in some cases, the more obvious and slight experiments may be much more welcome and serviceable to us, than others at other times much more considerable. So true is that of the wise man, That every thing is beautiful in its season.

FOR my part, I am very apt to hope, that natural philosophy will prove more and more serviceable, both to single persons in their particular occasions, and to trades themselves in general; as by other ways, so especially by making a further search into, and thereby detecting new qualities, or discovering unheeded uses, of the productions of nature, and of art, that are already known.

I will not here take notice of what may be further hoped for in the detection of medical virtues of things, because I treat of that subject in a more proper place: and as for the mechanical uses (if I may so call them) and applications of the works and laws of nature, though he, that gazes upon the seemingly great variety of productions to be met with among tradesmen, and in the shops of artificers, may be tempted to think, that art has curiously pryed into, and imployed, almost all the materials, that nature could afford it; yet he, that shall more narrowly and severely consider them, may easily discern, that tradesmen have really dealt with but very few of nature's productions, in comparison of those they have left unimployed; and, that for the most part, they have, in the things they daily converse with, scarce made use of any other, than the more obvious qualities of them; besides some few more lurking properties, which either chance, or a lucky sagacity, rather than inquisitiveness or skill, discovered to them. And indeed this great variety of productions we have mentioned, proceeds more from a manual dexterity of diversifying a small number of known things into differing shapes, than either from the plenty of natural or artificial productions they work upon, or any diligent or accurate search made into the qualities of those productions. But because, to a considering man, it cannot but be obvious enough, that the uses of the things they deal in, and much more those of other concretes, which they are not engaged to observe, have not been hitherto sufficiently inquired into; I shall content my self to add, that if men were but sensible enough of their own interest, and in order thereunto would keep their eyes heedfully open, partly upon the properties of things, and partly upon the applications, that may be made of those properties, to this, or that use in human life, they might not only discover new qualities in things, (some of which might occasion new trades,) but make such uses of them, as the discoveries themselves would never before-hand have suspected or imagined: whereof I may, God permitting, give you elsewhere divers instances.

SECTION VII.

After the foregoing general conſiderations (about the uſefulneſs of natural philoſophy to the empire of man over things corporeal,) which I thought fit to take notice of in this firſt eſſay, it remains, *Pyrophilus*, that I alſo add a word or two about thoſe, that are to follow.

And firſt you muſt not expect, that I ſhould methodically enumerate, and particularly diſcourſe to you of all the grounds and motives I may have of looking for great advantages to accrue to mankind, by men's future progreſſes in the diſcovery of nature. To entertain you with conſiderations, which perchance you would judge but ſpeculative and remote conceipts, would exceed my leiſure, and perhaps be unwelcome to you; and therefore I chooſe to confine myſelf to the inſiſting on thoſe grounds of expectation, which I can render probable by examples and inſtances of what is already actually attained to, or at leaſt very likely (in no long time) to be ſo. And this advertiſement I thought neceſſary to premiſe, partly indeed, that you may not think, that I have overlooked all the particulars pertinent to my ſubject, that I ſhall leave unmentioned, but much more, that you might not ſuſpect, that there are no other inducements to hope much from experimental philoſophy, than thoſe you will find treated of in the following eſſays. And this one thing in particular I dare not forbear to give you notice of, that for the freſhly intimated reaſon, you will there find omitted one of the principal grounds of hoping great matters from improved phyſiology; namely, that by the ſagacity and freedom of the lord *Verulam*, and other lights of this age, conſidering men are pretty well enabled both to make diſcoveries, and diſcern a poſſibility of removing all the impediments, and other cauſes of barreneſs, that have hitherto kept phyſicks from being conſiderably uſeful to mankind; ſuch as many falſe and fruitleſs doctrines of the ſchools; the prejudices, by which men have been hitherto impoſed on about ſubſtantial forms, the unpaſſable bounds of nature, the eſſential difference betwixt natural and artificial things, &c. a too plauſible deſpondency; a want of belief, that phyſicks much concerned their intereſts; want of encouragement; want of natural hiſtory; want of curioſity; want of a method of enquiring; want of a method of experimenting; want of phyſical logick; want of mathematicks and mechanicks; want of aſſociated endeavours; to all which but too many other particulars might be added.

2. You will not think it ſtrange, that in the following tracts much of the uſefulneſs, for which I would recommend phyſicks, ſuppoſes future proficiency in them, if you conſider the nature of my deſign; which is not to make an elogium of natural philoſophy, imperfect as it yet is, but to ſhew, that as it may be, and probably will be, improved, it may afford conſiderable advantages to mankind. And ſince, as I long ago intimated to you, my purpoſe in this book is to invite you, and aſſiſt you to invite other ingenious men, to a farther ſtudy of nature, it is very agreeable to my deſign, to repreſent the greateſt benefits I make it promiſe you, as effects and recompences of your future attainments: and I ſhould allowably enough diſcharge my part in this treatiſe, if I ſhould not do any more (which yet I hope I ſhall do) than give you reaſonable inducements to entertain high expectations of the fruits, that may be gathered from natural philoſophy, if it be induſtriouſly and ſkilfully cultivated: and the very rendring ſuch an expectation probable, I take to be a good ſtep towards the attainment of the things expected; many of which would queſtionleſs be obtained, if men were thoroughly perſwaded, that they are moſt worthy to be endeavoured, and very poſſible to be compaſſed. And therefore I wonder not, that ſo judicious a friend to philoſophy and mankind, as Sir *Francis Bacon*, ſhould in ſeveral places repreſent men's opinions of the impoſſibility of doing great matters of the nature of thoſe things we are ſpeaking of, as one of the chief obſtacles to the advancement of real and uſeful learning: and I the rather inſiſt on the things, that may heighten your expectations, not only becauſe many prudent and learned men, who have been bred in the philoſophy of the ſchools, are apt to judge of all philoſophy by that, which for ſo many ages has been barren, as to uſeful productions, (though fruitful enough in controverſies,) but becauſe I have met with ſome moroſe authors, and others as deſpondent perſons, who, becauſe they have unſucceſsfully attempted to perform things according to the preſcriptions of ſome unfaithful writers of natural philoſophy, fall preſently to believe themſelves, and to perſwade others, that nothing conſiderable is now (at leaſt without almoſt inſuperable difficulties) to be perfomed by natural philoſophy itſelf, eſpecially, whilſt men amuſe themſelves about ſpeculations and trials, that ſeem not to tend directly to practice; our anceſtors having had the luck to light upon all the profitable inventions, which ſkill in phyſiology is able to ſupply mankind with. But (to take notice firſt of what was laſt ſuggeſted) I make no doubt, but that many experiments, whereby men are not preſently enabled to do what they could not before, may yet be very uſeful to men's intereſt, by diſcovering or illuſtrating the nature or cauſes of things. For though that famous diſtinction, introduced by the lord *Verulam*, whereby experiments are ſorted into luciferous and fructiferous, may be (if rightly underſtood) of commendable uſe; yet it would much miſlead thoſe, that ſhould ſo underſtand it, as if fructiferous experiments did ſo merely advantage our intereſts, as not to promote our knowledge; or, the experiments called luciferous, did ſo barely enrich our underſtandings, as to be no otherways uſeful. For though ſome experiments may be fitly enough called luciferous, and others fructiferous, becauſe the more obvious and immediate effect of the one is to diſcover to us phyſiological truths, and of the

other

other, to enable us to perform something of use to the proffessor; yet certainly there are few fructiferous experiments, which may not readily become luciferous to the attentive considerer of them. For by being able to produce unusual effects, they either hint to us the causes of them, or at least acquaint us with some of the properties or qualities of the things concurring to the production of such effects. And on the other side those experiments, whose more obvious use is to detect to us the nature or causes of things, may be, though less directly, and in somewhat a remoter way, exceedingly fructiferous. For since, as I have formerly observed, man's power over the creatures consists in his knowledge of them; whatever does increase his knowledge, does proportionately increase his power. And perhaps I should not much hyperbolize, if I should venture to say, that there is scarce any considerable physical truth, which is not, as it were, teeming with profitable inventions, and may not by human skill and industry, be made the fruitful mother of divers things useful, either to mankind in general, or at least to the particular discoverer and dexterous applier of that truth. To countenance this opinion of mine, I have already given you some instances, and reserve more for the last essays of this treatise; especially having observed it to have been a fault, which, though prejudicial enough to the interest of mankind, is very incident to the more sober and severe sort of philosophers, and perhaps more to them, than to others, to conclude every thing to be impossible, or, at least, unfit to be attempted, that cannot be performed by the already known qualities of things and ways of applying them; without considering, that as many simples of excellent virtues grow in wildernesses, and not by the highway's side; so divers admirable properties of things may be found, out of the customary progress, or beaten roads (if I may so speak) of nature; and that philosophers are oftentimes deceived, when they think they think they may have made a true and perfect analysis of the possible ways, whereby such and such effects may be produced. For nature by her subtlety oftentimes transcends and illudes the greatest subtlety of human ratiocinations. And as she may have quite other ways of working, than we are aware of, so the knowledge of some peculiar and concealed property of a thing may enable them, that are acquainted with it, to perform that with ease, which, by the known qualities of things, is either not at all to be performed, or not without great difficulty.

This seeming paradox you may find in due place confirmed; and in the mean while, to return to those learned men, who having attempted some things, and possibly performed a few in natural philosophy, would keep the world from expecting any great matters from it, I shall venture to say of them, that as the Jewish spies, though they brought their countrymen out of the land of *Canaan*, some few of the goodly fruits of that soil, yet bringing them withal a discouraging account of the difficulties they were like to meet with in conquering it, did the *Israelites* more harm by their despondency, than good by their fruits; so divers of the authors we are speaking of, though they may have presented us, with some acceptable fruits of their enquiry into experimental learning, yet by bringing up an ill report concerning the study of it, and thereby deterring irresolute persons from addicting themselves seriously to it, they have more prejudiced them by their despondency, than advantaged them by their experiments. And though I dare not, a chemist would not, scruple to persue the simile, and tell you, that as only those two of the spies, *Caleb* and *Joshua*, who made no doubt but, that they should conquer the fertile (though never so well fortified) land of *Canaan*, did really possess it, all their disanimated brethren wandering and dying in the wilderness; so none but those generous attempters, that dare boldly venture upon the difficulties, that surround the knowledge of nature, are like prosperously to overcome them, and possess what they contend for.

Numb. xiii. 14.

Numb. xiv. 28, 29, 30.

But I must leave this digression to proceed to the last advertisement I am to give you, which is, that I know you may possibly expect, that I should say something to you distinctly of the chief means, by which the naturalist may probably advance trades, and assist man, by the blessing of the author of nature, to recover part of his lost empire over the works of nature. And I confess, I have more than once had thoughts of a kind of project (if I may so call it) for the advance of experimental philosophy, consisting of such heads as these: a prospect of what probably may be attained to in physicks (both as to theory and practice.) A summary account of what is attained already. The imperfectness of our present attainments. What helps men now enjoy. The incompetency of our present helps. The hindrances and the causes of them. And the means and helps, that may be employed. To which other heads might in case of need be added. But notwithstanding the expectations you may have, that I should handle such subjects, and the thoughts I have had about them; I purposely waved the treating of them by themselves in the ensuing essays, partly, because these unelaborate discourses are not designed for a just treatise on the subjects handled in them, containing but such loose experiments and observations, as could without too much impoverishing other papers, be put together on this occasion; and partly, because I have in effect been careful to mention several of those things, that you might expect to find separately treated of; but knowing, that a far less discerning eye than your's may easily, if there be occasion, distinguish them, I thought it more convenient to interweave them with the other parts of the following discourse, since every proposition of a probable way to improve philosophy is also a ground of expecting those advantages, that may be hoped for from philosophy improved.

OF THE

USEFULNESS

OF

MATHEMATICKS

TO

NATURAL PHILOSOPHY.

OR,

That the Empire of MAN may be promoted by the Naturaliſt's skill in MATHEMATICKS, (as well pure, as mixed.)

IF it were not allowable for any but thoſe, that are thoroughly ſkilled in the abſtruſer myſteries of the mathematicks, to diſcourſe of thoſe diſciplines; the title of this eſſay, would, I fear, (*Pyrophilus*) make you think me guilty of preſumption, ſince you may perchance remember, that when you were converſant about thoſe ſtudies, I confeſſed to you, that the great authority of ſome famous modern naturaliſts had, for a while, diverted me from making any great progreſs in thoſe ſciences, by their reſolute denying them to be uſeful to phyſiology. But, as I do not pretend to have taken that pains, which elſe I might have done, to become a ſpeculative geometrician; ſo I conſider, that without underſtanding as much of the abſtruſer part of geometry, as *Archimedes*, or *Apollonius*, one may underſtand enough to be aſſiſted by it in the contemplation of nature; and that one needs not know the profoundeſt myſteries of it, to be able to diſcern its uſefulneſs. And therefore I ſhall venture to propound ſomething to you concerning this laſt named ſubject, eſpecially, ſince otherwiſe you may be influenced, as I once was, by the great authority of thoſe modern philoſophers, who would have the uſe of mathematicks, as diſciplines, that conſider only abſtracted quantity and figure, to be rather hurtful than advantageous to a naturaliſt, the object of whoſe ſtudies ought to be matter. But though theſe endeavour to keep men from thinking the mathematicks to be of any great uſe toward making a man a good naturaliſt, by alledging the extravagant opinions that *Kepler* himſelf, who was mathematician to three emperors, and ſome other modern aſtronomers, have broached or maintained concerning matters phyſiological; yet I confeſs, that after I began, by reflecting upon divers of my experiments, eſpecially mechanical, to diſcern how uſeful mathematicks may be made to phyſicks; I have often wiſhed, that I had imployed about the ſpeculative part of geometry, and the cultivating of the ſpecious Algebra I had been taught very young, a good part of that time and induſtry, that I ſpent about ſurveying and fortification, (of which I remember I once wrote an entire treatiſe) and other practick parts of mathematicks. And indeed, I think, that a competent knowledge in mathematicks (for a profound one is not always neceſſary) may be ſo ſerviceable to thoſe, that would become philoſophers, that I ſhall not ſcruple to mention it as another thing, which may increaſe your expectation from phyſiology, that thoſe, who paſs for naturaliſts, have, for the moſt part, been very little, or not at all, verſed in the mathematicks, if not alſo jealous of them. And I the leſs ſcruple to write to you on this ſubject, becauſe I do not know, that others have prevented me: for though the learned *Clavius*, and ſome other expoſitors of *Euclid*, have ſaid much of the uſefulneſs of geometry to other mathematical diſciplines; and though not a little has been ſaid in the praiſe of mathematicks in general; yet it is left free for me to diſcourſe to you of (what is the ſubject of this eſſay) the utility of mathematicks, in reference to modern phyſicks, and therein not only to the notions of the corpuſcular philoſophy, but even to practical and experimental knowledge.

Now there are ſeveral ſcores, upon which ſkill in mathematicks may be uſeful to the experimental philoſopher. For there are ſome general

general advantages, which mathematicks may bring to the minds of men, to whatever study they apply themselves, and consequently to the students of natural philosophy; namely, that these disciplines are wont to make men accurate, and very attentive to the employment they are about, keeping their thoughts from wandering, and inuring them to patience of going through with tedious and intricate demonstrations; besides, that they much improve reason, by accustoming the mind to deduce successive consequences, and judge of them without easily acquiescing in any thing but demonstration.

AND indeed the operations of symbolical arithmetick (or the modern Algebra) seem to me to afford men one of the clearest exercises of reason, that I ever yet met with, nothing being there to be performed without strict and watchful ratiocination, and the whole method and progress of that appearing at once upon the paper, when the operation is finished, and affording the analyst a lasting, and, as it were, visible ratiocination.

BUT, *Pyrophilus*, I may not insist on these, or the like general uses of pure mathematicks, since there are divers others, which more immediately respect natural philosophy.

AND to shew this the better, give me leave to premise to the following particulars, a couple of observations.

THE first is, that the phænomena, which the mathematician concurs to exhibit, do really belong to the cognizance of the naturalist. For when matter comes once to be endowed with qualities, the consideration how it came by them, is a question rather about the agent or efficient, than the nature of the body it self. So the image or picture, that a man sees of his face in a looking-glass, though that be an artificial body, falls as well under the speculation of the naturalist, as when the like picture is presented him by calm and clear water. And the rain-bows, that are often artificially made in grottos, by dispersing the water of fountains into drops and showers, have a just title to his contemplation, as well as the rain-bow that is formed in the clouds. And the echoes, that are admired in some of those grottos, purposely and artificially contrived to afford rare ones, do as well belong to his cognizance, as those that nature makes in ruder dens, and other cavities of hills and mountains. And indeed most of those phænomena require (for the main) the same solutions, whether the skill of man do or do not intervene to exhibit them.

THE second consideration, which I am often obliged to repeat, is this; that since man's power over the creatures depends chiefly upon his knowledge of them, whatever serves to increase considerably his knowledge, is likely, either directly, or in its consequences, to add to his power: which two advertisements being thus given you, *Pyrophilus*, I now advance to the particulars, whose mention they made me suspend.

1. AND first, these disciplines teach men the nature and properties of figures, both upon surfaces and solids, and the relations (for they can scarce be properly called proportions) betwixt the surface and solidity of the same body. It is true, that matter, or body, is the subject of the naturalist's speculations; but if it be also true, that most, if not all the operations of the parcels of that matter (that is, of natural bodies) one upon another, depend upon those modifications, which their local motion receives from their magnitude and their figure, as the chief mechanical affections of the parts of matter; it can scarce be denied, that the knowledge of what figures are, for instance, more or less capacious, and advantaged or disadvantaged, for motion or for rest, or for penetrating or resisting penetration, or for the being fastened to another, &c. must be of considerable use in explicating many of the phænomena of nature; and it is sufficiently known, how much of the doctrine of figures may be learned from geometricians, who treating expresly and copiously of triangles, circles, surfaces elliptical, parabolical, hyperbolical, and other plain figures; as also of spheres, cones, cylinders, and especially prisms, pyramids, cubes, and regular bodies, intimate also the methods of judging of the figures of other bodies, that are either composed of them, or may, by reason of some analogy, be referred to them.

THERE are divers properties, as well of planes and solid figures, and their habitudes to each other, as of such lines as are described by motions, or wherein motions may be made; the knowledge whereof may be of good use not only to the speculative naturalist, but the practical.

TO know the proportion, that *Archimedes* has demonstrated to be between a sphere and a cylinder, and either of those to a cone so and so qualified; or to know, that a triangular pyramid is the third part of a prism, having the same base and height; and in a word, to know the proportions between geometrical bodies, may sometimes be of good use, in cases, where we can procure the one, and not the other, or at least not so well as the other. Of this an instance is given us by the ingenious *Marinus Ghetaldus*, (as I find him cited by a late mathematician) who tells us, that *Ghetaldus* finding it very difficult to procure an exact metalline sphere, wherewith to examine the proportion, in point of weight, between heavy bodies of the same bulk, found, that yet he could get a cylinder of tin to be turned true; and having therewith made his experiments or observations, it was easy for him, knowing out of his *Archimedes*, that the proportion of a cylinder, whose basis is equal to one of the great circles of a sphere, and whose height is equal to the diameter of that sphere, is to that sphere *in ratione sesquialterâ*, as they speak, i. e. has the same proportion, that three has to two; it, was, I say, easy for him, who had often had occasion to weigh his cylinder exactly, by subtracting a third part of the whole weight, to find in the remainder the desired weight of a sphere of tin, whose diameter was equal to that of the basis, or to the height of the cylinder *: which weight of a sphere of a known diameter being once obtained, he deduced from them the weights of the other spheres he had occasion to imploy, about the construction of those tables, which have been much made use of by divers succeeding mathematicians. And what applications I have made of the same Archimedean theorem, I may elsewhere inform you.

It being also taken for granted by divers modern geometricians and engineers, that the excellent *Galilæo*, and his not degenerate disciple *Torricellius*, had demonstrated the line, which a heavy body, projected, and even the bullet, shot out of a cannon, describes, to be parabolical; it may be of moment in the practice of gunnery, and in reference to divers experiments to be made with other projected bodies, to be well versed in the nature of the parabola and parabolical lines, which are also thought to be capable of doing wonders in burning-glasses, in case these metalline specula can be brought to a parabolical figure; one of whose remarkable properties is, that all the beams, that, being parallel to the axis, fall upon the internal superficies, are reflected to one point or focus; where consequently, if the burning-glass be any thing large, the heat must be very intense, especially in comparison of a spherical burning-glass of the same bigness.

And as for delightful and recreative experiments, you will easily allow me, that there are abundance of catoptrical ones of that sort, which depend upon the figure of spherical, cylindrical, and other sorts of reflecting glasses.

2. I might here tell you, *Pyrophilus*, that pure mathematicks themselves, setting aside the assistance they are wont to give to mixed mathematicks, may be of use to human life, and to the experimental naturalist; of which I shall give you, as a specimen, this notable example.

The properties of arithmetical and geometrical progressions in numbers seem to have very little to do with the practice of weighing out things in shops and warehouses. And yet by the knowledge of the double progression, beginning from an unite, (as arithmeticians call that, wherein the consequent is still double to the antecedent) as 1, 2, 4, 8. a great deal of cumber, and sometimes of charge, may be saved. For with three weights you may weigh all the pounds, that are from one to seven inclusively; with four weights, all those that exceed not fifteen pound; upon which observation is grounded the division of some boxes or sets of weights, used by our goldsmiths. And if you would, as is very usual, put weights (when there is occasion) in both scales, to help the thing to be weighed to bring the ballance to an æquilibrium, than the tripple progression (i. e. where the numbers increase in a triple proportion, as 1. 3. 9.) has a much more notable property for our purpose; by considering which, the industrious *Stifelius* concluded, that by three weights, you may weigh any number of pounds from one to thirteen inclusively; with four weights, any number of pounds from one to forty inclusively; with five weights, any number of pounds not exceeding sixscore and one; and with but six weights, any number of pounds from one to three hundred and sixty four. But the method of ordering so few weights to serve so many purposes is best found out by symbolical arithmetick, or algegra, by which I have taken pleasure to work so fine a problem; which, because it is applicable, not only to pounds, but to the parts of pounds, and those of differing denominations, it may be of so great use to you, if ever you busy your self about statical experiments, that I shall to the end of this essay annex a table, to shew, what weights are to be taken in every possible case, which I found ready calculated to my hand by the ingenious *Franciscus a Schooten*, professor of mathematicks at *Leyden*.

To the former instance, of the use that an experimenter may make of pure mathematicks, I might, if it could be sufficiently delivered in a few words, add the method of computing the combinations, that may be made of any number of things proposed, which some mathematicians call *Regula combinatoria*. For though I remember not to have found this method fully handled in any one author, even among the modern algebricians; yet, as it is delivered by some arithmeticians, it is by no means to be despised, but, as it may be managed by symbolical arithmetick, it will, if I mistake not, want nothing, but the being skilfully applied by the naturalist, to be on certain occasions very serviceable to him.

3. We may take notice in the next place, that mathematicks may much help the naturalist, both to frame hypotheses, and judge of those, that are proposed to him, especially such as relate to mathematical subjects in conjunction with others.

What wretched theories the ignorance of mathematicks has made naturalists, otherwise very considerable in their way, frame and propose, may be evidently shewn in the accounts that *Epicurus*, and his paraphrast *Lucretius*, give of the sun, and other celestial bodies. And indeed what satisfactory account can be given of the varying lengths and vicissitudes of days and nights, and the eclipses of the sun and moon, the stations and retrogradations observed in planets, and other familiar celestial phænomena, without supposing these great mundane bodies to have such situations in respect to one another, and to move in such lines, or at least to be made to appear to move in them by the motion of the earth in such a position, and in such lines? Nay, how without the knowledge of the doctrine of the sphere will the naturalist be able to make any sober and well grounded judgment in that grand and noble problem, which is the true system of the world? which is endeavoured to be solved after such differing manners by the Ptolomæans and Peripateticks, by the Tychonians and by the Copernicans, both less and more modern.

That then the knowledge of celestial bodies is not well to be attained, nor consequently

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* Archimed. proposit. 32. lib. 10. de sphæra & cylindro.

quently the theories, proposed of them, to be intelligently judged of, without arithmetick and geometry (those wings, on which the astronomer soars as high as heaven;) he must be very little acquainted with astronomy, and particularly with the various, and too often intricate theories of planets, that can doubt. And truly, when I consider the astonishing distance and immensity of the celestial bodies, and those almost numberless fixed stars (each of them perhaps much vaster than the whole earth,) which in a clear night I take pleasure to gaze at through the better sort of telescopes, both in the milky way, and in other parts of the sky, that seem not so much as whitish to our eyes; I cannot but highly prize a science, that acquaints us, that what we know of so much of the universe as the globe we inhabit and call the world, is but a point to it, taking up a little more room in it, than a physical center in the sphere.

THE usefulness also of pure mathematicks to geography is likewise evident: and sure inquisitive men ought not to despise this and the former part of learning, without which, as I was lately saying, they cannot know so much, as whether the earth we live upon, moves or stand still.

THERE are also divers phænomena of nature, that are neither astronomical, nor geographical, where the usefulness of mathematicks is manifest enough. For as to the phænomena of that sense, to which the naturalist is most beholding, sight, what a pitiful account is given of them by those Aristotelians, physicians, and other writers, without excepting many good anatomists, that have been strangers to mathematicks, in comparison of what has been done (not to mention *Euclid*, *Alhazen*, and *Vitelius)* by *Kepler*, *Scheiner*, *Herrigon*, and some other modern mathematicians.

AND it is evident to those, that are acquainted with dioptricks, that without some knowledge, not only of the properties of convex bodies, and of the laws of refraction from and towards the perpendicular, (as the masters of opticks speak) but also of the properties of lines, as circular, parabolical, hyperbolical, &c. and figures, as ellipses, circles, parabolas, hyperbolas, &c. it is almost impossible, either well to explicate most of the phænomena of that noblest of our senses, sight it self, or to make a well grounded judgment of others explications of them. He, that is altogether a stranger to this part of mathematicks, will scarce be able to conceive the reason of the admirable fabrick of the eye, and how the christalline humour does by its convex figure (like a lenticular glass) refract and converge the beams, (or at least the pencils) that proceed from the visible object, that they may paint the more lively picture of it upon the retina at the bottom of the eye: nor will he understand why, by reason of the decussation of the beams within the eye, this picture must be made inverted, though we apprehend the objects themselves in a right posture; nor why small objects, placed near the eye, where they are seen under a wide angle, appear as big, as very much greater, that are seen at a greater distance from it. And much less will he be able to understand the reason of those many delusive apparitions, exhibited by concave, convex, conical, and cylindrical glasses, the catoptricks, or doctrine of reflex vision, belonging yet more to the mathematicks than dioptricks do.

4. AND since that from the magnitudes of divers bodies, or of several parts of the same body, and so likewise from their degrees of celerity in their motion, there will arise a certain respect, which if they be but two, geometricians call a ratio, and if more than two, a proportion, (though these terms are oftentimes confounded, and promiscuously employed by authors:) and since proportion is so frequently to be met with in the works of him, who by an eminent, though apocryphal writer, is truly said to have made all things in number, weight, and measure; and since the doctrine of proportion, as such, belongs to the mathematician, as the noblest part of those sciences he treats of; I think it may safely enough be affirmed, that he, that is not so much as indifferently skilled in mathematicks, can hardly be more than indifferently skilled in the fundamental principles of physiology. Nor perhaps would it be rash to say, that the fifth book of *Euclid*'s elements, where the doctrine of proportions is chiefly delivered, may prove more instructive to the naturalist, than the fifth book of *Aristotle*'s physicks. And therefore I do not so much wonder, that *Plato* should over the gate of his school place an inscription, (ὐδεὶς ἀγεωμέτρητος εἰσίτω) forbidding the entrance to persons unacquainted with geometry, as unfit to judge of what was there taught.

NAY this, though you may think it strange, is very true, that there are some considerable phænomena of nature, which are so far from being explicable by their causes, that men cannot so much as understand what is meant by them, without some knowledge of the doctrine of proportions. As, for instance, when the teacher of opticks tells us, that the increments of light are *in duplicatâ ratione distantiarum, secundùm quas à corporibus recedunt, à quibus primum efficiuntur*; he, that knows nothing of proportions, cannot tell so much as what they mean by this theorem, much less whether or no it be true. And so, when the same proposition is by the diligent *Mersennus* * applied also to sounds, a common reader would not at all understand him, if he did not add by way of explanation, that if, for instance, the noise of a piece of ordinance be heard a league off, that noise will be four times stronger, if it be heard but at the distance of half a league. Nor will this example it self give such a reader, as we speak of, a clear understanding of the proposed theorem. But a considerabler instance in this kind may be afforded us by the noble discovery of the moderns, especially *Galilæo*, who observe, that when a heavy body descends through the air, the spaces past through, from the beginning to the end of the motion, are among themselves in a (not double, but) duplicate ratio of the moments or equal divisions of

* Harmonic. lib. I. prop. 12.

of time spent in the full; which requires the knowledge of what a duplicate proportion is, to be well understood: but it may in some sort be explained, and so noble a phænomenon must not be here omitted, by saying, that *Galilæo* affirms himself to have observed, that a brass bullet of 100 pound will, in the space of one minute of an hour, descend an hundred Florentine cubits, (which some reckon to be 180 feet of ours, and consequently, saith *Mersennus*, four cubits in one second, or sixtieth part of a minute; and by adding, that the bullet falls in such a ratio, that the acceleration of the motion is made according to the progression of odd numbers, beginning from an unite, or one; so that if in the first moment of time the weight fall down one fathom, in the second moment it must descend three fathom; in the third, five fathom; in the fourth, seven; in the fifth, nine: in the sixth, eleven; and so onward. Whence *Mersennus* gives this rule, to know how far the weight will descend in a determinate time assigned; and by knowing how far it has descended, to calculate how long it was in falling. * *Regula generalis*, says he, *hæc est. Si dentur tempora, & quærantur spatia, quadrentur tempora, & habebuntur rationes spatiorum. Si dentur spatia, & quærantur tempora, investigetur latus spatiorum, & dabitur ratio temporum.*

Divers other instances might be produced, to manifest the requisiteness and advantageousness of some knowledge in mathematicks to a speculative naturalist: but I shall content myself to name one more, viz. that the grand theorem or rule of the staticks, that in the ballance, or resembling instruments, the proportion betwixt the equivalent weights, and their distances from the fulcimentum or prop, is reciprocal, (so that it is usual with butchers, and other tradesmen, to weigh in the statera, commonly called the stiliards, 10 or 20 pound weight, for instance, hung near the fulciment, with one pound weight, placed on the other side of the beam, at 10 or 20 times distance from it,) and many other theorems, that serve to explicate the properties of the grand instrument of nature, motion, (especially as produced or modified by weight, or equivalent force variously adapted, and applied) cannot well be understood without an insight into geometry, and especially the doctrine of proportions; and how much the knowledge of the principles and theorems of the mechanicks may assist the naturalist, both to explicate many of nature's phænomena, and to try experiments, and work great changes on her productions, men will then more readily confess, when they shall better discern how many of her works are but engines, and do operate accordingly.

5. And give me leave, *Pyrophilus*, to add in this place, that the doctrine of proportions, as it is the soul of the mathematicks themselves, so it may be of vast, though perhaps yet unheeded, use in physiology too; not only as it helps the naturalist (as we have newly seen it does) to understand divers phænomena of nature, but as it may enable him to perform divers things, which he could not perform without it; of which though I may have occasion to give you hereafter in other papers several examples, yet I shall now mention two or three for illustration sake.

That the pendulum is the accuratest instrument, that we yet have of measuring short spaces of time, I presume you do not doubt: and I need not tell you, that he, who would know what length a pendulum must be of, to measure by its swing some determinate space of time, as, for instance, a half second, (or half the sixtieth part of a minute,) must find it out by trial and observation, if he be not unacquainted with the doctrine of proportions: but in case he is versed in that, as well as in the phænomena of pendulums, he may from the length of one pendulum, that exactly measures a known part of time, without making particular trials and observations, deduce the length of pendulums that will serve to measure other divisions of time. For instance, that diligent observer *Mersennus* assures us, that he found by frequent trials, that a slender string with a pistol or musket bullet at the end of it, whose length comprehending the bullet was three foot and a half, (elsewhere he mentions three foot and a 27th) vibrates second (minutes:) this now being taken for granted, and it being a received theorem concerning pendulums alike in all things but length, that the lengths are in duplicate proportion to the times in which their vibrations are respectively performed, or are as the squares of the vibrations they perform in the same time, and consequently, the times are in subduplicate proportion to the lengths of the pendulums; if a man would, as I was saying, have a pendulum that shall vibrate half-seconds, he must not take, as one unacquainted with these things would be apt to do, a pendulum of a foot and three quarters, which is one half the length of that which vibrates a whole second, for such a pendulum would prove much too long for his purpose, nor need he by multiplied observations laboriously find out how much it is too long, (which oftentimes for want of a standard he cannot do,) but since the proportion between a second and half a second is double, and the proportion betwixt the length of the strings, that are to vibrate these two differing spaces of time, must be duplicate of the proportion of the times themselves, it follows, that the length of the strings must be as four to one, which is the duplicate of the proportion of two to one, and so the length of the shorter string must be but a fourth of that of the longer.

This, if it were needful, might be confirmed by a problem of the learned *Ricciolo*'s, whereof I shall here give you an example, because I may hereafter have occasion to shew you the farther use of it. Let us then suppose, to avoid fractions, that a pendulum, that vibrates seconds, is three intire foot long, (as indeed some modern mathematicians tell us it is, and as it may well be according to the measures used in someplaces.) If then you multiply 3600, the

* Mersen. Harmon. Lib. II. Propos. 24. Corollar 1.

the square of the vibrations, which are 60, that your three foot pendulum makes in a second, by the length of the pendulum, which is 36 inches, and divide the product, viz. 129600, by 9 inches, the fourth part of the length of the former pendulum; and if lastly, of the quotient (14400) you extract the square root, you shall find it to be 120, that gives you the number of vibrations, that will be made in a second by a pendulum of nine inches long, and this root being twenty, which is the double of sixty, you may see, that to make a pendulum, that shall vibrate half-seconds, it must be but one quarter as long as that, which vibrates whole seconds. And if I thought you were like to think these rules as strange, as a person wholly unacquainted with the nature of pendulums, and the doctrine of proportions may do; I would invite you to consult experience, as I have purposely done in differing pendulums, that divide a minute into seconds, half seconds, and quarter-seconds; since though your trials should not be very nicely made, they may suffice to persuade you, that the above-mentioned rules are either accurately true, or at least true for the main, and therefore true enough to be very useful in many occurrences.

To the above-mentioned instances afforded by pendulums I shall here add but one more, that comprehends many thousands; for the art of composing of that great variety of harmonious tunes, that makes musick so delightful to us, depends upon the doctrine of proportions. And he, that being well skilled in that, knows how to apply it to the notes or words proposed, according to the observations, which experience has afforded, of the gratefulness of such and such consonancies, &c. may out of his own head compose a strange variety of new and pleasing tunes, which are so many exercises, that man makes of the power his skill gives him over the bodies, of which his musical instruments consist, and over those which they affect.

6. I know not, *Pyrophilus*, whether I may not reckon amongst the advantages, that mathematicks may afford the naturalist, that they will in many cases suggest to him divers new experiments, whereby to vary those, wherein the figures of bodies, the lines of motion, as also numbers, proportions, and the like affections, which the mathematician is wont to treat of, may come into consideration. For it is very likely, that those suggested experiments, which either would not be thought on, or could not be skilfully proposed, by a person not versed in mathematicks, may, either immediately, or upon the score of the applications, that may be made of them, prove serviceable to men: of which I hope in one of the following essays, to give you some instances.

See Essay IX.

I care not to mention to you, how great a variety of trials and observations, about the best way of levelling great guns, and the differing distances, to which they will carry at such and such elevations, and the lines described by the motion of the bullet, and other particulars belonging to the art of gunnery, have been proposed and tried, upon the hints suggested by geometry's mathematical disciples (especially) and others, because many good men wish these fatal arts had been less understood. And therefore I shall rather put you in mind of the great variety of phænomena, which pure mathematicks have helped men to discover and derive from these familiar observations; that a beam of light, passing through differing mediums, is not continued in a streight line, but broken or refracted; and, that in such and such conjunctures of circumstances, the sun or moon will suffer an eclipse, that will obscure such a part of the body, and last from such a time to such a time: from which observations of eclipses divers very considerable things have been deduced by mathematicians, not only as to astronomy, but also geography, navigation, and chronology. And he that considers, what the doctrine of proportions, and of concords (or, as our musicians call them, cords,) and discords, has contributed to the great number of musical instruments, that have been actually made, and delightfully practised, and that it may afford the naturalist divers hints applicable to other purposes, (which I shall hereafter have occasion to intimate,) he, I say, that considers these things, especially if he be also acquainted with ingenious, pleasant, and some of them useful, experiments, that have been or may be derived from the observations, that when a beam of light falls upon a body, and rebounds from it, the angle of incidence is equal to that of reflection; that if the superficies of the body be curve, the angle is to be estimated as if it fell upon a tangent to that superficies; that if the beam penetrate the body, and come to it through a thinner medium, it is refracted towards the perpendicular, if through a thicker medium, from the perpendicular; he, as I was saying, that shall consider these things, and withal, what a great variety of propositions, as well problems as theorems, have been deduced by mathematicians by the help of these few observations, and of as few propositions touching the place of the object seen by the help of specular and dioptrical glasses, will easily grant, what by so many instances I have been endeavouring to prove.

7. I come now to the consideration, wherewith I shall conclude this essay, viz. that divers disciplines, that are reckoned amongst the mixed mathematicks, are chiefly practical, and may assist the naturalist in making experiments and observations, which he either could not make, or could not make so accurately without them: as may appear, partly by the art of dialling, which teaches how to measure time, and tends chiefly to practice; partly by the art of perspective, which is of great use to represent solids and distances upon a small and plain superficies, and is very serviceable to the limner's art; wherein if scholars and travellers were more generally conversant, the history of nature would be far better adorned with lively representations of plants, animals, meteors, &c. and also by several parts of the art of navigation, and particu-

larly that, which they call histriodromia, or the doctrine of the lines, by which pilots make their ships to sail. Now if in these and divers other instances, that may be given, it must be acknowledged, that mixed mathematicks may be serviceable to the naturalist, and assist him to promote the empire of man; it ought not to be denied, that pure mathematicks themselves, as vulgar arithmetick, geometry, and algebra, may be of use to the naturalist, since it is from those speculative parts of the mathematicks, that not only these other more practical disciplines are derived, but a greater number of those disciplines, that are called mixed mathematicks, may, according to what I elsewhere observe, be hoped for. For as sounds and pure mathematicks make up musick, and water with the same sciences make hydrostaticks; so, as I elsewhere note, by a further application of the same parts of knowledge to other subjects, (and in some cases even to the same) those disciplines, that are called mixed mathematicks, may be advanced probably as to number, as well as certainly as to usefulnesses and variety of experiments. Nor is it only in those parts of learning, that I have now particularly named, that useful applications may be made of the theorems and problems of pure mathematicks, since upon these sublime sciences do also in great part depend those other mathematical disciplines, which are wont (by a synecdoche) to be called mechanical, and which it is now time, that I pass on to consider.

OF THE USEFULNESS OF MECHANICAL DISCIPLINES TO NATURAL PHILOSOPHY.

SHEWING,

That the Power of Man may be much promoted by the Naturalist's skill in MECHANICKS.

TO prevent the danger of stumbling (as they speak) at the threshold, I shall begin this discourse with advertising you, that I do not here take the term mechanicks in that stricter and more proper sense, wherein it is wont to be taken, when it is used only to signify the doctrine about the moving powers (as the beam, the leaver, the screws, and the wedge,) and of framing engines to multiply force: but I here understand the word mechanicks in a larger sense, for those disciplines, that consist of the applications of pure mathematicks to produce or modify motion in inferior bodies: so that in this sense they comprise not only the vulgar staticks, but divers other disciplines, such as the centrobaricks, hydraulicks, pneumaticks, hydrostaticks, balisticks, &c. the etymology of whose names may inform you about what subjects they are conversant.

Now that these arts (if you will allow them that name) may be of great use to the experimental philosopher, and assist him to enlarge the empire of man, may be made probable by this general consideration, that divers of those things, which in the former essay have been evinced to make the mathematicks useful to the naturalist, may be applied *mutatis mutandis* to the mechanicks also. Besides, that these disciplines have some advantages peculiar to themselves. But the truth of what is thus represented in general terms will possibly be better discern'd, and more persuasive, if we descend to some particulars.

I. FIRST then, the phænomena afforded us by these arts ought to be looked upon as really belonging to the history of nature in its full and due extent. And therefore as they fall under the cognizance of the naturalist, and challenge his speculation; so it may well be supposed, that being throughly understood, they cannot but much contribute to the advancement of his knowledge, and consequently of his power, which we have often observed to

be

be grounded upon his knowledge, and proportionate to it. When, for inſtance, we ſee a piece of wood, ducked under water, emerge again and float, even vulgar naturaliſts think, that it belongs to them to conſider the reaſon of this emerſion and floating, which they endeavour to render from the poſitive levity, which they fancy to be (upon the account of the air and fire) inherent in the wood, though ſome woods, that will ſwim in water, being put into oil, or high rectified ſpirit of wine, may ſink.

But I ſee not, why it ſhould not belong to philoſophers to conſider and inveſtigate the reaſon, why one part of floating wood appears above the water, whilſt the other keeps beneath it; and why the extent part is equal to the immerſed, or either greater or leſſer than it, in ſuch a determinate proportion; and why the ſame wood will ſink deeper in ſome waters than in others, (as in a river than in the ſea) as on the other ſide ſome woods will ſink lower than others in the ſame water. For if theſe things be duly examined, as they may by the help of hydroſtaticks, not only the cauſe of theſe and the like phænomena will be diſcovered; but by the applications of that diſcovery an eaſy way may be deviſed to meaſure and eſtime at the differing ſtrength of ſeveral ſalt ſprings, and alſo of divers kinds of lixiviums, and brines; to which may be added divers other practical corollaries from the ſame diſcoveries, which I ſhall hereafter have occaſion to particularize.

II. The mechanical diſciplines help me to deviſe and judge of ſuch hypotheſes, as relate to thoſe ſubjects, wherein the notions and theorems of mechanicks either ought neceſſarily to be conſidered, or may uſefully be ſo.

Of this we have inſtances, not only in thoſe engines, that are artificial, and are looked upon as purely mechanical, as the ſcrew, the crane, the ballance, &c. but in many familiar phænomena, in which the theorems of mechanicks are not wont to be taken notice of to have an intereſt. As in the carrying a pike or muſket on one's ſhoulder, in the force of ſtrokes with a longer or ſhorter ſword or other inſtrument, the taking up and the holding a pike or ſword at arms-length, and the power, that a rudder has to ſteer a ſhip; in rowing with boats, in breaking of ſticks againſt one's knee, and in a multitude of other familiar inſtances, of which the naturaliſt's ſkill in mechanicks will enable him to give a far more clear and ſolid account, than the ancient ſchoolmen, or the learnedeſt phyſicians, that are unacquainted with the nature and properties of the centre of gravity, and the ſeveral kinds of levers, the wedge, &c.

III. Nay, there are ſeveral doctrines about phyſical things, that cannot be well explicated, and ſome of them not perhaps ſo much as underſtood, without mechanicks.

That, which emboldens me to propoſe a thing, that ſeems ſo paradoxical, is, that there are many phænomena of nature, whereof though the phyſical cauſes belong to the conſideration of the naturaliſt, and may be rendered by him; yet he cannot rightly and ſkilfully give them without taking in the cauſes ſtatical, hydroſtatical, &c. (if I may ſo name them) of thoſe phænomena, i. e. ſuch inſtances as depend upon the knowledge of mechanical principles and diſciplines.

Of this we have an obvious example in that familiar obſervation, that we partly touched upon juſt now about the ſwimming and ſinking of wood in water. For if it be demanded, why wood does rather ſwim upon water than ſink to the bottom of it, a ſchool-philoſopher would anſwer, that wood abounds with air, which being an element very much lighter than water, keeps it aloft upon the ſurface of that liquor. But this anſwer will ſcarce ſatisfy a naturaliſt verſed in hydroſtaticks. For not now to queſtion what is taken for granted, that there is a poſitive levity, and that the air is endowed with that quality, experience ſhews us, that though when wood is not heavier than ſo much water, as is equal to it in bulk, it will ſwim; yet in caſe it be heavier than ſo much water, it will ſink. As we ſee in divers woods, and particularly in guaicum, which I therefore the rather name, becauſe chemiſts obſerve, that if it be burnt, it leaves far leſs aſhes (and ſuch are ſuppoſed to contain the terreſtrial and heavy parts) behind it, than many woods, that we know will float in water. And though ſtones and iron be, upon the ſcore of their weight, believed to be bodies, that have little air in them, yet if the liquor, into which they are put, be heavier, bulk for bulk, than they, they will not ſink but float, and if forcibly depreſſed, they will emerge; as you may try, when you pleaſe, by putting ſtones or iron, or the like ponderous body upon quick-ſilver, or melted lead; ſo that we need not here conſider, whether air be, or be not predominant in a propoſed body, when we would know, whether it will, or will not ſink in an aſſigned liquor.

And though we ſhould admit the air, whether included in the pores, or looked upon as an elementary principle, to be the cauſe of its being lighter than an equal bulk of liquor, yet the air would be but the remote cauſe of its ſwimming, its immediate cauſe being, that the floating body is lighter than an equal bulk of the liquor, and therefore the ſame body, without acquiring or looſing air, may ſwim in one kind of water, and ſink in another. As in the caſe of heavy bodies, as loaden ſhips, that having proſperouſly ſailed over the ſea, are recorded to have ſunk as ſoon as they come into harbour, i. e. into a more freſh water; and an egg, that will ſink in common water, will ſwim in a ſtrong brine. Nay a body may (as I, and others have tried) be ſo poiſed in water, that if the liquor be a little warmer, than when the body was poiſed in it, the body will ſink; as it will emerge again upon the refrigeration of it.

And if this general anſwer of the lightneſs of the air will not give ſo good an account as hydroſtatical principles, why a piece of wood will float or ſink, it will much leſs give ſo ſatisfactory an account, why differing woods in the ſame water, or the ſame piece of wood in differing waters, will ſink juſt ſo far, and no farther; whereas, by hydroſtatical principles, the phænomenon is eaſy to be accounted for,

for, according to that theorem of *Archimedes* *, περὶ τῶν ὀχυμένων, that solids lighter than the liquor they are put into, will sink in it so far, as that as much of the liquor as is equal in bulk to the demersed part, be equal in weight to the whole floating body: whence these corollaries are derived, that a floating body has the same proportion in weight to as much liquor as is equal to it in bulk, as the immersed part of the body has to the whole body. And likewise, that as much liquor, as is equal in bulk to the whole body, has the same proportion in weight to the said body, as the whole body has to that part of itself, which is beneath the surface of the liquor. And as these corollaries determine the proportion between the immersed and extant part of the floating body; so (to shew you, that these theories lead to practice) they suggest the way of making a small and light instrument, elsewhere described, to measure by a floating body the differing gravities of several liquors in reference to one another, as well as to the body itself. And upon the same grounds, the learned *Stevinus* shews, that if you know what part of a floating body is immersed in a liquor, whose specifick gravity is also known, as it easily may be, you may presently find the weight of the whole solid body, let it be never so much too great to be weighed in ballances or statera's, yea, though it were a vast ship itself; as supposing, that that part of such a vessel, that lies under water, should be 100,000 cubick foot, and that a cubick foot of water weighs 70 lb. (which though it be not the weight we have observed a foot of water English measure to amount to, yet that alters not the general rule,) by multiplying 100,000 by 70, the product will be 7,000,000 lb. for the weight of the whole ship, with all that is contained in it, as ballast, ordnance, &c. or rests or leans upon it. If I should ask a meer school-philosopher, why sucking-pumps will not raise water higher than 40 foot, (though it be commonly presumed they will raise it to any height,) or why in an inverted siphon of glass, if you pour water and quicksilver in a sufficient quantity, the surface of the water in one leg of the siphon, will not be in a level with the surface of the quicksilver in the other, but 13 or 14 times as high above the bottom of the siphon: or why, if a piece of iron, and a piece of marble or a flint, &c. be equiponderant in the air, if the scales be let down into the water, the metal will appear far heavier than the stone: if, I say, I should ask a meer naturalist both these or the like questions, I doubt I should much more perplex him, than he would satisfy me. And it were easy to add a multitude of examples, whereof a good account will scarce be given by a naturalist, that is unacquainted with mechanicks, and may easily be assigned by one that is skilled in them. But referring the schoolmen to *Aristotle*'s mechanical questions, to shew them the necessity and usefulness of mechanical knowledge, to give the solution of sundry phænomena, that frequently occur, I will only add an example or two to make good the most paradoxical part of what I was saying; namely, that there are divers physico-mechanical phænomena, which are not to be, I say not explicated, but so much as well understood, without the knowledge of mechanical disciplines.

THERE is a considerable theorem in hydrostaticks, which is thought to have been first taken notice of by *Mersennus*, and in a late writer, is thus expressed: *Velocitates motus aquæ descendentis & effluentis per tubos æqualium foraminum, sed inæqualium altitudinum, habent subduplicatam rationem altitudinum.* Of which the corollary is, that the tubes are in a duplicate ration to that of the velocities of the water, that subsides in, and runs out of them; so that to make one tube at a circular hole of the same diameter run out in the same time twice as much water as another, the greater ought to be not only twice, but four times as long as the shorter. And of the same proportion (my tryals about which I may elsewhere acquaint you with) divers other practical applications may be made, which must not be here insisted on.

IV. AS I formerly said of the mathematicks, so I now say of the mechanicks, that they may assist the naturalist to multiply experiments by those enquiries, that they will suggest, and those inferences and applications, whereto they may lead us.

OF this we have a noble instance in the great variety of tryals, which enquiries, versed in hydrostaticks, and other mechanical disciplines, have upon the score of their being so qualified, been either prompted, or at least assisted to make, about the famous quicksilver-experiment devised by *Torricellius*; about which though so much has been done already, yet almost every year brings forth new phænomena.

ANOTHER example to our present purpose we may take from the great number of new propositions, that the diligent *Mersennus* has given us in his balisticks, about the force and effects of bows, and the like springy bodies. But a yet more noble instance is given us by the most ingenious *Galilæo*, who, as we may learn from the already mentioned French writer, that has given us an account of *Galilæo*'s new thoughts in that language, has published so many propositions (of which he sets down 19 or 20, with the demonstrations) about the resistance of bodies to be broken, and the weights requisite to break them, and the lengths, at which they may be broken by their own weight, that he has reduced them into the form, and given them the title of a new art.

TO all which I shall need to add no more, than that he, who knows and considers, what a variety of useful propositions have been, or may be mechanically deduced from the observation of *Archimedes*, that a solid body weighs less in water than in the air, by the weight of water equal in bulk to that body, will easily dispense with me for not adding any farther instances on this occasion.

AND

* Lib. I. Prop. 5.

AND the mention of this hydroſtatical propoſition of *Archimides* falls in the more properly in this place, becauſe it will warrant me to tell you, that divers mechanical theorems are not only fertile in other theorems, but in uſeful applications too, of which I may hereafter have occaſion to give you ſome examples, by acquainting you with the uſes I have made of the lately mentioned propoſition of *Archimedes*, and ſome corollaries, that partly by others, and partly by us, have been inferred from it.

V. BESIDES the utilities, that may be aſcribed to the mechanicks in common, with the more ſpeculative mathematical diſciplines, they have ſome, as I formerly intimated, that are more peculiarly their own, ſince they may be of great uſe to the naturaliſt in making of ſuch inſtruments and tools, as for many of his obſervations, trials, and other purpoſes, he may either abſolutely need, or advantageouſly imploy.

OF this we have an example in the mariner's compaſs, as it is called; which is ſo neceſſary to thoſe remote navigations, whereto natural philoſophy and mankind owes ſo much. For though *Baptiſta Porta* * does, as well as other authors, aſcribe the invention of the directive faculty of the magnetick needle to one of his country-men (*Amalphi*, in the kingdom of *Naples*,) yet he confeſſes, that for want of the knowledge of making ſuch ſea-compaſſes as we now uſe, this lucky inventor was fain to make uſe of a piece of wood or ſtraw, to keep the needle a float, and then imbue it with a magnetick vertue; which was a ſhift ſubject to great and manifeſt inconveniencies. And indeed, notwithſtanding the knowledge of the verticity of magnetical needles, if by that of the properties of the center of gravity, or ſome practices derived thence, ſome men, verſed in mechanicks, had not deviſed a way ſo to poiſe the needle, that notwithſtanding the rolling and toſſing of the ſhip, it will continue horizontal enough to direct the pilot; what would become of him in thoſe ſtorms, when he has moſt need of a faithful guide?

BY the help of the centrobarical doctrine, mechanicks have been enabled to make thoſe dipping needles, whoſe phænomena are very odd: and though, as far as I have tried, they yet ſeem uncercain enough; yet it may very poſſibly happen, that farther obſervations may reduce them to ſome theory, whence practical inferences may be deduced.

AND you will the more eaſily believe, that the mechanical applications of centrobarical notions may be of immediate uſe, if we conſider, that by virtue of them, divers writers, and others of unſuſpected credit, aſſure us, that they have made a kind of lamp ſo poiſed, that one may roll it up and down like a bowl, without overturning the veſſel that contains the oil, or extinguiſhing the flame.

FROM the knowledge, that compreſſed air has a ſpring, whereby it reſiſts farther compreſſion, and a ſlight contrivance to make uſe of this pneumatical principle, an acquaintance of mine made a ſlight engine, which afterwards I found mentioned in a printed book, by which he was a great gainer, going, when he was well ſatisfied for his pains and hazard, to the bottom of the ſea, and by the help of this engine ſtaying there ſometimes for divers hours, till he had fetched up valuable things out of ſunk ſhips, and tied cables about their guns, that they might afterwards be buoyed up.

BUT there might be given ſo many examples of inſtruments and tools, that are uſeful to the naturaliſt, and for which, yet, he ought to thank the mechanicks, that it were tedious to enumerate them, eſpecially ſince the ſhops of mathematical inſtrument-makers, and other tradeſmen, may ſupply you with enough of them, to verify what this paragraph would perſuade.

VI. I ſhall conclude the conſiderations I deſigned for this eſſay by this, that as the knowledge of the theorems of mechanicks, and the practices, which have been thence derived, may very much aſſiſt the naturaliſt to make good mechanical contrivances, according to the exigences of his ſeveral purpoſes; ſo one good mechanical contrivance may be equivalent to, and may perhaps actually produce many good experiments.

THE former part of this propoſition will not, I think, require much proof. For a man muſt be but a dull naturaliſt, that ſhall know the properties of the center of gravity, of leavers, ballances, ſcrews, wedges, and other inſtruments for increaſing force, and by frequenting the ſhops and work-houſes of mechanitians, ſhall have ſeen variety of engines and inſtruments to compaſs different things, if he do not, from the ſurvey and conſideration of all theſe, grow more able, by compounding, varying, and otherwiſe improving them, to deviſe ſuch means and expedients, as he would not elſe have thought on, to make ſome trials, that he could not make before, and to make others more accurately, or more eaſily, or ſome way or other better.

AND as to the ſecond part of our propoſition, namely, that one good mechanical contrivance may be as conſiderable as many particular experiments, by enabling the naturaliſt to produce either numerous, or noble ones, or both, it may be manifeſted by ſeveral examples.

AND I ſhall begin with ſo familiar a one, as that afforded by valves, or trap-doors. For as ſlight and obvious as the invention of them ſeems, yet not only we owe to them a great variety of pumps and bellows for oeconomical uſes, but they make very conſiderable parts of ſeveral other engines, and may, as ſome trials have informed us, be applied about ſeveral new experiments, eſpecially if they be made of braſs, and yet ſo ſmall, that like ſome of thoſe I have had made by ſkilful workmen, (who, when I firſt directed them, told me, that they could not be made,) they may be uſed, not only in ſmall glaſs-pipes, but in ſyringes themſelves.

* Mag. Nat. Lib. VII. cap. 7.

By the help of small valves, and the knowledge of the spring of compressed air, have been made those wind-guns, which may be employed, not only to weigh the air, (whose weight we found them to evince, but not determine,) but to kill deer, and other game, without making a great noise, that would fright away the rest.

If I did not, *Pyrophilus*, foresee, that in the following essays of this treatise, I shall have occasion to mention some other instances of the service, that mathematical and mechanical disciplines may do the naturalist, I should here add divers particulars, which I had rather you should, when you meet with them, refer hither; and therefore I shall conclude what I intended now to say about these disciplines, by two or three short instances, that relate to what I have already said concerning them.

The first is, that it was not my design to treat of the utility of the mathematicks and mechanicks in an absolute way: for then I must have said much to their advantage, which I have omitted, because it would have too much swelled these essays, and not have been pertinent enough to them. And therefore I thought it sufficient for me to touch upon those things, on whose account these disciplines may be made useful to the naturalist, by assisting him either to frame theories, or to make observations and experiments, some (at least) of which, directly, or in their applications, either are already, or are like to prove, practical and useful. And it seems to me very probable, that the notions and practices of these disciplines, that have been too much hitherto restrained by meer mathematicians and mechanicians to the stars, the earth, the water, and some few other conspicuous parts of nature, may be very well extended, by a philosopher, to sundry other productions, as well of nature, as of art. As *Archimedes* deduced hydrostaticks from the application he made of vulgar staticks to bodies weighed in air and water, or in water only: and the ingenious *Torricellius*, and others, have of late applied the principles of hydrostaticks to that ponderous body (which the chemists reckon among metals) mercury.

My next advertisement is, that mentioning mechanical instances, not so much to acquaint you fully with the things themselves, as to make the mediums to infer what I would prove, I have taken the mechanical propositions, that I imployed, as they are delivered by the artists themselves, without warranting, that their proportions will hold true in mathematical strictness. For though I have made trials myself of several things of this nature, yet having often observed, how difficult it is to find a mathematical preciseness in physical and mechanical things, I think it not amiss to intimate thus much to you, though I may elsewhere have a fitter opportunity to make it out, that so great an exactness is in many cases not necessary to make the rules, that want it, useful in practice.

The concluding intimation I mean to give you, is, that I have not hitherto mentioned a service, that mathematicks and mechanicks may often do the naturalist, which is not fit to be silently pretermitted; and it is, that by lineal schemes, pictures, and instruments, they may much assist the imagination to conceive many things, and thereby the understanding to judge of them, and deduce new contrivances from them.

That I do not groundlesly say this, you will grant, if you consider, how difficult (not to say impossible) it were to go through with a long geometrical demonstration, without the help of a visible scheme, to assist both the fancy and the memory; and how difficult it is to give beginners an idea of the grounds of cosmography and geography, without material schemes and globes, your own very recent experience, as well as that of others, will, I presume, inform you. As it also may, how useful, not to say how necessary, pictures, and in some cases, models, are wont to be, when engines, houses, ships, and other structures are to be judged of, that they may be approved, or improved: but I shall rather take notice, that not only mechanical, mathematical, and anatomical things, need schemes and pictures, to represent them clearly to our conceptions; but many things, that are looked upon as more purely physical, may, in my opinion, be much illustrated the same way. Of which, if *Des Cartes* has, as some say, been the intruder, I think he deserves our thanks for it. For as *Plato* said, God does always geometrize; so in many cases it may be as truly said, that nature does play the mechanician, not only in animals, but in plants and their parts, and divers other bodies; in the explication of which curious, and oftentimes invisible contrivances of her's, pictures, that represent them well to the eye, and, if it were needful, in dimensions much greater than natural, may very much further the framing of right ideas of them in the mind.

THAT THE

GOODS of MANKIND

May be much increafed by the

NATURALIST'S INSIGHT

INTO

TRADES.

TO make out what is propofed in the title of this difcourfe, I fhall endeavour to fhew two things. The one; that an infight into trades may improve the naturalift's knowledge. And the other; that the naturalift, as well by the fkill thus obtained, as by the other parts of his knowledge, may be enabled to improve trades.

SECTION I.

AND firft, it feems to me to be none of the leaft prejudices, that either the haughtinefs and negligence, which moft men naturally prone to, or, that wherewith they are have been infected by the fuperciliousnefs and lazinefs, too frequent in fchools, have done to the progrefs of natural philofophy, and the true intereft of mankind, that learned and ingenious men have been kept fuch ftrangers to the fhops and practifes of tradefmen. For there are divers confiderations, that perfuade me, that an infpection into thefe may not a little conduce, both to the increafe of the naturalift's knowledge, and to the melioration of thofe mechanical arts.

I. AND I confider, in the firft place, that the phænomena afforded by trades, are (moft of them) a part of the hiftory of nature, and therefore may both challenge the naturalift's curiofity, and add to his knowledge. Nor will it fuffice to juftify learned men in the neglect and contempt of this part of natural hiftory, that the men, from whom it muft be learned, are illiterate mechanicks, and the things, that are exhibited, are works of art, and not of nature. For the firft part of the apology is indeed childifh, and too unworthy of a philofopher, to be worthy of a folemn anfwer. And as for the later part, I defire, that you would confider, what we elfewhere exprefly difcourfe againft the unreafonable difference, that the generality of learned men have feemed to fancy betwixt all natural things and factitious ones. For befides, that many of thofe productions, that are called artificial, do differ from thofe, that are confeffedly natural, not in effence, but in efficients; there are very many things made by tradefmen, wherein nature appears manifeftly to do the main parts of the work: as in malting, brewing, baking, making of raifons, currans, and other dried fruits; as alfo hydromel, vinegar, lime, &c. and the tradefman does but bring vifible bodies together after a grofs manner, and then leaves them to act one upon another, according to their refpective natures; as in making of green, or courfe glafs, the artificer puts together fand and afhes, and the colliquation and union is performed by the action of the fire upon each body, and by as natural a way, as the fame fire, when it refolves wood into afhes, and fmoak unites volatile falt, oil, earth and phlegm into foot; and fcarce any man will think, that when a pear is grafted upon a white thorn, the fruit it bears is not a natural one, though it be produced by a coalition of two bodies of diftant natures, put together by the induftry of man, and would not have been produced without the manual and artificial operation of the gardener.

II. BUT many of the phænomena of trades are not only parts of the hiftory of nature, but fome of them may be reckoned among its more noble and ufeful parts. For they fhew us nature in motion, and that too, when fhe is (as it were) put out of her courfe, by the ftrength or fkill of man, which I have formerly noted to be the moft inftructive condition, wherein we can behold her. And as it is manifeft, that thefe obfervations tend directly to practice, fo, if I miftake not, they may afford a great deal of light to divers theories, efpecially by affording inftances, wherein we fee by what means things may be affected by art, and confequently by nature, that work mechanically.

III. THE phænomena afforded by trades are therefore the fitter to be tranflated into the hiftory of nature by philofophers, becaufe they, whofe profeffion it is to manage thofe things, being generally but fhop-keepers, and their fervants being for the moft part but apprentices and boys, they neither of them know themfelves how to defcribe in writing their own practices, and record the accidents they meet with: fo that either learned men muft obferve and regifter thefe things, or we muft, to the no fmall prejudice of philofophy, fuffer the hiftory of nature to want fo confiderable an acceffion, accession, as the shops and workhouses of crafts-men might afford it; which accession would be much the more copious, if the experiment of trades were made by a naturalist, who would doubtless so manage them, as to make them far more instructive, and better fitted for the design of a natural history, than the same experiment would be, if they were related but by an illiterate tradesman, though never so honest.

And, *Pyrophilus*, to invite you, as you design a further progress in natural philosophy, to disdain, as little as I do, to converse with tradesmen in their work-houses and shops; give me leave to tell you, that as he deserves not the knowledge of nature, that scorns to converse even with mean persons, that have the opportunity to be very conversant with her; so oftentimes from those, that have neither fine language nor fine cloaths to amuse him with, the naturalist may obtain informations, that may be very useful to his design, and that upon several scores.

For first, tradesmen are usually more diligent about the particular things they handle, than other experimenters are wont to be; because these, if they want diligence, lose nothing, but what that very want of it keeps them from taking notice of, or at most, the satisfaction of an unnecessary curiosity; whereas tradesmen have anotherguise concern in the management of what they employ themselves about, for their livelihood depends upon it. And as, if they be careless, others more diligent will get away their custom; so, if they do any thing extraordinary well, the chiefest, and, for some time, the whole benefit will accrue to themselves, and by improving their profession they better their income.

Secondly, As it is proverbially said, that necessity is the mother of inventions, so experience daily shews, that the want of subsistence, or of tools and accommodations, makes crafts-men very industrious and inventive, and puts them upon employing such things to serve their present turns, as nothing but necessity would have made even a knowing man to have thought on. By which means, they discover new uses and applications of things, and consequently new attributes of them; which are not wont to be taken notice of by others, and some of which, I confess, I have not looked upon without wonder.

Thirdly, I have several times observed trades deal with things unknown to classical writers, and unused, save in their shops. And these are not only factitious, but divers of them natural; as manganese (by some called magnesca;) and zafora (if at least it be what many repute it) emery, tripoli, &c. and of both sorts there are some, that are exceeding useful; as of those formerly mentioned, the two first are to glass-men and potters; and the two later to a number of other tradesmen; and as among artificial concretes, soaders are of necessary use to gold-smiths, lock-smiths, copper-smiths, brasiers, pewterers, tin-men, glasiers, &c. amels to gold-smiths, glass-men, &c. lakes of several sorts to painters, heralds, &c. and putty to amel founders, potters, stone-cutters, gold-smiths, glass-grinders, and divers other professions. I shall add, that even of those natural things, of which some mention is made in famous books, one may learn many things in shops, not to be met with there, both as to the differing kinds of things, and as to the marks of their goodness, and as to other particulars conducive to the knowledge of those subjects. And I freely confess to you, *Pyrophilus*, that I learned more of the kinds, distinctions, properties, and consequently of the nature of stones, by conversing with two or three masons, and stone-cutters, than ever I did from *Pliny* or *Aristotle*, and his commentators.

Fourthly, You shall often find, that tradesmen, being unacquainted with books, and with the theories and opinions of the schools, examine the goodness and other qualities of the things they deal with, by mechanical ways, which their own sagacity or casual experiments made them light upon. And though these, having little or no affinity with those, that a book-man would have taught them, will appear to him extravagant; yet being such, as, if they really serve the craftman's turn, must be true and useful, their being extravagant will but make them the more new and instructive, and consequently the more fit to be admitted into the history of nature.

Fifthly, The observations, that tradesmen can supply us with, though they are not probably at any one time so accurately made by them, as they would be by a learned man; yet that defect is recompensed by their being more frequently repeated, and more assiduously made, than most of the experiments, wherein men of letters have furnished natural history: so that those circumstances, which are not heeded by the artificer at one time, may obtrude upon his observation at another, and, by reiterating the same processes so often, it can scarce be doubted, but that divers phænomena will offer themselves, even to an unattentive eye, that would not have been all of them taken notice of by a more heedful experimenter, that had performed the operation but once or twice. But this will be further confirmed in the next paragraph.

Sixthly, There are tradesmen, that do often observe in the things, they deal about, divers circumstances unobserved by others, both relating to the nature of the things they manage, and to the operations performable upon them.

Of the particulars, wherein the observations of tradesmen (for the utility of many of their practises is not questioned) may help us to investigate the nature of bodies, I could name more than my present haste allows me to mention; and I shall, as a specimen, take a little notice, first, of some of the remarks they have to distinguish and estimate what they call the goodness and badness of the things they deal with; and then of some few of their observations, that depend upon the influence, that time and season have on the things they handle, and upon the artificers operations on them.

For,

For, to begin with the first, although they commonly mean by such terms (of goodness and badness) no more, than the fitness, or unfitness of such things to yeild a good price, and in order thereunto for the purposes they are to be employed about in their particular trades; yet this fitness or unfitness is wont to consist in, or to suppose, qualities, that may relate to divers other things, and be applied to many other purposes. For some of the tradesmen's criteria discover to us a variety and a difference of kinds in bodies of the same denomination; as from the potters, the tobacco-pipe-makers, and the glass-men, we may learn a considerable variety of clays; and from stone-cutters and masons no less variety of stones untaken notice of by classick authors. So from carpenters, joiners, and turners we may learn, that some woods, as oak, are fit to endure both wet and dry weather; others will endure well within doors, but not exposed to the weather; others will hold out well above ground, but not under water; and others on the contrary will last better under water, than in the air.

AND as the distinguishing marks we were speaking of may inform us of the differences and kinds of bodies; so they may likewise on other accounts give us notice of divers of their qualities. Thus we find by the glass-men and soap-boilers, that some ashes, as those of kaly, bean-stalks, &c. do much more abound in salt, than other some; and yet some of those sorts of ashes make clearer, or otherwise better glass, than the rest do. We may likewise learn of the malsters the differing impressions, that the barley receives according to the fewel, whether straw, wood, furs, &c. that makes the fire, wherewith it is dried. And I remember, I have known an ingenious malster much advantaged by a way he had of so preparing malt, as if it had not been dried with wood, (usually the cheapest, but not the best, fewel for that purpose) whereas indeed it was a secret consisting only in the choice and seasoning of such a kind of wood, that even the solid parts of it cleft burnt almost like straw with a clear flame, so strangely free from smoke, that I could not behold it without some wonder.

THE other sort of instructive observations to be learned of tradesmen consists of those, that are made about the operation, that continuance of time, or change of season and weather, may have upon certain bodies, and ways of handling them. For naturalists, usually contenting themselves to make their experiments but once or twice, when their leisure best serves, or their occasions most require, have not the same opportunity to discern, what influence the temper, which the air then is put into, either by the season, or the weather, or both, may have on the event of the trial; whereas tradesmen, by long, and sometimes unwelcome experience, are taught such and such things will be best done at such seasons of the year, or in such kind of weather; which if they be not in some cases observed, either the thing will not succeed, or the tradesman will be damnified by his trial.

THUS we see, that tanners make choice of that part of the spring, when the bark abounds with the rising sap, to take it off from the trees; because at all seasons it will not be so good nor come off so easily. Thus joyners think not wainscoat sufficiently seasoned, till it be so many years old. And in several countries, butchers observe, that though a young bullock may be very good meat, if spent soon after it is killed; yet if powdered, to be long kept, before the beast be four or five years old, the salt will too much fret it, and make it little worth. And I look upon it as one of the advantages the naturalist may derive from tradesmen's observations, that the same things being successively dealt with by the father and the son, the master and the apprentice, they sometimes make far more long winded observations, than the philosopher has opportunity to do. As for instance, those, that make mortars of lignum vitæ, and will make them good, will keep it in the house twenty years, or perhaps more, to season, as they call it, before they will employ it. And experienced masons tell us, and as far as I have observed truly enough, that as there are some sorts of lime and stone, that will decay in few years; so there are others, that will not attain their full hardness in thirty or forty, or a much longer time. Of which I may elsewhere give you some instances.

To the six foregoing particulars one more may be added to the same purpose with the rest, and it is; that by frequenting the work-houses and shops of crafts-men, a naturalist may often learn other things, besides the truth and falsity of what they relate, concerning the history of the arts they make profession of. For though a tradesman, being for the most unlearned, and aiming only at making or performing those particular things, which, when done, are to bring profit, usually overlook those phænomena, that make not to his purpose; yet nature, (who minds as little his design, as he does those works of her's, that conduce not to it) is by some agents and operations, that he employs to compass his ends, engaged to do several things, that have a connection with those the artificer prosecutes, or else do depend upon them: so that the naturalist may oftentimes observe in shops divers considerable phænomena, that the tradesman regards not; because they neither further, nor hinder him in his work, and will be looked upon by him as impertinent to the history of his profession, in case he should be put upon delivering it. And yet some of these occurring phænomena being produced by nature, when she is as it were vexed by art, and roughly handled by ways unusual, and sometimes extravagant enough, may discover to a heedful and rational man divers luciferous things not to be met with in books, or probably not so much as dreamed of by the authors of them. Sundry examples of this I shall have occasion to disperse in the following Essay, and other tracts, that are designed you in this second volume of our present treatise.

SECTION II.

I WILL now therefore proceed to shew, that as the naturalist may, as we have seen, derive much knowledge from an inspection into trades; so by virtue of the knowledge thus acquired, as well as by that, which he has upon other accounts, he may be as able to contribute to the improvement of trades.

THIS he may do by several ways, and especially by these three. The first, by increasing the number of trades, by the addition of new ones. The second by uniting the observations and practices of differing trades into one body of collections. And the third, by suggesting improvements in some kind or other of the particular trades.

THE first of these I shall here lightly pass over, having elsewhere occasion to discourse of it more fully; only I shall here take notice, that, for the experimental philosopher to increase the number of trades now in use among us, it will not be absolutely necessary, that he should invent new ones, since he may do it by reviving the trades formerly known to the antients, but lost to us; such as the making incombustible cloth of lapis amiantus, the Tyrian purple, the making of Mosaick work, and those many other inventions, which you may find mentioned in *Pancirollus*, and his learned commentator *Salmuth*. Of which it were not amiss, that a catalogue were made publick; for such things, having been once actually done by men, are not impossible to be done again; and therefore I see no reason to despair, that in so ingenious an age as this, some, if not most, of them may be retrieved.

THE second advantage, that trades may derive from an inquisitive naturalist, is; that by this means the several observations and different practices of trades, whose managers want the curiosity, the skill, or the opportunity, to make a general inspection into trades, which they would find the more difficult to do, because crafts-men will often be more shy of one another, and more backward to disclose the mysteries of their art to one, that may make a gain of it, and thereby lessen theirs, than to a philosopher, that inquires to satisfy his curiosity, or enable himself to be helpful to them. And certainly, if so much as the known hints, that may be given by the experiments already dispersed among men of several professions, were known to any one man, though otherways but of common abilities; as my own experience has in some measure informed me; those united beams, which scattered are scarce considerable, would afford him light enough to better most of the particular trades, that are retainers to philosophy. And perhaps, it were not amiss, if there were some knowing and experimental persons appointed by the publick to take an exact survey of the trades in use amongst us, and inform themselves particularly of all the secrets and practices belonging to them, that thus discerning the errors and deficiencies of each, they may rectify the one, and supply the other, partly by the hints afforded by the analogous experiments of some other trades, and partly by their own notions and trials.

THUS a few of the more ingenious French gardeners have of late usefully applied to the watering of young and tender plants that way of filtration, which is used by apothecaries with moistened cotton wicks or rolls, or else with lists of either linen or woollen cloth, so ordered, that one end being immersed in the liquor to be strained, the other may hang over the brim, and out of the vessel somewhat lower than the bottom, or at least the surface, of the liquor. For if this lower end of the list be placed over the root of any seed or tender plant, it will, by constantly and leisurely dropping on it, water it much more temperately and uniformly, than can be done by common watering pots. And even this way of irrigation may by a cheap and easy mechanical contrivance be very much improved. There is another practice among stone-cutters, that cast or mold things with plaister of paris, to obtain finer powders, than searces are wont to give them, by stirring the powder well in water, and after it has rested a little while, pouring off the upper part of the troubled liquor into another clean vessel; at the bottom of which there will in time settle an impalpable powder. I will not here tell you what use I make of this in chemistry, to obtain much finer powders, than are usually to be met with of the same denomination. And I shall but intimate to you, that by letting the first water stand but so much the longer before you pour off the upper part of it, till not only the grosser and heavier, but the less fine particles be subsided, you may get a powder, yet much more subtle, than those artificers, that imploy the former way, without this circumstance, are wont to obtain. This, I say, it shall suffice me to have pointed at, because it is more proper to take notice, that the way of obtaining subtle powders by the help of water is useful, not only to the above-mentioned craftsmen, but likewise to glass-men, potters, makers of telescopes, and microscopes, those that cast metals in spaud, and other tradesmen too. Besides, that I may hereafter have occasion to tell you, that it is of great use in *China* for the makers of porcelain.

BUT it is not only by acquainting artificers of different professions with one another's practices, that the naturalist may further trades, but by making materials employed by one sort of craftsmen serviceable to another. That philosopher, who has surveyed a great number of trades, and compared them together, may do this with advantage, you will easily grant, when I shall have advertised you, that without any such assistance as that of a philosopher, in whom their distinct knowledge may concenter, and who has skill to enlarge the applications of them, we may observe, that sometimes tradesmen themselves can make use of one anothers productions. Of which I shall give you a couple of examples, the one furnished me by lytharge, the other by aquafortis.

THE former of these, which is but lead powdered and almost vitrified, by being blown off (or melted into) the refiner's test, as it serves the chemist to take his sugar of lead (which it has been observed to do better, than minium) and other saturnine medicines; so it serves divers comb-makers to die horns (as we have tried by the mixture of lytharge, quick-lime, and sharp vinegar. It serves also some painters and others to accelerate the preparations of their fat oils, as they call them. And some varnishers to make their varnishes dry quickly. It likewise serves some artists to make counterfeit gems; and we have tried, that by melting it with about a third part of pure white sand, or calcined crystals, and then putting in a small quantity of mineral concretes, according to the colour intended to be introduced, one may make sapphires, emeralds, &c. coloured like the natural ones; though this way makes these productions too ponderous, soft, and dim, and is far inferior to another we may elsewhere have occasion to disclose.

OTHER mechanical uses of lytharge I omit, to come to the second instance I was mentioning, which is taken from aqua fortis. For not only refiners use it to part silver from gold and copper (whence the French call it *Eau de depart)* but divers makers of curious wooden works use it for the discolouring and staining of their woods. Dyers make great use of it about their colours, and even about scarlet itself. Other artificers employ it to colour bone or ivory, steeped for a convenient space of time therein, having first made it of the colour they desire, by dissolving in it copper (instead of which I have sometimes used verdigrease) or other bodies, fit for their present turn; and some too by dissolving in it the fourth part of its weight of sal armoniac, turn it into aqua regia, and in that make a solution of gold, wherewith may be stained (as we have tried and taught some artificers) the ivory hafts of knives, and boxes of the same matter, with a fine kind of purple colour, which yet will not suddenly disclose it self on them. Some book-binders also employ aspersions of aqua fortis to stain the leather, that makes those fine covers of books that, for their resemblance to speckled marble, are wont to be called marbled. It is also employed (as themselves have acknowledged to me) by some of the diamond cutters, to free the dust of diamonds from metalline powders, as I shall hereafter declare. It is likewise of great (and as they imagine of necessary) use to those, that etch plates of copper or brass. To which may be added, that we have caused canes to be stained into the likeness almost of tortois-shell by a mixture of aqua fortis, not too well rectified, which is unexpedient in this work, and oil of vitriol laid on at several times and places, upon canes held over a large chafing-dish of coals, that by the heat the staining liquor may be the better sucked in by the canes, which must afterwards have a gloss given them, by being diligently rubbed with a little soft wax and a dry cloth. Nor are these all the uses made of aqua fortis, as you will find hereafter by instances, that I reserve for other places. But I thought fit to mention this liquor in this place, rather than any of those many factitious bodies I might have taken notice of, for these two particular reasons. The one, that the uses hitherto enumerated of this menstruum, may serve to confirm what I told you in the second essay, of the great utility of menstruums. And the other, that though aqua fortis be a liquor of exceeding common use, and wont to be distilled by men of several professions, as chemists, refiners, gold-smiths, &c. yet they have had hitherto so little curiosity to enquire into the nature of it, or vary the ways of making it, that not only the ways, that a skilful naturalist might direct for improving it, have not been taken notice of; but no small oversights may be observed to be generally and daily made about it. And an ingenious gentleman of my acquaintance, by making some trials to improve it, has been so far successful in his attempts, that he makes it by great odds better, than that which the refiners are wont to employ, or as far as my trials have informed me, than any I have used; and affords it for not much above half the price, that is commonly given for it. Nor have his experiments this way alone promoted the refiner's trade, but have also disclosed to him a way of clearly recovering most of his aqua fortis, after he has used it in the separation of metals, not only in its former strength, but somewhat encreased in virtue; which you will the more easily think possible, if I tell you, that aqua fortis may be made and received in other vessels, than those that are usual. As also, that without dreaming of this chemist's way, I have re-obtained that menstruum exceeding strong, after having employed it upon certain minerals, for from others I know not whether it may be so regained. And lastly, that there are some bodies, besides glass and earth, that are not brittle like these, and yet serve for the second distillation of aqua fortis, though made very strong at the first.

AND since I am mentioning of this liquor, I shall intimate (and only intimate here) that, by adding to salt-petre, instead of the usual addittament of three times its weight of brick, or clay, or the like, about an eighth or tenth part only of its weight of another substance, we have, even in ordinary sand furnaces, obtained, though slowly, a nitrous spirit, or aqua fortis much stronger at the first distillation, than that which is wont to be sold by our refiners, for double or rectified aqua fortis.

YOU, *Pyrophilus*, and divers other virtuosi, have much more opportunity to make an inspection into particular trades, than my other studies and occasions will allow me, and yet I have been more than once able to suggest to eminent artificers such things, concerning their own profession, as they tried and thanked me for. And therefore I have often wished, that some ingenious friends to experimental philosophy would take the pains to enquire into the mysteries, and other practices of trades, and gives us an account, some of one trade, and some of another, though the more are handled by the same person it will be, *cæteris paribus*, the better, not only delivering historically what is

practised,

practised, but also adding their own reflections, and any other thing they think fit to propose, towards the melioration of the professions they write of.

AND to give you, for a specimen of this (not perhaps the best that I could, but) such an one, as will be sure not to make you despair of out-doing it, I will add at the close of this essay, what came into my mind, and cost me about an hour to set down about the trade of those that sell varnished wares.

SOME Italian writers (who indeed are to be commended for it) have given us accounts of some particular professions, as besides others, that I have heard of, but could not procure, *Antonio Neri* has written *Dell' Arte Vetraria*, and *Benvenuto Cellini* of sculpture, and the statuaries art, and of some other professions, worthy, with the art of glass-making, to be made English.

AND indeed, I would willingly invite both you and other virtuosi of our own country, as well as of others, not to disdain to contribute their observations to the history of trades. And if you pitch upon any, you may command my thoughts of the method, wherein an account of it may be the most conveniently given. For I look upon a good history of trades, as one of the best means to give experimental learning both growth and fertility, and like to prove to natural philosophy what a rich compost is to trees, which it mightily helps, both to grow fair and strong, and to bear much fruit.

AND this I was so persuaded of, that I once designed, if the publick calamities of my country had not hindered, to bind several ingenious lads apprentices to several trades, that I might the better, by their means, both have such observations made, as I should direct, and receive the better historical accounts of their professions, when they should be masters of them.

III. BUT it is not only by making the practices and productions of some trades serviceable to others, that the experimental philosophy may be a benefactor to those professions. For he may do it by the third of the formerly mentioned ways (which in some cases is coincident with the second) namely, first by surveying the rules and observations already received, and the practices already in use of each particular trade he would improve, and then by taking notice of two things concerning it, viz. the deficiencies and inconveniencies, that blemish it, and the optatives, that may be made about it; that he may also in the last place propose rational (if not certain) methods or expedients to supply or remedy the first; and either accomplish the second, or make approximations to it, as far as it is feasable, or as his skill reaches.

BY deficiencies and inconveniencies, I do not here mean those things, which are wanting to the absolute perfection, which a philosoper might wish to find in the trade he considers; (for these belong to the optatives) but those, which are wont to be complained of, and not irremediable, or that are wanting to a more easily obtainable degree of perfection. I shall not pretend to enumerate these in particular trades, but only observe in general, that the chiefest of them seem to be such as these.

FIRST, that the artificer may be too much confined to certain materials, some of which may be scarce, or dear, or ill-conditioned, in comparison of others, that the naturalist might propose. As I remember, that being in a place, where we could not procure good vitriol to make aqua fortis with, after the manner of our English refiners, by a substitution of burnt allom for vitriol, but in a far less proportion, we made solvents for silver, as good as theirs, if not much better.

AND especially in such cases as these it is, that the naturalist may be very much assistant to tradesmen. For there are many things, which he, who is acquainted with variety of bodies, and the accounts on which they work on one another, will either quickly discern to be performable by other materials, than those, that tradesmen confine themselves to, or probably guessed to be performable by other agents more in the tradesmen power; and by making trials of his conjectures, it is like he will within a few trials discover what he seeks. I know an ingenious person, that upon the general complaint made by tanners, of the scarcity and dearness of the bark of oak, found a way to prepare leather without that or any other bark, as well, if not much better, than it is wont to be done the ordinary way, at least, as far as I, and divers other more skilful than I, could guess by some variety of it, which he shewed me. And this variety of materials, which may be suggested by the naturalist, is therefore the more considerable, because, that though the suggested materials be dearer, than that in common use, yet it may be so much better conditioned in other regards, as to be preferable to it. And though diamond dust be very many times dearer, than the powder of emery, yet I sometimes cause work to be done for me in a shop, where, to cut some gems, and even loadstones themselves, the craftsmen I made use of, did by my encouragement, employ the precious powder of diamonds, instead of that of emery, because the former makes so great a dispatch, and obliges them so much the seldomer to change their tools they apply it with, as makes an advantagious amends for the dearness. And so, though common spelter-soder be much cheaper, than that which is made with silver instead of spelter, yet in divers cases, this last is preferable, even by artificers themselves. For trial informs us, that this will run with so moderate a heat, as often needs not endanger the melting of thin and delicate pieces of work, that are to be sodered; and if this silver-soder be so well made, as some I can shew, you may with it, soder even upon soder itself, made the ordinary way, with brass and spelter, and so fill up those little holes or cranies, that may have been left or made in the first sodering, and are not safely to be mended, but by a soder more easily fusible than the first.

SECONDLY, that the tradesman may be confined to certain ways of working, when perhaps it would be much more advantageous to him, if he had others proposed him by the experimental philosopher, who may perhaps discern, that what is mechanically done by the artificer, may be better done physically, and on the contrary. Whereas goldsmiths, first directed probably by some chemist, by boiling silver-spurs, hilts, &c. of curious workmanship in salt, allom, and argol, give it that whiteness and clearness, which it would scarcely be securely brought to by brushing, or pumice-stone, or putty. And the like clearness, experience has informed us, that old sullied pieces of good gold may be brought to in a trice, by the help of warm aqua fortis. And as there are divers other things (some of which you will find mentioned in a following essay) that, though wont to be done mechanically, may be done better by physical means; so of those things, that ought to be done mechanically, many things, that are wont to be done by the labour of the hand, may with far more ease and expedition (the quantity considered) be performed by engines; by which, if they be skilfully devised, our observations make us bold to think, that many more of those, that are wont to require a laborious or skilful application of the hands, may be effected, than either shop-men or book-men seem to have imagined. For not to mention those several instruments, on which I have *extempore* played divers tunes, that I had never learned, when we see, that timber is sawed by wind-mills, and files cut by slight instruments, and even silk stockings woven by an engine, besides divers other artificial inventions left not named, because they cannot intelligibly be so in few words, we may be tempted to ask, what handy work it is, that mechanical contrivances may not enable men to perform by engines.

THIRDLY, there may be deficiences also in this, that what the artificer undertakes is either long in doing (as in the ordinary way of tanning, brick-making, seasoning of wood, &c.) or takes up more pains, or requires a greater apparatus of instruments, or else is some other way more chargeable, or troublesome, or laborious to be effected, than it needs be. And these kinds of deficiences may in very many cases be supplied by the experimental philosopher. As I know an inquisitive person, that has, upon a solemn trial, tanned as well as the masters of the profession, in far less time, (and if I much forget not, in less by above half) than they; so in some places they have a quick way of seasoning some kinds of wood, for the use of sea-timber, by baking it in ovens, (which way I have also known used here in *England*, to season some sorts of wood for other uses in a few hours;) so, whereas our grinders of dioptrical glasses have hitherto believed, that they must make use of Venice glass, which is very dear, and oftentimes very scarce to be come by, some virtuosi, considering, that the great clearness of an object-glass is rather an inconvenience, than a very desirable qualification, have newly taught some of the artificers to employ that coarser and cheaper sort of glass, they call green-glass, which is made here in *England*, instead of the other, which now begins to be thought by the skilful (with whom my observations disagree not) to be inferior to it. And several dyers employ our woad, which is not far fetched and much cheaper, instead of the eastern indigo, for dying of some, (if not all) sorts of blues, and those other colours, which that grand tincture prepares the cloth to receive.

FOURTHLY, another sort of deficiences or inconveniencies may be the want of durableness, either as to the very being of the thing produced by the artificer, or as to the beauty or the goodness of it.

OF the former sort may be (not to mention the decay and souring of cyder, perry, &c.) the cracking of glass of its own accord, and particularly that, which is complained of by divers, who deal in telescopes, that the object glasses, which are wont to be made, as I was saying, of fine Venice glass, will sometimes, especially in water, flaw of themselves, and so grow useless, to prevent which, some, that are very curious, carry them in their pockets.

OF the latter sort, is the fading of the bow-dye of water colours in limning, and the rust of shining arms, and other polished steel. Divers of these incoveniences also the naturalist may obviate or remedy; as some of the virtuosi above-mentioned, by teaching the glass-grinders to make the object glasses of their telescopes of green glass, have taught them a way to make them durable in spight of the vicissitudes of weather. And I have had pieces of artificial crystal, whereof some, though in no long time, cracked in so many places, that they changed their transparency for whiteness; yet another, though much larger, did, as I conjectured it would, hold sound during some winters, nor was ever broken but by accident: and I remember, I told the artificer, in whose furnace the crystal, that lasted not, had been made, that I took, as I do still, the reason of the difference to be, that the durable crystal had but a due, and the other an over great proportion of fixed salt. The reasons of which conjecture I shall have occasion to give you in another place.

AND, as to the scarlet dye (whereof I lately made mention) that it may be much advanced, as to point of fixedness and lastingness, beyond the common bowdye, I was persuaded by an honest merchant of *Amsterdam*, who had got a great estate by colouring of cloth, and was particularly curious about the scarlet dye. For he presented me with a piece of scarlet (of which he said he could make enough at a reasonable rate, wherein he almost defied me to find either any part undyed, or to stain it with vinegar, lixivium, and other liquors, that he named; and indeed by cutting it I found, that though it were a thick piece of cloth, the middle of it was not (as is usual in scarlets) white or pale, but it was dyed quite thorough; and though of scarlet I shall elsewhere have occasion to speak farther, yet I the rather mention it in this place; because it affords me a notable instance, that trades may be considerably improved by those, that do not profess them.

For the most famous *Cornelius Drebel*, who was the inventor of the true scarlet dye, was a mechanician, and a chemist, not a dyer; and as an ingenious man, that married his daughter, related to me, was so far from having been versed in that profession, when some merchants put him upon the advancement of certain way of dying a fine red, or rather crimson, that had been a while before casually lighted on in *Holland*, and proved very gainful to the finders, that he did not know so much as the common way of dying the ordinary reds, though the merchants having once taught him, that, by the help of a sagacious conjecture (to be told you in one of the following essays) he soon invented the true scarlet dye, which has since been so much esteemed.

IT now remains, that I mention in a few words the optatives, that may be proposed by the naturalist about the particular trades he would improve. By which name of optatives I mean all those perfections, that being desirable, are rather very difficult, than absolutely impossible, to be obtained. Of which optatives, there may sometimes belong several to one craft or profession.

OF this sort, in the black-smith's profession, may be the making iron to be fusible, with a gentle heat (as the flame of a candle) and yet hard enough for many ordinary uses. In the glass-men's trade, and the looking-glass-makers may be the making of glass malleable or flexible. In the clock-maker's trade, the making the newly devised pendulum clocks, useful in coaches, boats, ships, and in other cases where they are put into irregular motions.

IN the brasier and copper-smith's trade, the making of malleable soder. In the ship-wright's art, the making of boats and other vessels to go under water. In the diver's profession, some small and manageable instruments, to procure constantly, at the bottom of the sea, fresh air not only for respiration, as long as one pleases, but also for the burning of lights.

IN the assay-master's trade, the quick-melting down of ores, and cupelling of them, or at least of metals, in a trice, without bellows or furnace.

IN the carver's and joiner's trade, the way of giving a shape to wood in molds, as we do to plaster of Paris and burnt alabaster.

I know, *Pyrophilus*, that such optatives may be thought but a civil name for chimerical projects; but I shall hereafter more fully declare to you, why I think it not altogether unuseful, that such optatives should be proposed, provided, as I hinted above, that they be very difficult, and not impossible; that is, that they be such, as are not repugnant to the nature of the things, nor the general principles of reason and philosophy, and seem no otherwise to be chemically or mechanically impossible, than because we want tools, or other instruments and ways to perform some things necessary to the compassing of the proposed end, or to remove some difficulties, or remedy some inconveniences, that are incident to us in the prosecution of such difficult designs.

AND let me here tell you, *Pyrophilus*, that this advantage may be derived from the devising of such optatives to bold and sagacious men, that if they despair of attaining to the perfection they are invited to aim at, they may at least endeavour to reach some approximation to it. Thus unsuspected eye-witnesses have informed us, that in some countries, they are wont to shoe horses without the help of a forge bringing their iron to such a temper, that, having a company of shoes ready made, they can easily hammer them cold, so as to fit them to the size of any horse's foot, which the heat of the climate, where this is used, makes the greater conveniency. Nor do I much doubt, but, that by various tempers, iron may be made very soft and afterward hardened; and the rather because, as I elsewhere tell you, we have, without antimony or sulphur, melted it in a crucible, so as to pour it out like lead, and yet afterwards it grew harder, than it was at first. So, that flexible looking-glasses may be made with the help of selenitis, you will elsewhere be shewn; as also to foliate with ease all kinds of hollow glasses, and so turn them into specula. That malleable soder may be made, though we have not yet performed it, we do not much despair, and by good silver-soder some approximation to it has been already made.

SUBMARINE navigation, at least for a short space, has been successfully attempted by the excellent *Cornelius Drebel*, as *Mersennus* assures us; and as I have been informed, both by *Drebel*'s son-in-law, and by other judicious persons, that have had the account of trials from the very men, that went in the vessel under water for a good while together; who affirmed, that though there were many in the boat, yet they breathed very freely, and complained not of any inconvenience for want of fresh air. And here also give me leave to take notice, that this inventive *Drebel* was no professed ship-wright, nor so much as bred a seaman.

As for the optative proposed for the divers, I know one of them, who by a slight instrument, that is all under water, and has not, as others, any chimney open to the air above the surface of the water, has been able to stay divers hours at the bottom of the sea, and remove his respiratory engine (if I may so call it) with him; and *Mersennus* assures us, that a much better way, and in my opinion an admirable one, (if the thing be certain) was found out and practised in his Country, by one *Barieus*, who was able to stay six hours under water, by the help of an almost incredibly scant proportion of air, and even to preserve, at the bottom of the sea, the flame of a lamp or candle, in a vessel not much bigger, than an ordinary lanthorn.

As to the optative proposed in the assay-master's trade, I shall in the next essay teach you a way of cupelling in small quantities, without a furnace, or coals, or ordinary cupel, or other vessel.

AND I remember, that by way of approximation, I made a certain powder, with which, without a furnace, I have, in a trice, melted lead-

lead-oar (which very often holds silver) into metal, and perhaps consumed some of the baser metal too.

And lastly, as for the making of embossed works of wood in molds, I am credibly informed by a learned man, that it was actually performed lately at the *Hague* by the secretary of a foreign ambassador; but of the way I could not procure the least hint, though supposing the truth of the relation, I suspect it was done either by some menstruum, that much softened the wood, which may afterwards be easily hardened again, by which way tortoise-shell may be molded; or else, by reducing the wood into powder, and afterwards uniting the parts into one body with some very binding and thin kind of glue, whose superfluous parts may afterwards be pressed out. And I remember, I began (but was accidentally hindered to proceed) a trial to make an approximation to this, by the help of a rare glue, of which I had the hint, without being much beholding to him for it, from the practice of an ingenious tradesman, which as I now prepare it, is made by soaking the finest ichthyo-colla (i. e. izing-glass) for twenty-four, or at least for twelve hours, in spirit of wine (or even common brandy, for the menstruum need not be very good, unless for some particular uses.) When by this infusion the liquor has opened and softened the body (which will much swell) both the ingredients are very gently to be boiled together (and kept stirring, that the ichthyo-colla burn not, till all be reduced to a liquor, save perhaps some strings, that are not perchance very dissoluble) when it is boiled enough, a drop, suffered to cool, will soon turn to a very firm jelly, and whilst it is hot, it should be strained thorough a piece of clean linnen into a glass or other vessel, that may be kept well stopped; a gentle heat suffices to melt this glue into a transparent liquor with little or no colour, and yet this fine thin glue holds so strongly, and binds so very fast, that having sometimes taken two ordinary square trenchers (for the round ones are wont to be too thick) and laid the one a pretty way over the other, a little of this liquor put between them, and suffered to dry of it self, united the trenchers so fast, that when force was employed to break them, it did it elsewhere, not where they were joined together: so that it seems, the gluten, that fastened the trenchers together, was stronger than that, which joined the parts of the same trencher to one another. The other uses of this jelly (which by reason of the spirit of wine, will not easily corrupt like other jellies) belong not to this place. Only I shall add to our present purpose, that having taken some common saw-dust, and after having imbibed it with melted glue, strained out slightly what was superfluous, through a piece of linnen, and shaped the rest with my hand into a ball, this negligent trial (which was only made to see whether a more accurate might be hopeful) made the ball, after it had been leisurely dried, so hard, that being thrown several times against the floor, it rebounded up without breaking; but as I was saying, an accident hindered me from prosecuting the experiment, which therefore I recommend to you.

I will not now stay to tell you, *Pyrophilus*, how it may assist you toward the making such approximations, as we have been speaking of a little above, to take each of the difficulties, you would surmount, into the several parts it may be conceived to consist of, and make an enumeration of the possible ways of mastering each of these, according to some methods, that might be proposed; because to discourse of this subject would take up too much of the time alloted to the following essays, and therefore I shall conclude this, by observing to you, that as you are, I hope, satisfied, that experimental philosophy may not only it self be advanced by an inspection into trades, but may advance them too; so the happy influence it may have on them is none of the least ways, by which the naturalist may make it useful to promote the empire of man. For, that the due management of divers trades is manifestly of concern to the publick, may appear by those many of our English statute-laws yet in force, for the regulating of the trades of tanners, brick-burners, and divers other mechanical professions, in which the lawgivers have not scorned to descend to set down very particular rules and instructions.

APPENDIX.

I. HAVING in the foregoing essay mentioned a way of making spherical, and other hollow looking-glasses, with an intimation, that it should not be a secret to you, I shall no longer delay to acquaint you with it, partly, because, though it may seem but a curiosity, yet it may not prov euseless to you in making very easily, divers catoptrical experiments, that are otherwise difficult enough; and partly, because trial hath informed me, that some ways prescribed, of thus foliating glasses, were much inferior to what was pretended. And even in a recent and famous writer, I lately found a process of performing this, which when I had read over, I foretold it would not succeed, which prediction was soon verified by experience; and indeed they, that know the way and difficulty of foliating much more tractable glasses, than hollow ones, will scarce wonder, that it should not be found a very easy matter to foil, especially, without heat, spherical, cylindrical, and other concave glasses on the inside, to which the figure of the glass prohibits ordinary foils to be fastened: yet a mixture, that by the success appeared to be fit enough for such a purpose, I chanced to see employed by an illiterate wandering fellow I met with in the country, the consideration of whose practice did, I confeſs, ſuggeſt to me another mixture, that I afterwards ſeveral times tried, and found it to foliate not only ſpherical glaſſes (to which he confined himſelf) but other concave glaſſes, at leaſt as well as his, if not better, which he held for a great ſecret, and which indeed excelled any I have met with in print. To give you then the way I have practiſed my ſelf, take tin and lead, of each one part (by weight) melt them together, and forthwith add of a good tinned-glaſs (or biſmuth) two parts, carefully ſkim off the droſs, and afterwards, taking the crucible off the fire, before the mixture grow cold, put to it ten parts of clean quick-ſilver, and having ſtirred all well together, keep this foliating liquor in a clean new glaſs for uſe. When you would employ it, ſtrain it through a clean linnen cloth, to ſever it from droſs, and then by the hole of the ſpherical or cylindrical glaſs, put in a long and narrow funnel of paper, reaching almoſt to the bottom of the glaſs, that the falling liquor may not ſputter to the ſides. By this funnel you muſt ſoftly pour in ſome ounces of the mixture, and then dexterouſly and leiſurely inclining the glaſs every way, endeavour to make it faſten on all the inward cavity thereof: this being done for the firſt time, and the veſſel being laid aſide for ſome hours, that the foil may the better ſtick to it, it is beſt to take it in hand again, and after the former manner frequently, but ſlowly, paſs the liquor over thoſe parts of the glaſs, which by holding it againſt the light you ſhall diſcern not to have been ſufficiently foliated the firſt time; afterwards the glaſs being again laid aſide for ſome hours more, the former operation is to be reiterated once (or if it be needful twice) more, until you find the glaſs equally and ſufficiently foiled, which when you perceive it is, you may gently pour out the ſuperfluous liquor, to be reſerved for the ſame uſe in other glaſſes. Laſtly, with a cloth well ſprinkled with putty or ſcraped tripoli, or for need powdered chalk, the outſide of the glaſs muſt be carefully rubbed, to take off the foulneſs it may have contracted by being handled, and to make it look clean and poliſhed.

THIS way I have made uſe of in glaſſes of ſeveral ſizes, and figures, and preferred before that, which I remember, I once ſaw tried, and was aſcribed to a learned Italian, one *Caneparius*, as being more eaſy than it, and more ſafe in regard ours need no arſenick. I found it alſo much better than another, which is kept as a ſecret and highly eſteemed, becauſe though the the ingredients, abating the tin, be the ſame in both, yet in the way already delivered, the liquor or amalgam being uſed cold, there is no danger of breaking the glaſs to be poleated, or miſtaking the degree of heat to be given it, to both which inconveniences trial taught me, that the other way is obnoxious.

II. AND on this occaſion it will not be amiſs, to acquaint you, that I made this improvement of our way; that having made the outſide of glaſſes ſo foliated very clean, I have (by laying on very thinly ſuch a kind of varniſh, as that yellow one elſewhere deſcribed, as fit to make gilt-leathern hangings) made them appear richly gilt, and yet ſo bright and poliſhed, that they would, notwithſtanding this gilding, ſerve very well for looking glaſſes.

III. WHAT other improvements I made of this experiment, I muſt not here inſiſt on, eſpecially that I may comply with the haſte, which obliges me to omit, what I had thoughts of annexing here about varniſhes; ſo that though I have made many trials (whereof another time you may command an account) about ſeveral ſorts of them, ſome, that emulate gilding upon metals as well as leather, others, that imitate and diverſify (if not alſo excel) the China varniſh, and others deſigned for differing purpoſes, yet I can at preſent only tell you in general, that they are an uſeful, as well as ornamental, ſort of productions, and capable (if I miſtake not) of much improvement.

OF DOING BY

PHYSICAL KNOWLEDGE,

What is wont to require

MANUAL SKILL.

OR,

That the Knowledge of Peculiar Qualities, or Uſes of Phyſical Things, may enable a Man to perform thoſe Things Phyſically, that ſeem to require Tools and Dexterity of Hand, proper to Artificers.

THE particulars to be mentioned in this eighth eſſay might have been ranged partly under the preceding diſcourſe, and partly, under the eleventh eſſay, (which will be the laſt of this treatiſe,) whoſe titles are comprehenſive enough to take in the inſtances, that make up this preſent diſcourſe; which yet I have rather choſe to deliver apart, not only becauſe they ſeem ſomewhat differing from the examples alledged in the two mentioned eſſays, but chiefly becauſe the uſes, that may be made of ſuch inſtances, may make them deſerve a diſtinct and peculiar mention. For it is both a notable argument of the induſtry of mankind, and may prove a great encouragement to it, that the help of philoſophy may ſupply the office of manual dexterity, ſtrength, or art; and a knowing head may do what is thought not performable, but by a ſkilful hand, or an arm aſſiſted by ſome inſtrument or engine. And of theſe inſtances (which may be juſtly looked upon as ſo many trophies of human knowledge, and ſo many incitements to human induſtry) it will be needleſs to make any diviſion; and therefore I ſhall barely ſet them down as they come into my mind, no other order being neceſſary for particulars, that are brought but as proofs, and have not a dependency upon one another.

THE aſſertion, that makes the title of this diſcourſe, the king of *Spain* finds true ſo much to his advantage, that, if I miſtake not, it amounted, for a good while, to divers millions yearly. For whereas formerly in the ſilver-mines of *Potoſi* in *Peru*, (accounted the richeſt in the world) it was wont to be a very tedious laborious, and conſequently chargeable work, to ſever the ſilver particles of the ore from the ignobler parts of it, by many ſlow and coſtly, both manual and metalurgical fuſions, and other ways of ſegregation, much of that labour is now ſaved by *Pero Fernandes de Valcſco*, who, as *Acoſta* informs us, firſt made uſe of at *Potoſi* of the property of quick-ſilver to amalgamate with the nobler metals. For now, by accurately grinding the powdered and ſearſed ore with quick-ſilver (ſtrained through a cloath) and ſalt, and decocting them for five or ſix days, in pots and furnaces fitted for the purpoſe, the greedy mercury licks up the ſilver and gold (which it ſometimes meets with) without meddling with the ignobler parts of the ore; and being enriched with as much of them as it can imbibe, and diligently waſhed from the adhering ſordes, the amalgam is, by diſtillation with a ſtrong fire, freed from the mercury; which coming over revived into the receiver, leaves behind it the fixed metals, viz. gold and ſilver, which may be afterwards (if need be) eaſily reduced into bodies, and parted by the common way. And by a not unlike way ſome of our goldſmiths and refiners are wont (as themſelves inform me) to regain out of the duſt and ſweepings of their ſhops, the filings and other ſmall particles of gold and ſilver, which fall to the ground in their operations, and in proceſs of time may amount to a conſiderable value.

To make an head, exactly repreſenting the ſize, ſhape, and lineaments of the face of any living man, ſeems to require an exquiſite ſkill in the ſtatuary's art; and yet at my deſire, and in my preſence, that was lately performed by a tradeſman, after the following manner. The party, whoſe face was to be caſt off, was laid flat upon his back, having round about the edges of his forehead, his cheeks, and his chin, ſomething placed to hinder the liquid plaiſter from running over on his hair: then into each of his noſtrils was put a hollow piece of ſtiff paper, of about a quarter of a foot long, and of the figure of a ſugar-loaf, and open at both ends, that the affuſion of the plaiſter might not hinder him to take breath. And of theſe pipes, which were carefully oiled over, the acuminated extremes reſted upon his noſtrils, and

and the other were supported by one of the assistant's hands. Then his face being lightly oiled over, to hinder the plaister from sticking to it, with oil-olive, and his eyes being shut, alabaster newly calcined in a copper-kettle, till it was as white as before, was tempered up with fair water to the consistence of batter, and by spoonfuls nimbly put all over his face, till the matter lay every where near an inch thick. Almost as soon as it was all laid on, it began to grow sensibly hot, and in about a quarter of an hour hardened into a kind of lapideous concretion; which being gently and easily taken off, shewed us, in its concave surface, the exact impressions made there by the parts of the face, and even by the single hairs of the eye-brows. In this mould they cast a head of good clay, by working it in, and on that head they open the eyes, which in the prototype and mould were shut, and, if need be, heighten the forehead, and make what other amendments they think fit; and anointing this new face with oil, they after the former manner make a second mould (of two parts, contiguous all along the ridge of the nose) with calcined alabaster, and in this second mould (lightly oiled on the inside) they cast with the same matter the fore-part of an head, more like the original, than ever I saw made by the most skilful statuary, and yet with so much ease, that the very first trial I made my self to cast a face thus, succeeded.

To take the impression of a leaf, or other flattish part of a plant, it may seem requisite that a man be a good painter; and yet I found, that the thing may be performed, only by holding a whole leaf (or sprig of rosemary, &c.) in the smoke of a piece of common gum sandarack, rozin, camphire, or some such body, that emits a copious and fuliginous steam, (for which purpose I have made use of a common link, when that was most at hand:) for the leaf being well blacked by these fumes, and placed betwixt the leaves of a sheet of white paper, if you carefully press the paper upon the leaf with the haft of a knife, or some other smooth thing, you may thereby print on the paper in a few moments the exact size and figure, but not colour, of both sides, but especially the back-side, of the leaf, with the very ramifications of the fibres that are disseminated through it. And this may be performed, though not so lively, by blacking the plant, whose picture is required, with the fumes of a candle or taper, (especially if it be of wax) instead of those of the aforementioned resinous concretes, and afterwards proceeding as in the former experiment: which sometimes may be of good use to you, when you turn botanist, and in your travels meet with plants, whose pictures you think worth having, but have not time or conveniency to draw them.

ANOTHER instance, of the same import with the foregoing ones, may be afforded us by the art of etching, whereby copper and silver-plates may be enriched with figures, which may seem to have been made by the tool of some excellent graver; and yet those engravings do not require the presumed manual skill; and are made without such tools, by having a peculiar sort of varnish (for on the goodness of that, depends much of the success of the operation) on the plates, and drawing on it the figures to be engraved. For all those lines, where the plate is freed from the varnish, by skilfully tempered aqua-fortis (from whose corrosive violence the remaining varnish secures the rest of the plate) may be so curiously wrought on by those few artists, that are skilful in it, that I have very seldom seen lovelier cuts made by the help of the best tempered and best handled gravers, than I have seen made on plates etched, some by a French, and others by an English, artificer.

BUT the knowledge of the physical properties of things may sometimes enable a man to perform, not only things to which mechanical tools and manual dexterity seem to be necessary, but some things also, whereto even mathematical instruments, and skill in mathematicks, are thought requisite; of which I shall at present propose a couple of instances.

In the elsewhere mentioned French abridgement of *Galileo*'s Italian book *, I find a passage very pertinent to our present design, which agreeing very well with our observation of that kind, we shall propose it a little more clearly as follows.

SUPPOSE in a high church (the book exemplifies *Nostredame*) the great candlestick, that hangs from the top of the church being made to swing, a philosopher, that has observed, that the vibrations of a pendulum, though the arches it describes be unequal, are in the sense formerly declared equitemporaneous; and that, when the strings, at which such pendulums hang, are very unequal, their lengths will have the same proportion, as is between the squares of the numbers of their single vibrations performed in the same time: suppose, I say, that such a person have a pendulum with him, whose string (which may be of any length, so it be determinate) is, for example, a yard long, it will not be difficult for him, without any quadrant, or geometrical instrument, to find out the length of the string that supports the candlestick, and consequently the height of the church. For the candlestick and the short pendulum being made to swing, beginning both at the same time, let us suppose, that when the candlestick has made nine vibrations, the pendulum of a yard long has made 54, the squares of these two numbers will be 81 and 2916; and because, as we lately said, the length of the pendulums will have the same proportion with the squares of the number of their vibrations, dividing 2916 by 81, the product will be 36; which shews, that the string, at which the lamp hangs, is 36 times as long as that of the shorter pendulum, and consequently a yard, containing three feet, amounts to (36 yards or) 108 feet.

UPON the knowledge of another physical property of heavy bodies, I remember I have grounded a way to measure vast heights and depths without any geometrical instruments, and

* Nouvelles pensees de Galiléo liv. I.

and in such cases, where such an instrument cannot be imployed, by the help of a pendulum; which, because in this case it must be very short, will require an attentive and expert observer. For it being known, that a stone, or a piece of lead, or the like solid weight, falling from a height, does so accelerate its descent, that the differing spaces it has transmitted, at any differing times assigned, will have betwixt the same proportion with the squares of the times, wherein the respective spaces were transmitted; if it be once known by diligent observations, how far a stone, or such a solid body, (whose greater or lesser bulk is not here considerable) does fall at the end of the first second-minute of its motion downwards, it will be easy enough for a naturalist, versed in the doctrine of proportions, to collect from the time, that the stone imploys in descending perpendicularly from the top of a high tower or steeple, how high that building is. This way of measuring, provided attention and accuracy be not wanting, we found agreeable enough to divers observations of our own and our friends; and by this way, one may measure the depth of a well (to the surface of the water) how deep soever, though the bottom, as it is usual, by reason of the darkness, cannot be seen, which makes the depth unfit to be measured by quadrants, and such like geometrical instruments. For, if at the same time, that you let fall a stone, or other weight, you also let go a pendulum, that vibrates quarter-seconds, that is, makes two excursions, and as many returns in the sixtieth part of a minute, and reckon its vibrations, till you hear the noise made by the stone dashing against the water in the bottom of the well, you may easily enough collect the depth. For let it be supposed, that it be found by experience, that a falling stone, or other like weight, do in the first second-minute of its descent, dispatch (as the diligent *Mersennus* affirms himself to have often found) 12 feet, (which I understand of French, not having found it hold in English measure,) and let us also suppose the pendulum to have perfected six single vibrations before the dashing of the stone against the water was heard; if we proceed according to the rule formerly given, we shall find, that if the time, wherein the falling stone transmitted those spaces, that are to direct our calculation, be one and six, the square of those two numbers being 1 and 36, the stone must have fallen at the end of the sixth second 36 times as far as at the end of the first. And since by observation (about whose accurateness we need not be solicitous here, where we design only the giving an explanatory example) a falling stone in the first second descends 12 foot, we need but multiply 36 by 12, to obtain in the product 432, the perpendicular depth of the well to the surface of the water. And the same number may be collected, and perhaps you will think more easy, by supposing, as *Galileo*'s experiments seem to prove, that a falling body accelerates its descent according to a progression of odd numbers, beginning from an unit; so that, if in the first second-minute, or any other determinate part of time, it falls one space, whatever that be, in the next second it will fall three spaces, and in the third five spaces, and so onwards: according to which reckoning, if the falling body be supposed to descend 12 foot, during the first second, it will descend 36 (besides the former 12 in the next second,) in the third 60, in the fourth 84, in the fifth 108, in the sixth 132, which summed up together, amount to 432. And, by the same way, one may measure the height of divers precipices how great soever, as far as one can reach downward in a perpendicular line. And one may also give some guess at the depth of some volcanos, which are not accessible to those, that know but the common ways of mensuration, or which have burned the ropes, and even melted down the chains and weights, by which some curious persons have attempted to fathom their depth. It is true, that in mathematical rigour, some abatement ought to be made, because the stone strikes the surface of the water, or the bottom of the precipice, some little while before the sound, produced by that stroak, can arrive at our ears. But unless the height or depth to be measured be very extraordinary, this allowance, for the delay of the noise, either may be neglected without much inconvenience, or in probability, will scarce exceed a quarter (or at most half) of a second; since, as has been elsewhere noted, it has been found by observation, that a sound in the air moves above twelve or thirteen hundred foot in one second. And in what I have here delivered concerning the way of measuring depths and heights by the falling of a heavy body, I have been much confirmed by an observation I chanced to meet with in an outlandish book, which I have not now by me to look out the place, where the mathematician, that writes it, who seems to have been a diligent observer, affirms, that he found a weight let fall from the top of a church, or steeple (for I remember not which, nor is it material,) so high as to amount to 300 foot, to reach the ground in about five seconds; which agrees very well to what we have been delivering. For supposing the weight to fall 12 foot the first second, at the end of the fifth second it must have fallen 25 times as far, (1 and 25 being the squares of the numbers of the seconds of time,) and consequently 300 foot.

To slit (or divide transversly into flakes or leaves) so thin a piece of metal as an old groat, which seems not to exceed, if it so much as equal, the thickness of a leaf of white paper, may be thought, if it be feasable, to require some very subtle dividing instrument, with an edge finer than that of a razor; and yet the way of performing this by physical means is but an almost ludicrous experiment, which (if you know it not already) is easily thus made. Take three pins, and stick them in the form of a triangle, at such a distance from each other, that the groat may rest upon the heads of them: put upon this thin piece of metal almost as much flower of brimstone, or, at least,

least, finely powdered sulphur, as will conveniently lie on it; then kindling the sulphur, let it burn out of itself; which done, take off the groat, and throwing it hard against the floor, the upper part, with the adhering remains of the sulphur, will be parted from the lower: which (lower) if the coin were not very thin, will retain its former shape. I have observed in this experiment, a pretty circumstance or two, the knowledge of which is very apt to be misemployed, and need not here be mentioned: though I would not silently pass by the experiment itself, because as ludicrous as you may think it, it may suggest uncommon speculation to a considering naturalist, and also intimate a way of preparing silver, of which I may elsewhere tell you the practical use.

He that takes notice of so pretty a variety of colours and shapes, as may be discerned on a skilfully made sheet of marble-paper, will be apt to conclude, either, that the differing colours were laid on one by one with a pencil, which would require a great deal of time and pains; or that the sheet was marbled by being printed off from some plate, on which the differing shapes were cut or engraven, and the differing colours singly placed, which would require yet more labour, and a greater apparatus; whereas the whole sheet is painted thus variously and delightfully at once, and in a trice, by the contact of the surface of a vessel full of water, on which the colours (first blended a little, by a quick and easy motion of the artist's hand) are so ordered, as to swim without being confounded. This artifice hath, as I am informed, been delivered by the curious *Kircherus*. But if you have a mind to know the particulars of it more fully, you may command me to acquaint you with what I have learned from experience, by which the practice is supposed to have been of late improved.

If it were proposed to free weak spirit of wine, or aqua vitæ, from a great part of its phlegm, the generality of distillers would think it not to be effected, but by the help of fire and a furnace, an alimbeck, or some other distillatory vessels; and yet, without the help of any of all these instruments, I have sometimes taken pleasure to dephlegm brandy, (as they call weak spirits of wine of the first distillation,) only by putting it into salt of tartar. For considering the faculty this alkalizate body has to attract (as men commonly speak) or imbibe the aqueous particles, that swim in the air, and resolve itself, with them into that liquor, that the chemists call oil of tartar per deliquium, there seemed sufficient reason to expect, that the same salt being put very dry into phlegmatick spirit of wine, would embody with the phlegmatick parts, with which, if it were not overcharged, it would probably keep them separate from the more spirituous liquor; since such oil of tartar as I have just now mentioned, and dephlemged spirit of wine, will swim upon one another without mixing; and accordingly, I have sometimes taken pleasure, by putting a sufficient proportion of dry salt of tartar into brandy, and leaving it there for some time (for the experiment will, to be compleated, require some while) to make some separation of a great part of the phlegm, which by degrees dissolving the salt, will reduce again part of it into a liquor, that will keep its surface distinct from that of its supernatant spirit, and if confounded therewith by the shaking of the glass, would speedily part from it, and regain its own station; and if you would have a separation of the phlegm begin to appear quickly, you may compass what you intend, by tying up a convenient quantity of dry salt of tartar in a dry rag of linnen cloth, and immersing it a little while in the brandy, and then lifting it up a little above the liquor; for the phlegmatick parts being copiously imbibed in the salt, which will be thereby resolved into a ponderous liquor, will in drops (whose descent will be distinguishable enough, if the glass be held against the light) fall to the bottom of the spirit of wine. And lest you should suspect, that this descent comes not from their weight, but from the force they acquire in falling through the air, you may keep the rag immersed beneath the surface of the liquor, and yet may perceive the efflux and subsidence of the lixivium we have been speaking of.

There are some cases, wherein bodies, that are to be held very softly, are either so brittle, that it would be hard to hold them fast enough without danger of breaking them; or else so small, and so inconveniently shaped, that it would be very difficult to procure instruments to lay hold on them, and keep them moveless in the instrument: and in several such cases the use of tools, to hold fast such bodies, may be advantageously supplied by artificial cements. As I remember I have known the glass-grinders, instead of more mechanical tools, employ pitch, melted and made up with ashes, very well stirred and incorporated with it, into a stiff paste. For this mixture, being by a fit heat brought to a convenient softness, the glass to be ground or polished is bedded in it, in what posture, and as far as, the artificer pleaseth; and by the same mixture the glass being fastened, at the end of a stick or some proper instrument of wood, the glass, upon the cooling of the cement, remains firmly fastened, until the artificer have done with it what he designed; after which, by softning the cement with heat, he can readily take it off again.

And even the diamond cutters, who, to grind those stones into shapes, are wont to employ a very vehement attrition, make use, for holding their diamonds, especially when they would polish them, of a cement, the like to which I remember I have sometimes made to other purposes: for themselves have confessed to me, that they made theirs chiefly of rosin, melted and brought to a stiff paste, with fine brick-dust, to which one of the eminentest of them for skill adds a proportion of sealing-wax; (I told him I preferred plaister of Paris before brick-dust, and he told me he did the like.)

AND indeed by variety of cements we may be aſſiſted to make divers experiments, that we could not otherwiſe make ſo well, if at all; for which reaſon I have been ſomewhat curious about making a pretty numberof ſuch mixtures, whoſe compoſitions you may command of me.

THERE are divers artificers, eſpecially thoſe, that ſlit and poliſh cryſtal, agates, and other hard ſtones, and cut ſeals in gems, who have need of powders of emery, of differing degrees of fineneſs, and ſome of them extremely ſubtle: to obtain theſe, one would think it neceſſary to have variety of ſearces, and ſome of them as fine as it is poſſible. But the ſkilfulleſt artificers judge they can obtain their deſire much better by fair water, than by the beſt ſearces. For having in a mortar beaten the hard body of emery, as long as they think neceſſary, they put the powder into a pail, or other fit veſſel, full of water; and then with a ſtick, or ſome ſuch thing, they ſtir very well all, that is at the bottom, that it may be raiſed and thoroughly mingled with the liquor; then pouring it out into another veſſel, the groſſeſt and the moſt ponderous grains of the diſperſed powder will firſt fall to the bottom, and give a powder leſs groſs, than that, which remained in the firſt veſſel, which may be again beaten ſmall in the mortar. Afterwards they pour the troubled water of the ſecond veſſel into a third, and there ſuffer the duſt to ſubſide, and then decanting the liquor, if this duſt be not yet fine enough, they trouble the water again, and after a little while, pour it off either into one veſſel, or two, or more, ſucceſſively, according to the exigency of their uſes; and then ſuffering the tranſvaſated water to ſettle for ſome hours (more or fewer,) as the diſperſed duſt is more or leſs light, they decant the liquor, or ſuffer it to exhale, and take the remaining powders, of which that, which ſettles ſloweſt, will oftentimes be ſtrangely ſubtle. And by this way, if a man will have patience to pour ſucceſſively the troubled liquor into veſſels enough, and give the diſperſed powder a competent time to let fall the leſs light parts, before the upper part of the water be poured off into the veſſel it is finally to ſettle in; he may obtain ſeveral degrees of powders, leſs and leſs groſs, and ſome ſo fine, as one would admire how it was made ſo. And this (*Pyrophilus*) I the rather mention to you, becauſe it is not only from emery, but from divers other bodies, that one may obtain extremely minute, and (as they ſpeak) impalpable powders, of great uſe in ſome of the moſt curious trades, and perhaps in phyſick too. For I may elſewhere tell you, how I apply this way to magiſteries of cryſtal, and of gems, and even to *Crocus Martis*; the naming of which laſt puts me in mind to add, that a chemiſt, much prized for finer *Crocus Martis*, than others of his profeſſion, and thereby enabled to ſell it at an extraordinary rate, confeſſed to me, that it was to the artifice I had been commending, that the *Crocus* he ſold owed all its advantages.

IT has long been, and ſtill is, in many places, a matter of much trouble and expence, as well of time as money, to cut out of rocks of alabaſter and marble, great pieces, to be afterwards ſquared or cut into other ſhapes; but what by the help of divers tools and inſtruments cannot in ſome quarries be effected, without much time and toil, is in other places eaſily and readily perfomed, by making, with a fit inſtrument, a ſmall perforation into the rock, which may reach a pretty way into the body of it, and have ſuch a thickneſs of the rock over it, as is thought convenient to be blown up at one time; for at the farther end of this perforation (which tends upwards) there is placed a convenient quantity of gunpowder, and then all the reſt of the cavity being filled with ſtones and rubbiſh ſtrongly rammed in, (except a little place, that is left for a train,) the powder, by the help of that train being fired, and the impetuous flame being hindered from expanding itſelf downwards, by reaſon of the newly mentioned obſtacle, concurring with its own tending another way, diſplays its force againſt the upper parts of the rock, which, in making it ſelf a paſſage, it cracks into ſeveral parts, moſt of them not too unweildy to be manageable by the workmen.

AND by this way of blowing up rocks a little varied and improved, ſome ingenious acquaintances of ours, employed by the publick to make vaſt piles, have lately (as I received the account of themſelves) blown up or ſcattered, with a few barrels of powder, many hundred, not to ſay thouſand, tuns of common rock.

To give ſmall glaſſes the ſhape, that is requiſite to fit them to ſerve for covers to the dial-plates of watches, and for other purpoſes, to which artificers ſometimes employ them, one would think it neceſſary, that they ſhould be ground, or otherwiſe wrought with tools, by a ſkilful hand, to give the glaſſes the concave, as well as the convex figure they ought to have. And yet I have learned by trial, that a flat plate of glaſs of a competent thickneſs, that has its two ſurfaces ſmooth and parallel to each other, being carefully laid upon a deep ring of iron, or a ſhallow and hollow cylinder of the ſame metal, and of the diameter required, ſo that the edge of the glaſs (which is to be reduced to roundneſs) may every where reſt upon that of the cylindrical piece of metal; the heat of the fire warily and ſkilfully adminiſtred will ſo ſoften this plate of glaſs, that its own weight will ſo depreſs the middle parts, that the glaſs will thereby obtain the figure required. And though ſuch glaſſes do not conſtantly fall juſt into the deſired figure, yet when they are ſkilfully ordered, they fall into it ſo often, that I am told, that ſome ingenious artificers have quitted the ordinary way of making covers for watches, for that we have been deſcribing; which, though not free from caſualties, is yet ſo much more cheap and eaſy.

WE have in ſome parts of *England* various kinds of talk, or *lapis ſpecularis*, (ſeveral of which I have been poſſeſſor of,) and of ſome of them there is ſo great plenty, that one may procure good ſtore for little or no charge: but the reducing of a great lump of this talk to fine powder, if it muſt be done the common way

way, by beating it in mortars, and searcing it often, will require much time and pains; but as I have several times tried, the smaller pieces may, by the help of an actual flame, be quickly reduced to a snow-white calx; so by the experiment of a sagacious acquaintance of mine, even great lumps of it may, almost in a trice, be brought to fine powder, by heating them red-hot, and casting them, while they are so, into cold water, whereby there will presently be made a comminution of them into a fine, and as it were, mealy calx.

THE ground of this operation is much the same with that, whereby some chemists granulate masses of gold and silver, when they pour the strongly melted metal from a competent height into cold water, whereupon there happens a dissilition of the parts of the metal; many of which fall to the bottom in little fragments. But the more easily fusible metals, tin and lead, may be quickly and better granulated by the mechanical way, freshly mentioned, as to talk. I remember, I was wont (especially if the ignition and extinction were repeated two or three times) to reduce crystal flints, almost in a trice, to a fitness to be easily brought to a very subtle powder, proper to make amauses (or counterfeit gems) of.

THE mention I have already made in this essay, of what may be performed by the faculty, that burnt alabaster, made liquid with water, has to grow hard again, puts me in mind of another instance, very properly referrible to the subject of this essay. For one, that beholds how curiously oranges and lemons, and other fruits are counterfeited in wax, would imagine, that so lively a representation of them could not be effected, but by a hand, as skilful at least as that of a painter; since by this plastick art, not oranges, and lemons, &c. in general, but this or that particular orange or lemon may be most lively represented; and yet you may learn this art within one hour or two, the thing being performable easily and quickly: for having the orange, &c. we would imitate, we bury it half way in a coffin of clay, whose brim, together with the extant part of the fruit being oiled over, to keep the mixture from sticking, the tempered alabaster (or plaister of paris) is nimbly laid on to a good thickness, and, upon its concretion, removed, whereby you obtain an half-mould for that part of the orange; then the formerly latent part of the fruit being likewise placed uppermost in the half-mould, which should have some pretty deep notches cut in the rim of it, which, with the protuberant part of the fruit, ought to be oiled, the tempered mixture is likewise put upon that, and thereby an exact mold is compleated, at any convenient part of which a hole being made, to pour in a little tempered and coloured wax, when it is brought by fusion to a due heat, (for every degree of that quality is not convenient,) shaking the mold nimbly and every way, the wax comes to be so applied to the internal surface, that when the mold is cold, and the parts taken asunder, you have an orange of wax very lively representing the original.

THERE are some circumstances belonging to this easy and delightful art of molding and casting in wax, (which is pleasant enough to be practised even by ladies) that I purposely omit; what has been mentioned being sufficient to shew you as much as is necessary for my present purpose. And I the rather pitched on this experiment, because it may afford us another instance, not impertinent to the design of this tract. For one, that should see how great a cavity is left within the counterfeit orange, would think, that there were some great and rare artifice requisite to cast it thus hollow, and make so small a quantity of wax reach to the counterfeiting of such a fruit; whereas the bare shaking of the mold, when the melted wax is in it, together with the expansive endeavour of the included air, applies the wax to every part of the inside of the mold, and thereby turns it into one great film, which one would think it very difficult to separate, without injuring it, from the mold, to which it is applied so close: and indeed it might be so, if nature did not again assist the artist, by making the mixture, when it cools, shrink a little, and thereby part easily from the mold it stuck to.

BUT one of the prettiest and the strangest artifices, that belong to this essay, is that, whereby the knowledge of a few unheeded physical properties of two or three bodies may enable a man to perform that, which seems to require, not only good tools, and great dexterity in the art of graving, but likewise an exquisite skill in caligraphy, or the art of writing fair: for I know a graver, famous for skill in his profession, who writes, as I have had good opportunity to observe, but a bad hand; and yet this man with his tool writes rarely well, and will imitate and emulate the finest copies of the choicest writing-masters, so that even virtuosi have much admired how a man, with a stiff iron tool upon a tough copper-plate, can write incomparably fairer, than the same person can with a good pen upon paper. But to ease you somewhat of your wonder, I shall add, that though this artifice be kept for a choice secret, and though I could not learn a considerable particular or two, which belong to the delicacy of it; yet (partly by putting questions, and partly by some trials of my own) I attained to the substance of this mystery, as they call it, which seems to be this.

A writing-master, or some other, that writes a very fair hand, is desired to write a copy, or what else is to be engraven, with a peculiar kind of ink, which differs not in shew from common ink, being fully as black as it. Then they take a very clean and well-smoothed copper-plate, which being moderately warmed, is to be so rubbed over with a certain white varnish, or something equivalent (to be mentioned a little beneath,) that when the plate grows cold again, it may be thinly and evenly cast over with a kind of skin or film (if I may so call it) of varnish; then lightly moistening the paper, that it may part with its ink the more readily, the written side is to be laid on the prepared side of the plate, and that, together

ther with the paper being passed through a rolling-press, enough of the ink will stick (but in an inverted posture) to the varnish, whose whiteness renders the black letters very conspicuous; so that it is easy with a needle, fitted with a wooden handle, to draw over the very same lines and strokes through the yielding varnish upon the metalline plate, whence they may, after the plate is by heat, or otherwise, freed from the varnish, be compleated with a graver; and lastly, when the whole engraving is finished up, may be printed off in a rolling-press like ordinary cuts. And even without a rolling press I have sometimes taken off written characters, only by laying the moistened paper very smooth upon the varnished copper, and rubbing it hard thereon with a convex piece of glass, or some such smooth and hard body, whose pressure makes the ink stick to the varnish, for which I have used the purer sort of virgin wax, if the ink be good, and have been laid on plentifully enough by the pen. That ink, which I most used, I made only of fine Franckfort black, as the painters, that sell it, are wont to call it; by grinding it little by little, but very diligently, with water, till it had attained the consistence of a somewhat thick ink; in which this only circumstance is carefully to be observed, that no gum be added, as is usual in other inks, lest that hinder its coming off.

AND here it will not be impertinent to the argument in hand to add another artifice, whereby a printed cut may be so far taken off, that at least the out-lines and the principal strokes may be ready copied for the graver's hand; by which way, besides other uses that may be made of it, copies of rare and choice pieces may be procured, and the perishing or want of the originals supplied: if then the print to be taken off be recent enough, (as it is wont to be, if it exceed not a year, or perhaps two,) then the paper needs only be well moistened, as if it were to be printed off at a rolling-press (with the ink proper to which, it is supposed, that the cut was, as usually cuts are, printed off:) but if the picture or scheme be more ancient, it must be laid all night to soak in water, and then hung in the air, till it have but such a degree of moisture as makes it fit for the rolling-press. The paper being thus prepared, either by bare wetting, or by steeping, the printed side is to be laid upon a copper plate, thinly cased over, as was formerly directed, with virgin-wax; for the plate and paper being put into a rolling-press, the compression of that will make the moistened ink stick to the pure wax, which consequently will take the impression of the cut, or at least of the outlines and chief strokes of it.

THERE is another thing, which seems above all these to require the express and immediate operation of the hand, and it is a physical way, if I may so speak, of transcribing a whole page of a letter, or other writing, all at once. Whether this can be performed cheaply and easily enough for common use, is hereafter to be considered. But that, abstracting from these circumstances, it is possible to be done, (by an artificial application of physical things) I have been persuaded by some experience; of which I may in one of the following papers give you a more particular account, than I now conveniently can.

IN the former part of this essay, *Pyrophilus*, I have presented you some instances, wherein physiological knowledge may be substituted for manual dexterity, mechanical tools, and even mathematical instruments: but now to shut up this discourse, I shall subjoin a relation, that will manifest, that even a mathematician and an engineer may sometimes perform that by the knowledge of a slight physical quality of obvious bodies, which, without that knowledge, all his skill in mathematical disciplines, and his vast and artificial engines, will not have enabled him to accomplish. For who would think, that by a comparatively few pounds of water (perhaps the moisture of the air in wet weather might have sufficed) a massy body of peradventure some hundred thousand pounds in weight should be raised; and yet, that this was performed at *Constantinople*, is one of the remarkablest things I remember I met in the ingenious account of his voyage, that is given by the learned *Busbequius*, embassador from the king of the Romans to the Turkish emperor. His words are these. * *De obelisco, cujus suprà memini, qui est in hypodromo; sic Græci commemorant; à basi convulsum multis seculis jacuisse humi: tempore posteriorum imperatorum repertum architectum, qui operam suam in eo suæ basi restituendo deferret; illumque, postquam de pretio conventum esset, ingentem apparatum organorum ex trochleis & funibus præsertim instituisse, quibus lapidem illum ingentem erexerit, sublimemque eo evexerit, ut uno tantùm digito abesset à dorso astragalorum, quibus imponi debebat, tum indicâsse populum spectatorem oleum illi operam tanti apparatus periisse, magnisque denuo laboribus & impensis opus instaurandum: at illum minimè diffisum perito à rerum naturalium scientia subsidio jussisse afferri immensam aquæ vim, qua multis horis in machinam illam injecta, funes, quibus obeliscus librabatur, sensim madefactos rigentesq; (ut eorum est naturæ) se contraxisse, sic ut obeliscum altiùs sublatum in astragalis statuerunt, magna cum admiratione & plausu vulgi.* And for confirmation of this narrative, it may be added, that the same thing is mentioned by good authors, as having been practised elsewhere; and a like story is allowed, and somewhere made an argument of, to another purpose, by that great master of mechanicks *Galileo* himself.

To catch any store of fish the ordinary way, you know it is customary, that even in rivers, either store of angles, and some skill in using them, or nets, or some other artificial instruments be made use of; and if it be in the sea, that men are to fish, large nets or some peculiar contrivances are employed as necessary; and one would not expect from such people as the Americans, easier ways of fishing than these, and yet these illiterate barbarians, by having found out (probably by chance) the physical property of a wood, make that serve them to catch

* Aug. Busbequii, Epist. I. pag. 69.

catch fish in great plenty, and with as much ease. For our late English navigators have observed, as their voyages witness, that in some parts of the *West-Indies*, the natives, by impregnating the water with this wood, do so stupify the fish, that rolling up and down upon the surface of the water, as if they were foxed, they are easily taken up in great numbers in their hands: which relation of our seamen I therefore, notwithstanding its strangeness, scruple not to alledge, partly because, that though we do not use a simple drug, much less a wood, for the same purpose, yet our foxing-stuff, as they call it, which is but a slight composition, produces effects not much inferior; and partly, because having purposely enquired of a learned physician, that came not long since out of a part of *America*, where this practice is in request, he assured me, that he saw the English themselves use this way of fishing, only by tying a log of this wood, to which, for what reason I know not, they have given the name of dogwood, to the stern of their boats; so easily does the odd property of this wood enable them, that make use of it, to catch fish.

To take off the hair is generally supposed to require both a razor and other implements, and the manual skill and operation of a barber, especially if the hair be grown under the armpits, and in other places, which an inconvenient situation or figure makes to be of difficult access; and yet by the knowledge of a property of that natural production, formerly mentioned in the sixth essay under the name of *Rusma*, the hair may be, without instruments, taken off from any part of the body, and that not only in much shorter time than is required to shaving, but, as far as the eye is wont to discern, by the roots, which makes it much longer before the part be again covered with hair of the former dimensions. The way used in the east to effect this the forecited *Bellonius* annexes, instead of which I shall tell you what I tried with a parcel of it, brought into *England* before I met with his observations about it. We mixed the fine powder of it with an equal weight of strong powdered quick-lime, (*Bellonius*, probably not without reason, prescribes but half as much quick-lime,) and having suffered them to soak together a short while in a little fair water, we thinly spread the soft past or slime, made by the water and ingredients, upon that part of the body, which we designed to free from hair; and having suffered this mixture to stay on about three minutes, or sixtieth part of an hour, measured by a minute-watch, (our author prescribes as long time as is requisite to the boiling of an egg,) we wiped it off with a linen cloath dipped in warm water, and found the hair taken off by the roots, without any inconvenience to the part, that we could discern, though I several times shewed the experiment to others, and the trial of it was more than once made upon my self.

IT may seem scarce possible, without the help of water, or any engine made with springs or wheels, to measure time, though but for a little while, as exactly as our best clepsydra's, clocks, or watches, are wont to do. And yet (which is now a known, and almost vulgar thing) such an account of time may be kept by him, that has observed, that the vibrations or diadroms of a pendulum, are made in sensibly equal spaces of time, though the arches continually decrease, that are made by the swinging pendulum, (as you know they now call a bullet, or the like weight hanging at the end of a string from a nail, or other fixed supporter.) For by so slight a thing, as I have been mentioning, if you watchfully observe and reckon the returns, that the swinging weight makes towards you in a minute, or other determinate space of time, doubling the number of those returns, and adding thereto an unite, if you left off counting, when the weight was at the further end of the arch described by its motion, you may obtain a more accurate division of time, than by any of the formerly known ways of measuring it. For if you make your pendulum of the length of very little (perhaps a tenth of an inch) less than ten inches (or twelve parts of our English foot*) accounted from the nail, or other thing, whence it is suspended, to the center of the pistol-bullet, (or the like small round weight;) and, removing this a pretty way from the perpendicular it naturally rests in, suffer it to fall gently out of your hand, each of its two swinging motions (the one whereby it is carried from you, and the other whereby it returns to you) will be (especially, whilst the arches are of a moderate length) physically *æqui-temporaneous*; and these motions will very distinctly enough, to an attentive eye, divide a minute, or sixtieth part of an hour, into an hundred and twenty parts, (called half seconds,) and will consequently divide an hour into seven thousand two hundred parts, if not perfectly equal, yet less unequal, as to sense, than the divisions of time made even by good watches are wont to be. And therefore this way may be of very great use, in making astronomical and other observations, that last not long, but require exact measures of time. And by the help of a pendulum, a skilful musician of my acquaintance, teaches his unpractised scholars to keep time when they sing in his absence. But when we measure experiments by the excursions and returns of a weight, the best way is to make the duration of the pendulum's whole motion (before it come to rest) as long as the place where the experiment is made will permit, renewing now and then, if need be, the impulse given to the weight, when the arches begin to grow too short; that being increased, the vibrations may be the better reckoned.

THE mention I have been making of the uses of pendulums, joined to that I lately made of æquivelocity of sounds, brings into my mind another instance pertinent to this part of our discourse. For it is not impossible, by the knowledge of the velocity of a sound's motion in the air, and the æquivelocity (as to sense) of

* N. B. The author has elsewhere shewn, that the English foot differs very little, if at all, from the ancient Romans.

of great and ſmall ſounds, to meaſure without geometrical inſtruments, in ſome caſes, the breadth of a river, though exceeding wide, or the diſtance of the place one ſtands in, from the top of a high tower or hill on the other ſide of a river, or ſituated in ſome inacceſſible place, and this, in caſes, where the difference of ſtations uſually in geometrical menſurations is not allowed. The way is evident by what is elſewhere delivered. For it having been found by *Merſennus*'s trials, that ſounds (as well ſmall as great) do move in a ſecond (as they call the ſixtieth part of a minute) 230 fathom, or thirteen hundred and eighty foot; if I ſee my correſpondent fire a gun on the other ſide of the river, or if I ſee muſkets or other guns caſually fired on ſome tower or baſtion, though never ſo far diſtant, and never ſo inacceſſible to me, it is eaſy for me, by letting fall a ſhort pendulum, as ſoon as I ſee the flaſh of light produced by the kindled powder, and by reckoning the vibrations (made by that ſhort pendulum, which diſtinguiſhes ſeconds into halves or quarters) that ſhall happen to be made before the noiſe arrive at my ear, to know how far off the place, where the gun was diſcharged, is from that I am in. As if a correſpondent, ſtanding over againſt me on the other ſide of a river, or ſome ſoldiers being there exerciſing, I ſee the flaſh or ſmoak of a muſket, or other gun, two ſeconds ſooner than I can hear the report of it, I may conclude the river to be 2760 foot broad; and if a piece of ordnance being fired upon the tower of a beſieged place, the noiſe arrive at my ear in half a ſecond, I may collect 690 foot to be the diſtance betwixt that gun and my ſtation. And by this means may that problem be performed, that we elſewhere mention as a thing, which, when nakedly propoſed, may ſeem impoſſible. For if I ſee a ſhip at ſea be ſhooting, whether in earneſt, or for ſalutation, or for joy, it is very poſſible for me to meaſure, without geometrical inſtruments, how far it is off, though the ſhip itſelf be under ſail. For veſſels, that fire guns, uſually firing more than one, whether to offend their enemies, or to ſalute their friends, it is eaſy to take warning, by the firſt gun, to be in readineſs with a ſhort pendulum againſt another to be fired, and in this way of meaſuring (though not in any other yet known) one may take diſtances in the darkeſt night. For it matters not, whether I ſee the ſhip or place, whoſe remoteneſs from me I would know, provided, by ſome candle or taper I ſee the pendulum before the flaſh of the fired gun, which will ſufficiently diſcover itſelf by its own light. And (to add, that upon the by) I have had ſometimes thoughts, that if the velocity of eccho's, which are but reflected ſounds, be ſo well determined as that of direct ſounds, navigators might ſometimes make uſeful eſtimates in dark nights, whether they be near coaſts, or conſiderably great rocks. For though upon diſcharging a gun, they cannot conclude, how near the ſhore they are, becauſe there may be parts of it leſs remote than thoſe that ſend the eccho; yet if they follow very quick upon the diſcharge of the gun, they have reaſon to ſuſpect, that the ſhore, whoſe approach the ſeamen do ſo juſtly fear in the night, is at leaſt, as near as the vibrations of the pendulum inform them, that the eccheing place is.

ESSAY X.

OF MEN'S GREAT IGNORANCE Of the USES of NATURAL THINGS: OR, That there is ſcarce any one Thing in Nature, whereof the Uſes to human Life are yet thoroughly underſtood.

THIS being an entire propoſition, and clear enough of itſelf, will not need to be explicated, but evinced.

AND evinced ſomewhat ſolemnly it will require to be, not only becauſe it is a paradox, but ſuch an one, as will meet with a peculiar indiſpoſition to be entertained; ſince men cannot allow this paradox to be a truth, without ſuch a confeſſion of their ignorance, as muſt implicitely accuſe them of lazineſs too. But however, I think, we may juſtly enough apply, with a little variation, to our preſent purpoſe, that true ſaying of *Seneca*, *Multi ad ſapientiam perveniſſent, niſi*, &c. and affirm, that many

many had attained to a greater knowledge and command of nature, if they had not presumed, that what is arrived at already, is much greater, and more considerable than indeed it is; especially, in comparison of what is still behind, and yet attainable: and therefore, I think it not fit to suppress the considerations I was about to mention, since the displaying them may perhaps do you and others service, if they rouze up your curiosity, by shewing how much it has been defective, and if (which they ought to do) they encourage it also, by shewing you how much of nature undiscovered there yet remains, to recompence, as well as exercise your industry.

BUT because that of the particulars, whereby our paradox may be confirmed, there are divers, that properly belong to the next ensuing essay, the proofs, that we shall mention in this discourse, though I hope they will appear sufficient alone, will yet be much strengthened, both as to number and weight, if you please to add to them those instances to be mentioned in the next discourse, that may be conveniently referred to this. In which I shall therefore insist but upon five general considerations; in all which I hope you will not forget, that I have already taken it for a supposition, which I doubt not of your granting me, that the usefulness of the works of nature to us depends chiefly upon the knowledge we have of their properties and other attributes; and consequently, that the more we know of these, the greater use we are like to be able to make of those physical things, (and on the contrary.) And therefore, that ought to be looked on as an use of a physical thing, even though not immediately practical, that helps us to make discoveries of things, that properly may prove so.

SECTION I.

AND I consider in the first place, "That "there are very few of the works of na-"ture, that have been sufficiently considered, "and are thoroughly known," even as to those qualities, and other attributes of this and that body (or other physical thing) which belong properly to it, and are not thought to be so relative to other bodies. It is not only in the terrestrial globe, but in almost every body to be met with in it, that there may be a kind of *terra incognita*, or undetected part, whose discovery is reserved for our further industry.

THIS will appear the less improbable, if we consider these two things; whereof the one is, that there are divers ways of investing the attributes of bodies, as chemical, optical, statical, &c. which being artificial, and requiring skill, and industry, and instruments, there are very few men, that have had the curiosity and ability to examine them after these several ways: without which, nevertheless, divers other attributes, some of which probably are capable of useful application, are not like to discovered. To the proof of which, if it were needful, a multitude of passages in these present essays, as well as in our other writings, might be easily referred.

I shall therefore rather insist a little on the second of the two particulars lately mentioned. For it will easily appear not unlikely, that there should be many things undiscovered in the others works of nature, when there are so even in those obvious and familiar objects, that men are frequently conversant with, and have occasion to take notice of; nay, even in those noblest of mere corporeal things, our own bodies, whose structure does so much merit our curiosity, and of which it so highly concerns no less than our healths and lives, that we have an accurate knowledge. How many new discoveries have been made in the present age, beyond what the industry of the physicians and philosophers for above two thousand years has been able to take notice of? Witness the circulation of the blood, the Asellian, Pecquetian, and Bartholinian vessels; to which may be added, the Ductus Pancreaticus, and to which I doubt not will be added divers other discoveries, to recompence the industry of the anatomists of this inquisitive age.

IN so familiar bodies as eggs and chickens are, which so many thousand persons do daily see and handle, and perhaps eat, though many ages since, even *Aristotle* was solicitous about the history of them, concerning which he has delivered divers not inconsiderable particulars; yet there has been little within these few years so much undiscovered, that whilst men were hotly disputing, whether the chick was first formed of the yolk or the white, our excellent *Harvey* made it evident (which our own observations have confirmed to us) that it is made of neither, nor yet of the tredle, (as some modern observers have taught,) but of the cicatricula, or speck, that appears on the coat of the yolk.

WHO would imagine, that in a body so familiar, and so often treated of by philosophers, as snow, mankind should, for so many ages, take no notice of a thing so obvious as the figure of it frequently is? and yet *Kepler* is, by a very learned writer, acknowledged to have been the first, that acquainted the world with the sexangular figure (as it is wont to be called) of snow, in a discourse by him published on that subject. And though I find mention made of it in *Olaus Magnus*, and have observed it so often (but not constantly in the same shape,) especially about the beginning of the season of snow, that I cannot but admire, men should not have very early heeded so obvious a phænomenon; yet I find not the discovery of it had been made so much as an age ago.

AS many ages as vinegar has been one of the commonest liquors in the world, yet, that it oftentimes abounds with shoals of living creatures, that move, and in the microscope look like little eels, was looked upon but few years since as so new a discovery, that when, as I formerly noted, I first proposed it here in *England* to divers very learned men and virtuosi, as a thing to be seen even without the help of a magnifying glass, they took it to be a deception of my eyes, till their own assured them of the contrary.

THAT the milky way, though consisting of innumerable stars, should for two thousand years pass for a meteor, the inconspicuousness of those stars keeps me from much admiring. And for the same reason I wonder not, that the men, that lived before *Galileo*, reckoned no more than seven planets, or suspected not, that *Venus* herself is sometimes horned, and has her full and wane as the moon. Though these instances may serve to confirm what I lately told you, that many of the attributes of bodies are not like to have been discovered by those, that employed not artificial helps. But what may we not expect, that mankind may overlook, when the sun himself, which is not only the most conspicuous body in the world, but, that by whose light, we see all the others, may have vast and dark bodies (perhaps bigger than *Europe* or *Asia*) frequently enough generated and destroyed upon him, or about him; and men, without excepting astronomers, never took notice of it, till of late years the excellent *Galileo*, or the industrious Jesuit *Scheiner*, informed the world of them. And though I grant, that they discovered them by the help of telescopes, (instruments unknown to the ancients,) yet if men had been as watchful, as the nobleness and conspicuousness of the object would make one expect, they might have discovered some spots at least, without those helps. For I find by an Italian letter of *Galileo*'s, that some curious persons of his acquaintance, after his discoveries had awakened them, descryed and discovered some of those solar spots with their naked eyes unassisted by his tubes.

IT may belong to this first section of our present essay to take notice, that one account, on which we may reasonably suppose men to be ignorant of the uses even of those things, wherewith they think themselves well acquainted, may be, that the bare difference of climates, and of places, may even in such bodies, as we familiarly converse with, beget such new relations betwixt them, as may endow them with qualities, and fit them for uses we dreamed not of.

I will not here mention the differing qualities, that bodies vulgarly referred to the same species of plants, animals and other bodies, in almost all countries, are endowed with in some countries; (as, that spiders are not venomous in *Ireland*, and Irish wood in general, if the received tradition be true, has an hostile faculty against venomous creatures,) because the insisting on this subject would take up too much room in this place, and is reserved for another; and therefore I'll only add a couple of instances, the one to manifest what difference of climates may do, and the other to shew the unexpected influence of difference of places, though perhaps in the same climate.

THE first of these examples is afforded us by water and ice; for those, that live in those warmer regions, where it never freezes, and who have divers of them derided the relations of what happens in gelid climates as ridiculous, in probabilty would never dream, that it could be a familiar use of a liquor they were so well acquainted with as water, to be broken or beaten in mortars like a dry body, and carried in carts or wheel-barrows from place to place, and kept all the year in that form, to make other water intensly cold in the greatest heats of summer. And even amongst us, those, that have not been very inquisitive, can scarce imagine, that one of the uses of water should be to serve for high-ways, whereon armies may march for divers days together, with all their carriages and artillery, and whereon they encamp and fight battles with as much assurance as on the firm land; and yet those, that have been in *Russia*, and the neighbouring northern countries, assure us, that during the winter, when the rivers are frozen over, they usually take great journies on them, and oftentimes rather than in summer, and choose that rigorous season, which allows them to march every where without sinking into the ground, to prosecute their wars in.

The second of the forementioned instances we are supplied with by the declination of the magnetick needle from the true north and south points, and the variation of that declination. For though the loadstone were highly admired as well by philosophers and mathematicians, as the vulgar; and though, since the great and happy use of it to navigation has been generally known, men have been upon several accounts invited to consider it with a peculiar attention and regard, yet that in some places the magnetick needle does not point directly, perhaps not by a great many degrees, at the pole, as in others it does, is no ancient observation, since it is ascribed to *Sebastian Cabot*; and it appears by the writings of our famous countryman *Gilbert* himself*, that it must be some body, that lived since he wrote, that must have the honour of being allowed the first observer of that strange and unexpected phænomenon, that oftentimes in the self same place, the declination of the needle towards the east or west does in process of time considerably alter. Which discovery I could confirm, by comparing some observations I have had opportunity to make, with those recorded by some modern authors.

AND as the same kind of bodies may have differing qualities, and consequently uses in differing places; so they may have, if examined or employed at differing times, comprising under that name, together with the four seasons of the year, those peculiar seasons or periods of time, to which some signal change of qualities or state in particular bodies do belong.

THE mutations, upon the account of time, which I am here speaking of, are not those, that are obvious to every eye, such as the differing qualities of fruit green and ripe, or the degeneration of wine into vinegar; but such as are not vulgarly taken notice of, and require either skill or curiosity, or both, in the observer; and of these a few instances will suffice for a taste.

WHEN common urine either is freshly made, or has not long been kept, the volatile and

* *Gilbert* de magnete, lib. I. c. 1. See the same *Gilbert*, lib. IV. c. 3.

and pungent salt is so clogged with other particles, wherewith it is associated, that usually, to obtain it, one must evaporate or distil away near eight or nine parts of ten of the liquor, and then at length give a not inconsiderable heat to force up the last: but though the tradesmen, that deal in urine, do commonly overlook the difference, yet if the crude liquor be kept six or seven weeks, though not near the fire, the saline and noble parts will have so extricated themselves, that a very gentle heat will make them ascend, and leave behind them that phlegm, that formerly would have preceded them.

THAT the Thames water, which our navigators are wont to take with them in long voyages, after a while, if they sail into hot climates, stinks very often too offensively to be potable, that, which happens usually to water, which is vulgarly observed to putrefy by long standing, will easily persuade us; and yet it is found, that this water, by being kept long enough in the same vessels, though it be in the same, or even in an hotter climate, will at length loose its stink, and grow potable again; as I have, upon enquiry purposely made, been assured, not only by the vulgar tradition, but by two very inquisitive persons upon their own knowledge; the one having particularly observed it, sailing betwixt *Europe* and *Africa*, and the other in a voyage to, and from *America*. And I the rather mention this, because I am very credibly informed, that there are divers other waters, that have this faculty of recovering after putrefaction, which is supposed to be peculiar to the water of the Thames.

AND, if I much mistake not, one or both of these very persons named another river to me, with an affirmation of its having the same power of self-recovery. And having held some curiosity to try experiments, how pump-water, or the like rough water, as they call them, that would not bear soap, may be helped; an industrious person I employed assured me, that he met with pump-waters, which after having stood a few days, without having any thing done to them, would bear soap, which before they would not do.

CORIANDER seeds being freshly gathered have been observed to have so much acrimony, that divers of the ancient physicians reckon them among venemous plants; and in dispensatories they are usually prescribed to be prepared with vinegar, or some other corrective: whereas the more accurate observers take notice, that within a competent time after the seed is gathered, it loses of it self that excessive acrimony, that at first blemished it. And the like I find observed, by good apothecaries, of the roots of aron, which are mitigated by keeping, (and which some noted physicians of my acquaintance do little less magnify to me than does *Quercetan* himself.)

[THAT vegetables, what known way soever they are wont to be laid up, and ordered, do not afford, unless first reduced to soot, any dry volatile salt, like that of animal substances, I elsewhere more particularly declare, and those, that have had the curiosity to try it, will confirm: but yet by some discourse I lately had with a very ingenious person, and some subsequent trials made after a way I devised to examine distilled liquors, I was satisfied, that there are divers vegetables, and those very commonly growing here in *England*, which being gathered and laid together at a certain season, and distilled also at a certain nick of time, will yeild, instead of the vinegar-like, and other liquors, wont to be afforded by such plants distilled the common way, a volatile spirit; which in smell, taste, and divers operations, as turning syrup of violets green, hissing with acid spirits, &c. resembles the volatile spirits and salts of animal substances; and, which I doubt not but you will wonder at, this great change, whose secret I wish I durst teach you, is effected without the help of any additament.]

AND, that you may not think, that it is only in vegetable and animal substances, that are commonly of a more loose or alterable texture, that the trying things at one time rather than another may be very considerable, I will add a couple of instances, even in mineral bodies.

IT is a chemical complaint, even of the curious and experienced, that though authors teach us to make the salt of violently distilled or calcined vitriol, by forthwith taking the caput mortuum, (from which all the oil has been by the violence of fire forced out) and extracting the saline part by effusions of water; yet those, that make exact trials of it find, that when the dark red mass of powder is newly taken out of the vessels, it is so totally robbed of its saline particles, that no affusion of water will at all obtain from it the expected salt. Notwithstanding which, having purposely enquired of some, that distil great quantities of oil of vitriol, whether or no, when they had made an end of one distillation, if they lay by the caput mortuum for a pretty while in the air, they could not find it impregnated enough with new saline particles, to be fit to yeild more menstruum, and be worth another distillation? I was answered in the affirmative, provided, that (as I mentioned in the state of the case) there were a competent time interposed between the former and the latter distillations. (The reason of which, according to my trials and conjectures, may be assigned of this odd phænomenon, belongs not to this place, but you will hereafter meet with it in another.)

THE second instance I promised you, is afforded me by stones; for there are, and not far from this place, quarries of solid and useful stone, which is employed about some stately buildings I have seen, and which yet is of such a nature, wherein divers other sorts of stone are said to resemble it, that though, being digged at a certain season of the year, it proves good and durable, as in those structures newly mentioned; yet employed at a wrong time it makes but ruinous buildings, as even the chief of those persons, whose profession makes him more conversant with it, has himself acknowledged to me to have been found by sad experience.

rience. But concerning this observation, you may expect to meet elsewhere with a farther account.

AND though time and place be two of the principal, yet they are not the only circumstances, whose variations may make some such attributes discovered in natural things, as are not usually heeded; of which I shall mention but a couple of instances, because they may serve to shew you, that such circumstances, as are thought the slightest, may afford new uses even of solid and lasting bodies. Skilful artificers, that grind optical glasses for tubes, have complained to me, that oftentimes the convex glasses they fashion, will prove veiny, and consequently, after all their labour, of little value; and yet they are not able to discover these unwelcome veins in the glass, by the most careful viewing it against the light, till they have spent a pretty deal of time about working of it; and even then they are unable to descry these blemishes, if they hold the glass at an ordinary distance from the eye; but they are obliged to remove it a great way (perhaps six or seven foot) farther, so much may an increase of distance become serviceable, even where one would expect the quite contrary.

BUT probably you will look upon posture as a slighter circumstance than distance itself; and yet Dr. *Gilbert* has observed, and I have found it true by many trials, that long irons, as the bars of windows, that have stood upright for a great while, do, by that perpendicular posture, acquire a verticity or magnetick virtue, as having acquired magnetick poles. So that if you apply the needle of a dial, which I mention as the readiest way of trial, to the lower part of the bar, it will draw the south end of the needle; whereas the upper extreme of the bar will seem to drive away that end, and will draw the northern.

BUT here I must not forget to take notice, that I can scarce think men will be able to know all the properties and uses, even of familiar bodies and other things, till they have mathematically considered them; there being several attributes belonging even to such things, which a naturalist, though curious, will probably never find out, unless he be both acquainted with mathematical disciplines, and have the curiosity to apply them to physical subjects. And though in other essays of this book, divers things are delivered, that do directly enough tend to manifest what I have now said; yet it is of such importance, that naturalists should be thoroughly persuaded of a truth, that may be so much more useful than it is yet generally admitted, that I am content to inculcate it, by setting down here a few instances of somewhat a differing sort from those elsewhere delivered, and more appropriated to the present subject of our discourse.

YOU will not doubt, but that ever since the first ages of the world, the majority of men have had some occasion or other to see bodies swing; and yet, till *Galileo* (for he is generally believed the discoverer) took notice of the vibrations with a mathematical eye, men knew not this property of swinging bodies, that the greater and smaller arches were, as to sense, equitemporaneous; from which discovery have been derived several practises of good use, some of which have been already mentioned in these essays.

THAT water, running out at a hole made in the sides near the bottom of the vessel, makes a parabolical line, or one that near resembles it, and that in such effluxions of water, there is a determinate proportion assignable betwixt the perpendicular height of the liquor, and the diameter of the hole, whereby the velocity, and quantity of water, that would run out, may be computed, has not been, that I know of, taken notice of, till the observations of the above-named *Galileo*, and the diligent *Mersennus* (to which we may elsewhere add some of our own) have endeavoured to define those matters.

AS constantly as we have occasion to take notice of the air, and water, and glass, yet the curiosity of our modern masters of opticks has observed many things touching the refraction of the beams of light, made in those mediums in different quantities, and to and from the perpendicular, not to say any thing of the equality of the angles of incidence, and of reflection made on the surface of still water, unheeded by those that are not versed in opticks: the drops of dew, that hang in numberless multitudes upon the grass and leaves, are things, that every eye has been invited to take notice of by the orient colours the sun is wont to make them afford us; but till the excellent *Des Cartes*, contemplating them with Meteorum, cap. VIII. a more critical eye, found, that in such a determinate angle made at the spectator's eye, between the ray of light coming from a certain part of the drop, and the imaginary straight line reaching from the eye to the sun's center, the drop appeared red, and in another determinate angle exhibited yellow, blue, and other colours, and at other angles, shewed no colour at all; the world ignored a considerable property of spherical diaphanums irradiated by the sun, and seems not to have dreamt of a neat hypothesis, with which some ingenious mens minds are no less taken, than their eyes are with those vivid colours of the rainbow, which it pretends to give a clear account of. And though we daily see pieces of wood and timber broken by the weight of over heavy bodies, yet till the often named, and still to be commended *Galileo* applied geometry, and the doctrine of proportions to matters of this kind, the resistance of solid bodies to be broken by weight (whether their own, or that of other bodies) seems not to have been so much as suspected to be reducible to such an estimate, as he and others have brought it to. And a virtuoso of my acquaintance, (for whom *Mersennus* laid the way) in a musical instrument, that I have with pleasure heard him play on, can observe a property of metals, that chemists thought not of, namely, that equal wire-strings made of differing metals, and having a due tension, will yeild sounds differing as to sharpness by determinate musical notes, or the divisions of them. And to these I might add divers

vers other remarks of *Mersennus* and *Galileo* about the force of guns, (which were found to increase with their length but till such a number of feet, beyond which the length did but lessen it) and the parabolical line, in which bullets (that are thought of all other bodies to move the straitest) are said to move; and I know not how many other mathematical attributes, if I may so call them, of natural things, that geometricians, astronomers, engineers, &c. have already observed, might be here added, but that I think it sufficient to subjoin one instance more, that may well serve to keep us from imagining, that even the most familiar objects in the world, and that seem likely to afford the least discoveries, have been sufficiently considered. For how few phænomena in nature are there, that occur to us more frequently than the falling of heavy bodies? And yet though the ancients and *Aristotle* himself took notice, that there was an acceleration of descent in falling bodies, yet we find not, that any so much as fairly attempted to determine that acquired velocity, till *Galileo*'s observations reduced it to the proportion mentioned in some of the former essays, wherein most of the following mathematicians (for I have scarce met with two dissenters) have acquiesced; and whereby in the eighth essay we endeavoured to measure heights and depths without geometrical instruments. In a word, till geometry, mechanicks, opticks, and the like disciplines be more generally and skilfully applied to physical things, I cannot think otherwise, than that many of the attributes and applications of them will remain unknown; there being doubtless many properties and uses of natural things, that are not like to be observed by those men, though otherwise never so learned, that are strangers to the mathematicks.

AND as I have hitherto observed of bodies, so I shall venture to add of qualities, and divers other natural things, that even those, that are very familiar, may have attributes and uses, which the generality of men, without excepting those that are otherwise learned, are not wont to take any notice of.

THAT black bodies, for instance, as such, are much more strongly and easily warmed by the sun beams than white ones, nay, though the disparity be not so great, than bodies of any light colour, *cæteris paribus*, is perhaps more than even you have taken notice of: and yet I shall hereafter have occasion to prove it by divers instances, and you may easily try it, either by exposing for some time to the summer-sun a white glove and a black, or a couple of eggs, whereof one is inked, or otherwise blacked all over.

COLD is one of the most familiar qualities men have to deal with; and though they otherwise are not wont to expect much from it, yet least of all would they expect that it should, contrary to the received definition of it, which is, *congregare tam heterogenea quàm homogenea*, that it should, I say, perform the office of heat in spirit of wine, nay, and in presenting us ardent spirits from beer and other liquors inferior to wine; and yet, not to mention *Paracelsus*'s process of making the essence of wine by freezing all the flegm, we have the repeated experiments of navigators into the frigid zone, who assure us, that not only from wine, but from beer, by the congelation of the aqueous parts, there may be separated or obtained a liquor, strong, hot, and spirituous, almost like aqua-vitæ.

AND even in our temperate climate some odd separations may be made by cold; for, not to anticipate those trials of mine, that belong to other papers, there may, by such cold as we have here, be made a separation in oil, of a liquor much finer and more spirituous than the rest; for I know an eminent artificer, who kept it as a choice secret to resort (as himself confessed to me he did) in hard frosts to the great jars of oil, where he often found greater or lesser cracks or chinks in the congealed part of the oil, in which cranies was contained an unfrozen liquor, that appeared thinner and finer than common oil, and was much better than it to preserve things from rusting, as perhaps having left many of its saline parts in the concreted oil; and for that purpose was much prized, not only by him, but by some watchmakers, that were made acquainted with the virtue of it.

BUT it were tedious to insist on all the instances, that may be brought of the applications, that may be made of colour, sound, levity, springiness, fermentation, and even putrefaction; and it would be not only tedious, but almost endless to prosecute those instances, that might be afforded by other more general and operative states and faculties of bodies. For not only motion and rest, fluidity and firmness, gravity, and the like, have a more universal influence of natural things, than even philosophers are wont to take notice of; but those less catholick affections of matter, that are reckoned among but particular qualities, such as gravity, and heat, may have so diffused an influence, and be applicable to so many differing purposes, that I doubt, whether all the uses of that particular degree or pitch of heat, that reigns in fire, will have all its uses discovered, before the last great fire shall dissolve the frame of nature.

NOR must I pretermit one consideration more, that belongs to my present subject, which is, that probably many more qualities, or other attributes, would be taken notice of, even in those natural things, that are reckoned among the most known, if men did not want a measure of curiosity, that might justly be expected. For I speak not here of curiosity in general, (which I doubt not would make far more numerous discoveries, than were necessary to justify my present discourse,) but I only speak of such a curiosity about the things of nature we familiarly converse with, as we could scarce want, if it were not out of laziness, or a prejudicate opinion, that makes us take that for granted, that we should find to be quite otherwise, if we did not chuse rather to presume than to try.

THUS, that falling bodies, the heavier they are, the faster in proportion they fall, has been a received opinion in the ſchools ſince *Ariſtotle*'s time, and has kept the equivelocity, as to ſenſe at leaſt, of bodies of very differing bulks and weights falling from moderate heights, ſuch as ſurpaſs not ordinary towers and ſteeples, from being taken notice of, till of late inquiſitive men by experiments found it out.

THAT water by glaciation is reduced into a leſſer room, has been and is ſtill the opinion, not only of the vulgar, but of the generality of learned men; and yet, that water is not condenſed, but expanded by freezing, he that will congeal that liquor in veſſels ſtrong enough, may eaſily find by trial. And the floating of ice upon water, and the bubbles, that are uſually to be obſerved in it, may alone to ſuffice to make a conſidering man diſtruſt the vulgar opinion.

THAT the common air we breath and live in, is a body endowed with poſitive levity, has been for many ages, and continues to be almoſt univerſally believed; and yet if men had the curioſity to examine this ſuppoſition by one or other of thoſe ſeveral ways, by which the gravity or levity of the air may be diſcovered, they would quickly find, that it is not devoid of weight. And even ſo ſlight a way as the condenſing the air in a blown bladder, by tying a ſtring ſomething ſtrong about the middle of it, may bear witneſs to what we ſay. For though we ſhould oppoſe, as ſome have lately done, that in ſuch caſes the air is not in its natural ſtate, but condenſed; beſides, that is an objection, to which all the expedients of weighing air are no way liable, it makes rather againſt the objectors, than the concluſion, againſt which they urge it; ſince, if the particles of the air be really light, the filling the bladder the fuller of them ought to make it rather lighter than heavier.

THAT greater and leſſer ſounds do, as to ſenſe, move with an equal ſwiftneſs, is that, whoſe contrary is taken for granted; and the more excuſably, becauſe it is evident and confeſſed, that great and ſmall ſounds do not move equally far: and yet, that this equivelocity of ſounds has been made out by the late obſervations of the diligent *Merſennus*, and others, you may remember to have been delivered in a foregoing eſſay, where I alſo endeavoured to ſhew, that this property of ſounds is not unappliable to human uſes. The VIII.

THAT the loadſtone, which by immediate contact will take up iron, ſhould have ſo ſtrange a property, as to take up far more when a cap, or conveniently ſhaped piece of ſteel is interpoſed betwixt it and the body to be raiſed, is a thing ſo unlikely, that though the ancients knew and much admired the attractive virtue of the loadſtone, yet they ſeemed not to have ſuſpected it enough to vouchſafe it a trial: and yet ſince *Gilbert*'s writings came abroad, he muſt be a great novice in magnetical affairs, that either ignores or doubts it. But I muſt not do any more than touch upon magnetical experiments, ſince they alone would afford me ſo many truths (which the generality of men would not have thought likely enough to be worth trying) that to enumerate them, though it might convince your underſtanding, would, I fear, exerciſe your patience.

THAT it is the property of unſlaked lime to grow hot by antiperiſtaſis, upon the pouring on of cold water, and other cold liquors, and conſequently not to grow hot upon the effuſion of liquors, that are not cold, is not only generally believed, both by learned and unlearned; but this property of lime has been employed as an argument to prove other matters, as well by divers of the new philoſophers, as by many of them, that embrace the old Ariſtotelian principles: whereas I doubt not but a little trial might eaſily diſabuſe them: for by the affuſion of divers liquors actually warm, I have made lime ſlake with its wonted violence, if not with a greater. And in other liquors actually cold, like unheated water, and one or two of them far more thin or ſubtle than it, I have kept lime long without ſlaking, and without imparting to the ambient liquor any ſenſible heat. The quality of theſe inſtances makes me think it needleſs to increaſe their number, ſince we can ſcarce wiſh a greater inducement to expect, that many new attributes may be diſcovered in the works of nature, if men's curioſity were duely ſet on work to make trials, than that divers have been found out, that ſeemed ſo unlikely, that men thought it would be in vain to try them.

To theſe ſeveral ſorts of inſtances, that have hitherto been reduced to our firſt conſideration, might well be added, that bodies, which have the ſame denomination, and from whence men are therefore wont to expect the ſame, and but the ſame, operations and uſes, may yet have peculiar ones, and ſome of them very differing from thoſe of the generality of other bodies, that bear the ſame name. But examples of this kind will more conveniently be mentioned in the laſt eſſay: and leſt this ſhould ſwell too much, diſmiſſing this preſent conſideration, we will advance to the next.

SECTION II.

I Conſider in the ſecond place, that the faculties and qualities of things being (for the moſt part) but certain relations, either to one another, as between a lock and a key; or to men, as the qualities of external things referred to our bodies, and eſpecially to the organs of ſenſe, when other things, whereto theſe may be related, are better known, many of theſe, with which we are now more acquanted, may appear to have uſeful qualities not yet taken notice of.

I ſhall elſewhere, *Pyrophilus*, have occaſion to ſhew you more fully on what grounds, as well as in what ſenſe it is, that I take the moſt of the qualities of natural bodies to be but relative things. To our preſent purpoſe it may ſuffice to adumbrate my meaning by the newly hinted example of a lock and a key, where, as that, which we conſider in a key, as the power or faculty of opening or ſhutting, ſuppoſes, and depends upon the lock, whereto it correſponds; ſo moſt of thoſe powers and other

other attributes, that we call qualities in bodies, depend ſo much upon the ſtructure or conſtitutions of other bodies, that are diſpoſed or indiſpoſed to be acted on by them, that if there were no ſuch objects in the world, thoſe qualities in the bodies, that are ſaid to be endowed with them, would be but aptitudes to work ſuch effects, in caſe convenient objects were not wanting. As if there were no lock in the world, a key would be but a piece of iron of ſuch a determinate ſize and ſhape. And this compariſon I the rather imploy, becauſe it may be further applied to our preſent diſcourſe. For as if ſome barbarous American ſhould, among other pieces of ſhipwreck, thrown by the ſea upon the ſhore, light upon a key of a cabinet, he would probably look on it as a piece of iron, fit only for the inconſiderable uſes of any other piece of iron made much broader at each end than in the middle; but, having never ſeen a lock, would never dream, that this piece of iron had a faculty to ſecure, or give acceſs to all, that is contained in ſome well furniſhed cheſt or rich cabinet: ſo there is many a thing, that ſeems to us uſeleſs, whilſt we look upon it only in itſelf, which will perhaps hereafter prove highly uſeful, when we ſhall light upon ſome other bodies peculiarly fitted to act upon it, or receive impreſſions from it. But this will be better apprehended by the following inſtances.

THOUGH iron be ſo common a body as it is, and its uſes are very many, and have been known as long as ſince *Adam*'s time, yet all thoſe differing bodies, on which men of all ſorts imployed it to work, and all thoſe various ways, whereby chemiſts, phyſicians, and mineraliſts have wrought on it, during ſome thouſands of years, did never diſcover to man one of its nobleſt and uſefulleſt properties, which, for aught we know, was never found out till within theſe three or four ages: for a ſteel needle, being applied to a loadſtone, manifeſted itſelf to be capable of conſtantly ſhewing the north and ſouth in all ſeas, in all weathers, and in all times of the day and night to navigators, who, by this property, which depends upon the relation that iron has to one only ſtone, have been enabled to diſcover the new world, and enrich the old with the drugs and treaſures of it.

AFTER all the vain attempts, that even ſubtle chemiſts have made to arreſt the fluidity of quickſilver, the knowingeſt perſons, that have medled with that mineral, and eſpecially if they have obſerved, that the keeneſt froſts, that are capable of freezing even aqua vitæ, are unable to congeal it, have been very much indiſpoſed to reckon an eaſy coagulableneſs amongſt its qualities; and yet we ſee, that though the mixture of no other known body will diſcloſe its having any ſuch affection, yet the vapour of melted lead will ſometimes (for that experiment will not always ſucceed) reduce quickſilver, even in its maſs, into a conſiſtent and ſomewhat tough and hard body.

VINEGAR being a liquor, that has been generally known and uſed for ſome thouſands of years, men have imployed it upon great variety of bodies, and to very many uſes, but eſpecially to communicate a ſowerneſs to the things, wherewith it was mingled; but when it came (probably by chance) to be applied to the diſſolving of lead calcined or crude, it manifeſted, that it had a faculty to exhibit a more than ſaccharine ſweetneſs, which, for aught I know, it exhibits with that metal only; for I have not yet known crude vinegar diſſolve tin, though calcined: and though by a ſlight artifice, elſewhere mentioned, we have been able to make ſtrong vinegar diſſolve the calx of Jupiter, yet was the ſolution far differing from, and inferior to, the taſte of the ſolution of lead newly mentioned.

SPIRIT of urine is a liquor, that has been long known to chemiſts, and might reaſonably be looked upon as likely to be a good menſtruum for ſeveral bodies: but it is not probable, that after it had been imployed to diſſolve divers compact bodies, it ſhould be ſuſpected, that it would coagulate ſo thin, light, and fugitive a body, as ſpirit of wine itſelf; and yet we have often (as there will be hereafter divers occaſions to relate) tried, that if both liquors be ſufficiently pure and dephlegmed, they will afford that ſtrange ſnow-white concretion, that *Helmont* calls his *Offa alba*; which, however by his followers aſcribed to him as the inventor, I find mentioned in ancienter books than his: and I remember, that even *Raymond Lully* relates, with what wonder he firſt ſaw this experiment (which indeed is conſiderable) performed.

AND as the ſpirit of urine has ſuch an odd property, when it meets with ardent ſpirits dephlegmed; ſo the ſpirituous parts of urine, without being ſeparated from the reſt, have a faculty, that one may yet leſs expect, if they be duly imployed, to operate upon muſk: as I have had the opportunity to inform myſelf by inquiry of a ſcholar, who lived in *China*, and affirmed himſelf to have divers times ſeen muſk made. For this perſon anſwered me, that he had obſerved it to be the practice of others, and had made trial of it himſelf in thoſe eaſtern parts, that the muſk being made up, and put into cods or bags made of the ſkin of the ſame animal, (in which form I have received preſents of muſk ſent me from the *Indies*) they do, either before or after, hang it in a houſe of office, ſo as it may, without touching the groſſer bodies, receive the fœtid exhalations of that naſty place; by which urinous ſteams, which it is expoſed to for ſome days, the leſs active, or more immerſed ſcent is, as it were, called out, and excited or heightened. And I found, by farther enquiry of the ſame perſon, that having carried muſk from thoſe eaſtern regions, where it is made, to other and remote parts of the ſame *Indies*, he found, that, by the length of the voyage by ſea, his muſk had very much loſt its ſtrength, which he afterwards reſtored to it, by following the advice of ſome ſkilful perſons, according to which he tied the muſk cloſe in a bladder, wherein, having pricked many little holes with a needle, he hung it up for ſome days in ſuch a ſtinking place as has been

been newly mentioned. Whereto agrees very well what I have read in a late eminent physician of *Rome*, (where the art of perfuming is very much cultivated) who communicates it as the chief secret practiced by the perfumer's there, for recovering the scent of decayed musk, that it be kept for a competent time in linnen well moistened with rank urine.

THE uses of gesso (as the Spaniards and Italians call it,) or gypsum, are numerous enough in the shops of stone-cutters, moulders in plaister or wax, and divers other artificers; but one would scarce suspect, that, besides the various uses these tradesmen put it to, it should have one so very differing from them, as to be an excellent medicine, if I may so call it, for wine: and yet, that they use great store of it about those choice ones, that come to us from the Canaries, is a noted tradition among those, that deal in that sort of liquor, and has been confirmed to me by an eminent wine-merchant, that lived several years in those islands. And, that about *Malaga* they put up a good proportion of it into the juice of their grapes, when they tun it up, is affirmed to me by a curious eye-witness, who was there in vintage time, and of whom I purposely enquired about it.

THOUGH silver be so noble a metal, and so much known and used, that it was the price of things as early as *Abraham*'s time, yet one very fine use of it has been known but since the art of annealing upon glass came to be practised. For among other experiments of this art we find, that prepared silver (and I have sometimes done it pretty well with the crude metal) being as it were burned upon a plate of glass, will tinge it with a fine yellow or golden colour: there are also divers mineral earths, and other coarse fossils, of use in this art, which, by the help of the fire, makes them impart colours to glass, both transparent, and sometimes very differing from those of the bodies themselves, as I may elsewhere have occasion to specify. In the mean time, give me leave to name this reflection upon the art of painting, that it is very hard for us to be sure, that we know so much, as all the several sorts of uses, that may be made of the particular bodies we converse with, since upon the invention of a new art or trade, of which we know not how many remain yet to be found out, divers uses and applications of bodies come be disclosed, that were never suspected before.

THE use of lyes made with common ashes to wash linnen has rendered them for these many ages very familiar: but though their effects on the other bodies, upon which they have been employed, seemed not to have any affinity with what I am going to mention; yet when a strong lixivium is applied to syrup of violets, (which is also a very known liquor) to which it has a peculiar relation, it will then immediately change the colour of that syrup from a blue to a perfect green, and so it will the violet leaves crushed on a piece of white paper, without the help of sugar, or any preparation.

REDNESS, though a colour as obvious as most others, and to the generality of men very pleasing, however it hath no offensive property, in reference to other animals, familiarly known amongst us, (at least, that we have taken notice of;) yet being presented to the eyes of turkey cocks, it has such an incongruity with them, that oftentimes it is observed to make them very angry, as far as can be judged by the tokens of being displeased it produces in them.

THE leaves of oaks, that are such common things, and are not observed to have, in reference to any other body, which chance or industry applies them to, any such property as that I am about to name; these leaves, I say, if when fresh, they be immersed in the water of mineral springs, impregnated with the subtle corpuscles of iron, I have several times found to turn the liquor blue or black, according to the proportion and vigor of the two ingredients.

ONE would not expect, that so dark and black a body as charcoal should be the main thing employed, not only to cleanse and brighten some metals, but to procure a clearness, and give a gloss to some transparent bodies. And yet I learned from the makers of mathematical instruments, gravers, and other artificers, that the best way they have, and which I have seen them employ, to polish their plates of brass and copper, (after they have been rubbed clean with powdered pumice-stone) is with charcoal, (which some of the more curious burn a second time, and quench in appropriated liquors,) as that, which both serves to fetch out the scratches of the pumice-stone, and itself scours without scratching, and thereby polishes very smoothly. And by the same way they may cleanse and polish the plates of horn, of which they make lanthorns, drinking-cups, &c. To which, as to the metalline plates, a gloss may be afterwards given with tripoli.

PERHAPS it will not be improper to take notice to you, *Pyrophilus*, in this place, that not only the nature of the body to be wrought upon, but some peculiar circumstances relating to it, may contribute to the effects of such experiments, as those treated of in this section. As for example, one would not expect, that water, which is so apt to run out at the chinks of wooden vessels, should, without addition, become the fittest instrument for closing them. And yet I have more than once found by trial, as I presume many tradesmen have done, that when wooden barrels or firkins, and the like vessels, by having been long kept too dry, come to have clefts and commissures, this inconvenience may be remedied by pouring water into them. For though at first the liquor quickly runs out again, yet by frequent affusions of it, the wood, especially those edges between which the water runs out, becomes so softened and plumped up, that the little intervals or chinks are, by the swelling of the neighbouring parts, closed up, and the vessel becomes stanch.

AND upon a like reason seems to depend that odd experiment, much talked of by some of our eminent English seamen, who, for the hasty stopping of a leak, that is not too great, much commend the thrusting into it a piece of powdered beef; for this being much more salt than the sea-water, that liquor pierces into the compact

compact and (in great part) dry body, and by opening the salts, and soaking into the flesh, makes the swelling beef expand it self, so as to bear stronly against the edges of the broken planks, and thereby hinders the water from flowing into the ship as it did before.

SECTION III.

I consider in the next place, that a body in association with others may be made fit for new uses, and some of them quite differing from those, that were proper to it before.

This third consideration is, in some regards, of affinity with the first, but yet is not the same, since in the former we consider the power, that one body has to act upon another, or the disposition it hath to be acted upon by it; whereas now we consider the two bodies or more, as being by conjunction qualified to act on a third body, or suffer from it, as one entire concrete, upon the account of new and emergent properties, accruing to the compound by the association of the more simple bodies, that compose it.

You will meet with store of instances, both in these essays, and other of my writings, easily applicable to the illustration of what is here delivered, and therefore it will suffice to name in this place the fewer.

He that takes notice, how flexible a metal tin is, and how dead a noise it yields, will scarce dream, that one of its uses, and that none of the despicablest, should be, to make another metal, which is less yielding, and has a less dead sound than its self, not only hard, but sonorous: and yet we see, that bell-metal, which, when cast into bells, makes a hard mixture, that sounds so loudly, is made principally, as has been already noted, by the addition of a certain proportion of tin to copper.

In the common experiment of making ink, the infusion or decoction of galls is yellowish, or reddish, and the solution of vitriol will, as the concrete participates more of iron or of copper, be either green or blueish; but from the mixture of these two liquors there will emerge an inky blackness.

That oil, that is a body so mollifying and slippery, and whose unctuosity makes its moisture so much more difficult to be wasted or destroyed, than that of water, wine, or other not tenacious liquors, should be one of the two or three main ingredients, and the only moist one of a hard and durable cement, is that, which probably you would very little expect from it: and yet, not to mention what trials of that nature I have made, because I had not time to observe the full event, a very ingenious man, much employed about costly water-works and dams, assures me, that the best way he has to join together, and, if need be, piece and mend with a close and lasting cement the pipes, that are used for subterraneal aquaducts, that are long to hold running water, is to take good clay (such as tobacco-pipes are made of,) and having dried it, and reduced it to very fine powder, and mixed good store of short flocks with it, beat it up very diligently with as much linseed-oil, as will serve to bring it to a stiff paste, almost like well kneaded dough. This paste he fashions into pipes of the length and bigness required, which, though they will be long a drying in the air, yet, when once throughly dry, are very stanch and lasting. And I remember, that before I learned this, having occasion to try divers experiments about cements, I chanced to meet with an ancient artificer, employed to keep in repair the conduits, that brought water to *London*, and in exchange of a lute or cement, that I taught him, he was forward to satisfy the curiosity I had to know what cement he employed about so important a work, and he assured me, that oil was one of the main ingredients (and the only liquid one) he employed.

He that considers, that lead is one of the most opacous and flexible bodies, that the world affords, will not easily imagine, that one of its uses should be to make up about three parts of four of a mixture transparent, and exceeding brittle; and yet this is easily performed by divers chemists (and I elsewhere mention my having often done it) in making of calcined lead, and powdered flints or sand, a brittle and diaphanous composition, called by Spagyrists Vitrum Saturni.

And this mention of glass suggests to me another instance, fit for my present purpose: for who would imagine, that such a body as the fixed salt of kaly, which, as other alkalies, that take their denomination from it, has a strong and fiery taste, and is not only readily dissoluble in water, wine, or any such liquor, but will in a short time, being but left in the air, be reduced into a liquor; who would expect, I say, that it should be of any use, much less the main of this caustick, and easily dissoluble body, to be one of the two main ingredients of substance both perfectly insipid, and indissoluble, not only in water, wine, &c. but even in aqua fortis, aqua regia, spirit of wine, quick-silver, spirit of urine, and other menstruums, some of them highly corrosive, and others extremely subtle and piercing? and yet such a mixture is usually afforded us in glass, (especially the more durable sort of it) wherein that there is actually a great proportion of alkalizate salt, I confess, I doubted, till having purposely enquired of an ingenious master of a glass-house, how much glass he usually obtained, when he put in such a quantity of sand, I found by his answer, that the glass obtained was many pounds in the hundred more than the sand, that was employed to make it: whence I gathered, (what he also affirmed) that the alkaly did not only seem (as one might suspect) to promote the fusion of the sand, but does materially and plentifully concur with it to compose the glass.

And whereas I intimated at the very beginning of this third section of this essay, that bodies, when associated, may be applied, not only to new uses, but perhaps to some, that are quite differing from those, that belong to some of the respective ingredients; this observation may

may be made good by several instances, and even by some that are very obvious, as well as by others that are not so familiar. For we may take notice, that though oil, and tallow, and other such unctuous bodies, be those, that do grease and spot linnen and woollen clothes; yet those very bodies, being skilfully associated with others, though with but a lixiviate salt and fair water, do plentifully concur to the making up of soap, by the solution of which grease is readily washed out of linnen cloths, and others, besides those, are also freed from the spots of it. But divers other instances applicable to this purpose belonging more properly to the following part of this essay, till we come thither, it may suffice, that I illustrate and confirm what hath been proposed by the single, but noble instance of Aurum fulminans. For though salt of tartar be a fixed body, and of a fixing quality, yet being skilfully associated with gold, dissolved in aqua-regis, though that be thought the fixedest, not only of metals, but of bodies; yet the gold precipitated by this fixed and incombustible salt becomes so exceeding fugitive, that by a gentler heat, than would kindle any known body in the world, it is made to fulminate like gun-powder, (but many degrees more violent than it;) and, which you will also think strange, though sulphur be a body of so quick accension as is obviously known, yet by an easy way, elsewhere to be taught you, of mixing those two only, you may, as trial hath informed us, make it (which you will easily allow to be one of the unlikeliest uses of sulphur) even by its being set on fire, to hinder the accension of this so easily kindled gold, which I have known thereby readily turned into a medicine, that some cry up for excellently diaphoretick, (though I doubt whether Aurum fulminans work not rather another way,) and which I remember I have, in a crucible, kept long in the fire without loss.

I shall only add to this third consideration this one particular, that is of too great moment to be pretermitted here, though it have been already in part taken notice of on another occasion; namely, that the effects and uses of mixtures do not only depend upon the nature of the ingredients, but may be oftentimes much varied by their proportion. And of this the mineral, which at the glass-houses they are well acquainted with, under the name of manganez, will afford us a pertinent and considerable instance. For though it be a coarse and dark mineral it self, and though being added to the materials of glass in a fuller proportion, it make the black glasses, that are sold in shops; yet not only a moderate proportion of it is used to make glass red, but, which is more remarkable, a small and due proportion of it is commonly employed to make glass the more cleer and diaphanous.

SECTION IV.

IN the fourth place I consider, that a body, by a differing preparation or management, may be fit for new, and perhaps unthought of, purposes. For the qualities of bodies depending for the most part upon the texture of the small parts they are made up of, those ways of ordering greater bodies, which do either by addition, detraction, or transposition of their component corpuscles, or by any two, or all of those ways, make any notable change of the former texture of the body, may introduce new qualities, and thereby make it fit for divers uses, for which it was not proper before.

WE see to how many several uses men, that were neither philosophers nor chemists, but for the most part illiterate tradesmen, have been able to put iron, by but varying the visible shape of certain portions of it, and connecting some of them after a peculiar manner: as is obvious in the shops of blacksmiths, lock-smiths, gun-smiths, cutlers, clock-makers, iron-mongers, and others. But to give you a more physical instance in the same metal, be pleased to take notice, how much a change, made by a natural agent, the fire, in the invisible texture of iron, does speedily alter it; when of the same bar of iron, by the help of fire and water, the artificer makes hardened iron, and iron of a temper fit for drils, and knives, and springs, and I know not how many other instruments, which require distinct tempers in the metal they are made of; that temper, which renders them fit for one use, leaving them unfit for another.

BUT we need not confine our selves to instances, wherein no new ingredient is added to, or taken from the body to be altered; it being sufficient, that the additament upon its own account do not bear so great a stroke in the change produced, but that it be principally ascribed to the way of ordering the body wrought upon; and speaking of the management of a body in this sense, (which is usual and proper enough,) I shall subjoin a few instances, of the many I might add, to make good our proposition.

THOUGH paper be one of the commonest bodies, that we use, yet there are very few, that imagine it is fit to be employed otherways than about writing, or printing, or wrapping up of other things, or about some such obvious piece of service, without dreaming, that frames for pictures, and divers fine pieces of embossed work, with other curious moveables, may, as trial has informed us, be made of it, after this or the like manner. First, soak a convenient quantity of whitish paper, that is not fine, about two or three days in water, till it be very soft; then mash it in hot water, and beat or work it in large mortars or troughs, (much after the manner used in some places to churn butter) till it be brought to a kind of thin pap, which must be laid on a sieve, without pressure, to drain away the superfluous moistness, and afterwards put into warm water, wherein a good quantity of fish, glew, or common size, has been dissolved. Being thence taken out by parcels with a spunge, it must therewith (for the spunge will dry up the superfluous moisture) be pressed into moulds of iron, or of such plaister as statuaries use, wherein having acquired the figure, which is intended to be given it, it is thence to be taken out, and permitted to dry, and is to be ſtrengthened, where need requires, with plaiſter, or grated chalk, made into pap with water, or ſome other convenient matter; and afterwards, having firſt been leiſurely dried, it is to be either painted or overlaid with foliated ſilver or gold, as the artiſt pleaſes. I may elſewhere have occaſion to mention another unlikely uſe of paper, namely, to ſtop the clefts and commiſſures of wooden inſtruments and veſſels, that are to hold water. For paper being thruſt into theſe narrow places, the firſt water, that comes to it, being ſoaked up, occaſions a forcible dilatation, which makes the ſwelling paper fill the chinks it is lodged in, according to what was lately delivered at the cloſe of the ſecond ſection.

THE ſugar-cane has been a plant well enough known to many countries and ages, who were not unacquainted with the ſweetneſs of its juice, and yet ſeem never to have made ſugar of it, for want of knowing the way of ſo ordering it, as to coagulate it into a durable, as well as delicious ſubſtance.

TOBACCO was likewiſe a noted plant in the *Weſt-Indies*, which was yet ſuffered yearly to rot and periſh like other herbs, till the induſtry of the moderns finding the way of curing it, as they call the method of ordering it, made it, by the help of mere ſkill, laſt in an improved condition for divers years, and fit to be tranſported, as it plentifully is, over all the world.

THE leaves likewiſe of indigo, which would uſeleſsly periſh like thoſe of other ſhrubs, by the mere way of ordering them, which too is rather by ſubſtraction than addition, have been long made a laſting pigment or dying ſtuff, and one of the moſt ſtaple merchandiſes, that even the *Eaſt-Indies* ſend us.

I might add the great uſe, that we are enabled to make of madder, woad, and divers other periſhable plants, by the way of ordering them; but there is one inſtance of this kind ſo conſiderable, that though I have formerly named it to another purpoſe, and though I am willing to mention but one example more of this ſort, I cannot but pitch upon this; ſince it excellently manifeſts, what may be expected from a ſkilful ordering of nature's productions, by ſhewing us, what even the ſavages of *America* have been able to perform in this kind. For though their mandioca be confeſſedly a poiſonous plant, yet without addition they make of it their caſſavi-meal, whereof not only the Indians, but alſo many Europeans make their bread, which I alſo have made ſome uſe of without diſlike. And with no addition, unleſs it be perhaps that of ſpittle, they make of the poiſonous juice of the ſame root a not unpleaſant nor ſtrengthleſs drink, which divers, even of the Engliſh, compare with our beer. And of the bread made of that caſſavi-root, they brew, in ſome of our American colonies, a liquor by the planters called Perino, which I have known, even by perſons of quality, equalled, if not preferred to wine itſelf.

THE ſhreds of leather pared away and thrown aſide by the glovers, by ſo ſlight a way of ordering them, as only the boiling them long in fair water, diſſolves them in that liquor, and reduces them with it, the decoction being ſtrained and cooled, into a kind of jelly, that they call ſize, (which may be alſo made the ſame way of cuttings of parchment, and better yet with thoſe of vellom) which is of great uſe towards the production of very differing trades: ſome of which productions are already touched upon in this book, to which I ſhall here only add, for the eaſineſs of the experiment, that the fine red ſtands, and hanging-ſhelves, are made with ground vermilion, being only tempered up with it, and laid upon wood, which being thus coloured, is, when it is dry, laid over with common varniſh, which preſerves it from wet, and gives it a gloſs.

IT would ſcarce be ſuſpected, that ſo white a body as ivory ſhould, among other uſes, be proper, without the addition of any black, or ſo much as dark coloured body, to yeild one of the deepeſt blacks that has been hitherto known; and yet many of our eminent painters count that black, which they call ivory-black, the perfecteſt, that hath been hitherto employed in their art. And this ſable may be made of ivory, without addition, only by burning it a-while in a cloſe pot; and we have made it by keeping it a-while among coals and aſhes, only wrapped in ſtore of wet paper to keep it from ſpending its denigrating ſulphur in an actual flame; (to prevent which, the pots, it is burnt in, are wont to be cloſed with lute, or otherwiſe ſufficiently ſtopped) as if artificers were acquainted with the old rule, *aduſta nigra, peruſta alba*.

AND on this occaſion I ſhall add, that this black made of ivory is ſo excellent in its kind, that I ſcarce know any thing ſo proper to make foils of, for that nobleſt ſort of gems, diamonds. And I remember, that a very ſkilful jeweller, of whom I bought ſome of thoſe ſtones, and whom I employed to ſet others for me, confeſſed to me, that burnt ivory was the thing he made uſe of, for foils to the diamonds he had a mind to ſet well.

ANOTHER inſtance there is, which I muſt by no means pretermit, now that I am endeavouring to ſhew, what the preparation or management of a body, even by illiterate tradeſmen, may do to make it fit for unlikely uſes. For one would ſcarce imagine, that from ſo groſs and foul a body, as the *inteſtinum rectum* of an ox or cow, there ſhould be obtained a tranſparent ſubſtance, more thin by far than paper; and yet of ſo great a firmneſs and toughneſs, as is ſcarce at all credible to thoſe, that have not been, as I have, convinced of it by experience. But it is certain, that ſome of our gold-beaters in *London*, and perhaps not there only, do, by cleanſing and otherwiſe preparing the above-mentioned naſty gut of an ox, obtain exceeding fine membranes,

branes, ſome of which I keep by me, that though clear and ſtrangely thin, are yet of ſuch tenacity, that when the thin plates of gold are put between them, or in their folds, the force of a man frequently ſtriking them, with a vaſt hammer made of purpoſe, almoſt as heavy as he can well lift up, does uſually, as I have ſeen with ſome wonder, attenuate and dilate the included gold, without being able to break theſe ſo fine ſkins.

THESE inſtances, *Pyrophilus*, we have hitherto produced, are almoſt all of them ſuch, as either nature herſelf, or nature aſſiſted but by tradeſmen, and other illiterate perſons, has preſented us. And therefore queſtionleſs, the power, that a ſkilful management may have to produce great changes in bodies, and thereby fit them for new uſes, will be much advanced, when they ſhall be ordered by ſuch, as are either good chemiſts, or dexterous at mechanical and mathematico-mechanical contrivances, eſpecially, if in the ſame perſons a ſkill in theſe two ſorts of knowledge ſhould concur.

THAT ſkill in mathematicks may teach a man ſo to manage natural things, as to enable him to make other uſes of them, than thoſe, that want it, will dream of, we may be perſuaded by ſeveral particulars. For we ſee, that from a bare giving to a piece of ordinary glaſs a priſmatical ſhape, that diaphanous and colourleſs body may be made to exhibit in a moment all thoſe delightful and vivid colours, for which we admire the rainbow; and though merely by giving a piece of foliated glaſs or metalline ſpeculum a concave figure, it may be made to burn ſtrongly by reflection, yet by giving a piece of glaſs a convex figure, you may qualify it to burn by refraction, and even with water fitly figured, you may readily kindle fire. For though a round and hollow ſpherical vial of pure glaſs will tranſmit the ſunbeams without making them burn, and conſequently has not of itſelf the faculty I am going to name, but ſerves chiefly to terminate the water, that is to be poured into it, and give it its due figuration; yet by filling a ſpherical vial, I have taken pleaſure ſo to unite the ſunbeams, as, when froſt and ſnow was about me, to make them burn; (and perhaps ice itſelf, if choſen free from bubbles, and conveniently ſhaped, may, as ſome incompleat trials make me hope, be made fit enough for that purpoſe.) And much more vigorous the accenſion would be, if two bare concave glaſſes of like ſhape, equal bigneſs, and truly ground, had their edges ſo joined by a cloſe frame, that the cavity contained between the inſide of the glaſſes and the frame may be filled with fair water; for by this means (the convex-ſide of each glaſs being outermoſt) the whole inſtrument (one or two of which I have ſeen in a virtuoſo's hands) will ſerve for a double convex-glaſs, which may by this means be made far larger, and more efficacious, than other burning-glaſſes of that figure, which conſiſting each of them of a ſingle piece of ſolid glaſs, are wont to be far inferior in bigneſs to ſuch hollow ones, as may be eaſily enough attained.

AND now I have named ſolid glaſs, give me leave to take hence a riſe to add, that though glaſs ſtopples are made only by giving them an almoſt conical figure, and a ſuperficies fitted by grinding, for an exquiſite contact with the inſide of the neck of a glaſs-bottle; yet this way of ordering glaſſes, which is aſcribed not to mere philoſophers, but men verſed in optical and mechanical trades, produces ſtopples much ſurpaſſing all known before; not only in this, that neither aqua fortis, nor other corroſive liquors, work upon them, but alſo in their being able to keep in even the ſubtileſt ſpirits ſo ſtrictly, that I remember having once forgot ſome ſpirit of ſal armoniack in a large bottle, which it did not near a quarter fill, when I long after (as I remember about ſeven years) came to that part of *England* again, I chanced to find this bottle in a place, where, being without an inſcription, I knew not what the contained liquor was. And taking off the glaſs-ſtopple, to diſcover by the ſcent what it might be, upon ſmelling to that ſolid body, the adherent ſpirits operated ſtrongly enough upon my noſe and eyes to make me almoſt ſtagger, and wiſh my curioſity had been more cautious.

WHAT I have further obſerved about the way of making, and the applications of this kind of glaſſes, belongs not to this place, where it would be fit to proſecute my former diſcourſe, by ſhewing you, how much the chemical management of things may alter and improve them; were it not, that it would be improper to venture upon ſo copious a ſubject in one of the ſections of an eſſay, where I ſhall therefore but point at it, without pretending to treat of it.

WE ſee, that chemiſts can out of ſome fruits, that grow wild in the hedges, and are not edible, as alſo out of the lees of ale and beer, draw an inflammable ſpirit, which, for many purpoſes (not medicinal) may be made uſe of for that of wine. We ſee, that out of the dry body of hartſhorn, as likewiſe out of the ſkull and bones of dead men, and other animals, which have been wont to be looked upon to be ſo devoid of moiſture, that men proverbially ſay, as dry as a bone, chemiſts do ordinarily, to the wonder of the ignorant, draw ſtore of ſpirit, and oil, and phlegm, as they likewiſe do from the drieſt woods. Some of them alſo, of the opacous body of lead mixed with ſand, and a few grains perhaps of metalline pigment, can make in a few hours variety of amauſes, or metalline ſtones, which, by their tranſparency and lovely colours, do pleaſingly emulate rubies, emeralds, and other native gems; about the imitation of which, I may elſewhere acquaint you with ſome of my trials.

HOW unlikely effects may be ſometimes produced by a ſlight ſpagyrical preparation of things, may ſufficiently appear by the Bolonian ſtone, from which (though one would not, upon the ſight of it, expect any ſuch matter, yet) being duly prepared by chemical calcination, it acquires that ſtrange property of ſhining in the dark a-while after it has been expoſed to the ſun, for which it is ſo juſtly admired by us, that have ſeen it, that it is judged unfit to be believed by many criticks, that have not.

AND here let me take notice to you, *Pyrophilus*, that very ſlight circumſtances in the management of a body, may ſometimes produce conſiderable and unlikely effects.

THAT ſalt, diſſolved in water, is a powerful hinderer of the congelation of that liquor, is a matter of common obſervation; neither the ſea-water, nor brine, being uſually frozen with us by ſuch froſts, as turn common water, and ſome liquors more indiſpoſed than that is, into ice. And yet ſea-ſalt, which being diſſolved in water, keeps it from freezing, being outwardly applied to water, does ſo powerfully concur with ſnow or ice to make it freeze in artificial glaciations, and is ſo neceſſary to the effect, that the ſnow or ice, without the ſalt, would not ordinarily, here in our climate, produce in a ſeaſonable time any ice at all, as I more than once purpoſely tried.

THERE is a certain powder, which by the proportion and mixture of nitre (whereof it chiefly conſiſts) with other ingredients, obtains ſo odd a texture, that if putting it into a crucible, you ſhould place that upon the coals, as is uſually done in other fluxes, the powder would blow up, or take fire with violence enough, and perhaps not without ſome danger; and yet, if inſtead of kindling this power from the bottom upwards, you kindle it from the top downwards, there will be no danger in it, but it will make a powerful flux for the reduction of metalline powders mixed with it into a body.

SECTION V.

IN the fifth and laſt place I conſider, that the generality of effects to be performed, being not produced by one ſingle and unaſſiſted production, either of nature or of art, but requiring the concurrence of more; he, that knows not the nature or properties of all the other bodies, wherewith that, on which the experiment propoſed is actually, or may be uſefully aſſociated, or otherwiſe imployed, can hardly diſcern all the effects the experiment may poſſibly concur to produce. For, whereas many inventions or operations conſiſt, as it were, of ſeveral parts, and require, as it were, diſtinct actions; a body, that ſeems uſeleſs to the main and ultimate effect, may uſefully concur to the performance of ſome intermediate or ſubordinate part of the operation, (by being requiſite to which, it may be of uſe to the experiment conſidered in the groſs, though not to each diſtinct part of it.)

THOUGH ſpirit of wine will ſcarce (if at all) even in a very long time draw a red tincture out of the flowers of ſulphur, yet, when they have been opened, by having been fluxed together with an equal weight of ſalt of tartar, we have found, that they will in a few minutes, and in a gentle heat, give, in thoroughly deflegmed ſpirit of wine, a tincture or ſolution as red as blood; which being freed from the ſuperfluous menſtruum, will afford us a balſam much finer than that vulgar one, which is wont to be made of the ſame flowers diſſolved in oil of turpentine.

THAT ſuch amalgams of gold and mercury, as goldſmiths are wont to gild ſilver with, cannot by ordinary ways be made to adhere either to iron or ſteel, is a thing ſo well known among gunſmiths, and ſuch artificers, as work upon iron, that when I inquired of ſeveral of them (as well Dutch as Engliſh) whether they could gild iron with water-gold, (as they call that way of gilding by the help of quickſilver,) they judged it a thing not to be done: and yet I know a very ingenious tradeſman, who was able to perform it, but not (that we may apply this experiment to our preſent purpoſe) without the the aſſiſtance of another body, which was to perform one part before the amalgam could perform the other. The artificer's way was to coat (if I may ſo ſpeak) the iron or ſteel to be gilt, with a coat of copper, to which purpoſe he uſed diſtilled liquors tempered with other ingredients, wherein the iron was to be immerſed with great warineſs and dexterity; for otherwiſe, not only the trial would not ſucceed, but oftentimes the iron would be ſpoiled. To obviate which inconveniencies, there occurred another way of caſing the iron with copper, namely, by diſſolving very good vitriol, that has copper in it (for it is not every vitriol, that is fit for the purpoſe) in warm water, till the liquor be ſatiated with vitriol, and immerſing ſeveral times into this ſolution the iron, firſt ſcoured till it be bright, and ſuffering it each time to dry of itſelf; for this immerſion being repeated often enough, there will precipitate upon the iron enough of the cupreous parts of the diſſolved vitriol, to fill all its ſuperficial pores with particles of copper. So that by this ſafe, cheap, and eaſy way, having, as it were, overlaid your iron with copper, you may afterwards gild it as copper with the above-mentioned amalgam, which will adhere to copper, not to iron.

BUT here we muſt not omit an obſervation very conſiderable to our preſent ſcope, namely, that though the ſeveral parts of an experiment or a proceſs may in moſt caſes, each of them be purely phyſical, or chemical, &c. yet in divers other caſes, it may far more uſefully be ſo ordered, that one part of it may be phyſical, (taking here that term in contradiſtinction to ſubordinate parts of learning) and ſeveral, or each of the reſt may belong to other arts, as one may be chemical, and another ſtatical, another mechanical, another hydroſtatical, &c. and by ſuch a concurrence of differing parts of knowledge to the ſame operation or production, I doubt not but many things may be performed, that have not yet been attempted, nor ſo much as thought of. For he, that has ſkill but in one of theſe ſingle parts of learning, muſt needs have his attempts as well as his knowledge much ſtraitened, by confining himſelf to operate by ſuch means and inſtrument, as are within the

the compaſs of his own art; which, aſſiſted by others, may bear a good part in the performance of diverſe conſiderable things, which it is by its ſelf very inſufficient to accompliſh.

Of this we may take notice of ſome inſtances in the productions, that art and nature have preſented us with already; for not only handicraft trades, as we have formerly noted, do many of them aſſiſt each other in their operations, but even thoſe arts, that are counted ingenious, have ſometimes need or uſe both of the ſervice of the more mechanical trades, and of mutual aſſiſtance among themſelves. The maſters of captoptricks know very well what would be the properties of ſpherical, cylindrical, and other ſpecula; but to procure ſuch ſpecula, you muſt have recourſe to the chemiſt, or the founder, whoſe part it is by artificial mixtures of metals and minerals, and by mechanical contrivances, to caſt bodies, that give a more ſincere and vivid reflection, than the ſingle metals would do, and to give them withal that curious poliſh, for which the metalliſts and chemiſts are beholden to ſmiths, ſtone-cutters, watch-makers, or other handicrafts men.

Another eminent example to the ſame purpoſe may be taken from the conſideration of organs uſed in churches. For to deviſe the rules of making them well, there is firſt requiſite no ſmall ſkill in the ſpeculative part or theory of muſick: next, he, that would make the inſtrument well, muſt know how to chooſe wood proper for that purpoſe, (moſt woods being unfit for it,) how to ſeaſon it, and how to diſcern, whether it be duely ſeaſoned, and otherwiſe well conditioned. To excavate and faſhion the pipes, and other parts of the inſtrument, that are made of this wood, there is uſe of the turner's and joiner's crafts. It is often needful alſo, that the organ-maker be ſkilled in the effects of metals, and perhaps their mixtures; and the ways of caſting them, in order to the making of his pipes of a ſonorous matter, and to the giving them a due ſhape, and other deſirable qualifications. I might here borrow further inſtances from bells, lutes, harps, and other muſical inſtruments; but I haſten to examples of another kind.

He that has never ſo attentively conſidered the nature of ſalt-petre or of brimſtone apart, ſhall never be able to make the conſiderableſt uſes of either of them, till he ſkilfully aſſociate them to one another, and incorporate them into that wonderful body, called gunpowder, which will afford us an inſtance fit enough to explicate what we have been ſaying: for conſiſting of three differing ingredients, nitre, brimſtone, and charcoal, though neither of theſe be ſufficient, *in omni genere*, (as they ſpeak in the ſchools,) yet each of them is very uſeful by being ſufficient *in ſuo genere*, and really concurs to the effect produced by them all, as you may elſewhere find more particuly declared.

He muſt remain ignorant of another conſiderable uſe of ſulphur, that is unacquainted with ſome properties of common oil and calcined alabaſter. For artiſts have a way of making molds, wherein to caſt off the impreſſion of medals, and other works emboſſed on metals, which, though the effects of it ſeem ſtrange to thoſe, that know not how they are produced, they eaſily thus perform. They make about the emboſſed work, whoſe impreſſion they deſire to have, a little border or ledge of clay, to hinder the melted ſulphur to be poured on it, from running over; then they lightly (but very carefully) with a pencil or feather anoint the metalline work with oil, to hinder the ſulphur from adhering to it; then they melt good brimſtone in any convenient pot, (which they cover well to prevent its taking fire) and whilſt it is hot, they pour it gently upon the emboſſed metal, all whoſe extances will make perfect impreſſions on the lower ſurface of the thus melted brimſtone, which ought to be poured on in a conſiderable quantity, that the molds thus made may prove the ſtronger. About the edge of this mold they make a little rim or border of clay as before; and lightly anointing both all the ſurface of the mold, and the inſide of the clay with oil, (which if it be too copious, is, as we have tried, apt to prejudice the accurateneſs of the impreſſion,) they pour in by degrees to the thickneſs of about a fourth of an inch of that mixture I formerly mentioned (in the eighth eſſay,) to be made of recently calcined alabaſter, ſtirred and incorporated with ſuch a quantity of fair water, as may ſuffice to bring it to the conſiſtence of the thicker ſort of honey. And this mixture in about a quarter of an hour growing hard, and then being taken out of the mold (to which the oil hinders it from ſticking) will, if the work have been dexterouſly done, and the mixture before affuſion carefully freed from bubbles, perfectly exhibit the ſhape and dimenſions of the work emboſſed upon the metalline pattern. And by this way in a few minutes have we ſometimes caſt off a coin, a medal, and ſometimes too a whole landſcape, without any trouble, and not without ſome delight.

And here, *Pyrophilus*, let me perform what I lately intimated an intention of, by now taking notice to you in this fifth ſection of this eſſay (of what I had not long ſince occaſion to obſerve in a former part of it,) that you may oftentimes find ſuch particular bodies conducive to the main effect of an operation or experiment, by performing ſome ſubordinate part or office in it, as yet may ſeem nothing at all of kin to the ultimate effect promiſed by the perfected experiment.

That aqua fortis, that ſo greedily corrodes and devours ſilver and braſs, ſhould eminently conduce to the real ſilvering over of the latter metal by the former, is that, which few goldſmiths, or even chemiſts would judge probable. And yet this fretting liquor performs a principal part in that ingenious way of ſilvering over braſs and copper, which is more applauded than known. For firſt, aqua fortis ſerves very well to make clean ſuch emboſſed or otherwiſe uneven pieces of metal, whoſe inequality hinders us from being able to cleanſe their little cavities with Tripoli, or thoſe other

powders

powders commonly used to scour brass: whereas if such bodies be lightly washed over with aqua fortis, and immediately thrown into fair water, the foulness may be fretted off, and the work not disfigured. And this is esteemed the best way of scouring such metalline pieces of work by the best maker of mathematical instruments, that I have met with. And I the rather mention it to you, *Pyrophilus*, because that though it be not always requisite to our experiment of silvering, (for many pieces of brazen work may well enough be made clean after the ordinary manner) yet divers trials have assured us, that the scouring of the brass and copper is necessary to the success of this experiment; probably, because any grease or filth remaining upon the surface of the metal is sufficient to keep out those little parts of dissolved silver, which ought to lodge themselves so thick in the pores of the metal, as to seem one continued silvered body.

The remaining part of this operation may be thus performed. The metal to be wrought upon being made very clean, you must dissolve good silver (the finer the better) in aqua-fortis in a broad-bottomed vessel of glass, or at least of glazed earth; and having, over a chafing-dish of coals, or with some such heat, evaporated away all the aqua-fortis, you must upon the remaining dry calx pour of water five or six times its quantity, or as much as will be needful perfectly to dissolve it. This water with the like heat must be forced away as the former menstruum, and the like quantity of fresh water must be poured on, and evaporated quite away the second time, and, if need be, the third time, toward the latter end making the fire so strong, as to leave a perfectly dry calx; which, if your silver has been good, will be of a good white, and will by these operations be competently freed from the stinking and fretting spirits of the aqua fortis. Of this calx you must take one part, and about as much (in quantity, not in weight) of common salt, and as much of crystals of tartar, (or at least powder of good white tartar) as of either of the former ingredients; which, like this, ought to be finely beaten, and these three powders being exquisitely mixed, you must plunge the scoured brass, to be silvered over, into fair water; and then taking up as often as need requires, with your wet fingers, some of the newly mentioned mixture, you must rub it on well, till you find every little cavity of the metal sufficiently silvered over; remembring, that if you would have it richly done, you must rub in more of the powder. And last of all, you must wash well your silvered metal in fair water, and rub it very well and hard with a dry cloath, that it may appear smooth and bright. And this way of silvering, though it be presently and cheaply performed without quick-silver, yet may be made to last some years, as experience has partly informed me, and may be easily renewed, when the silvering begins to decay or wear off.

And here, *Pyrophilus*, it will not be improper to give you this advertisement, that we ought not to conclude, as we are very prone to do, that such an use is not to be expected, or endeavoured to be obtained from such a thing, because we see the like use to be made of things, that are thought to be of a quite differing nature from that we consider, or perhaps quite contrary to it: for in many cases, as there are more ways than one, or even than a few, to bring to pass a thing proposed; so among the various instruments, that may be employed the same purpose, some may exceedingly differ between themselves as to other qualities, and yet agree in that, which is requisite and sufficient for the performance of the thing designed. As though, for instance, rosin and sal armoniack be differing in colour, smell, taste, weight, hardness, &c. though the one be a vegetable concrete juice, the other an aggregate of urinous, fuliginous, and marine salts; the one readily dissoluble in water, the other not dissoluble in that liquor, but in oil; and though there be I know not how many other differences between them; yet either of them single may be, and is, usefully employed for the tinning of brass and copper-vessels, each of them being endowed with a fitness to make tin stick to those metals, as I elsewhere more particularly declare. Thus, though water, sand, and tin, are bodies in other respects very unlike, yet the two latter are found fit to make hour-glasses, as well as the first; though that alone, as is presumed, were for many ages employed by the ancients for that purpose.

To the foregoing advertisement I shall annex another, that may seem very differing from it, but yet is no less true; namely, that we are not always to suppose, that because a natural body has such an use on some occasions, the same body cannot on other occasions be employed to uses, that seem of a quite differing, and perhaps of an opposite nature.

This I conceive may be done principally by these two ways. First, by the differing constitutions of the several bodies the same agent works upon; as when the heat of the sun melts wax and hardens clay; and the same spirit of vinegar, which on filings of copper will by digestion obtain an abominable taste, will upon filings of lead acquire, by the same way, a very great sweetness: and spirit of salt, that will dissolve copper and iron, as aqua-fortis also does, will yet precipitate silver dissolved in that menstruum. And to this first way I shall subjoin the second, which is, that such a parcel of matter, as is wont to be considered as one and the same body, may contain in it parts of very differing natures, upon whose account its operations may be diversified. Thus when we calcine some unripe minerals with nitre, the inflammable parts of the nitre do burn up and dissipate into smoke the volatile and combustible parts of the mineral; but by virtue of the remaining alkali of the nitre, several other parts of the mineral are made far more fixed and capable of enduring the fire, than they were before. So sulphur has in it some parts, that make it more readily inflammable than even nitre or oil; and yet it abounds with acid and vitriolate particles, that are not inflam-

inflammable themselves, and much resist the accension of flame in divers other bodies. And accordingly, though in matches used in tinder-boxes to take fire readily, the kindled brimstone acts upon the shivers of wood, whose ends were crusted over with it, as an ordinary flame; yet the same burning body, by virtue of its acid parts, works in another capacity, than that of a common flame upon some metals, especially iron, and likewise upon the leaves of red roses, which its fumes turn white.

I could, if it were needful, propose in this place, sundry other instances of the differing actions of the differing parts of a body, and could likewise subjoin other cases, than I have yet mentioned, wherein bodies may be applied to uses, that many would be unapt to expect from them. But judging it more convenient to reserve those for other places, especially in the last essay, I shall conclude this with the two following advertisements.

THE first is, that I have in all this discourse purposely forborn to treat of the medicinal uses of things, because my scope in the volume, whereof this essay is a part, obliged me so to do. But yet I am sensible, and would have you so too, that hereby I have forborn to employ a multitude of particulars, that would have much enriched this treatise. For there is a great number of bodies, both natural and factitious, that being employed as medicines for human bodies, have there very various and sometimes seemingly repugnant operations, many of which would serve to illustrate and confirm sundry passages of this essay. Thus rhubarb, whether taken in substance or infusion, does by virtue of its differing parts, first purge, and then bind. Spirit of wine taken inwardly exceedingly heats the body; whereas outwardly it is employed to appease the heat caused by some hot humours and inflammations. Mercury taken inwardly, crude as it is, has often, though not always, proved an effectual and harmless medicine in worms, and some other distempers, even to children and women in labour: but the same mercury rarified into fumes, (which yet may be condensed again into running mercury,) and in that form taken into the body, does too often cause vehement and dangerous commotions in the juices of the body, as excessive salivations, fluxes, &c. declare. And he, that shall attentively consider the various operations of that one mineral antimony, and the not only differing, but oftentimes contrary effects, that it produces, according to the complexions and dispositions of the taker's body, and the preparation of the mineral itself, will not, I presume, stick to allow me, that the medicinal uses of things, if I had not thought fit to decline them in this essay, might have much increased the number of instances it contains; the effects of other bodies upon those of men being no less proper instances of nature's ways of working, than the changes they produce, when they work only upon one another.

THE second advertisement, wherewith I shall conclude this essay, is, that though what I have hitherto discoursed, hath almost solely related to the neglected uses of particular natural bodies; yet I would not have you thence take occasion to imagine, that there are not other natural things, whereof divers uses may be made, that men have hitherto either ignored, or overseen. By other natural things I mean the differing states of matter, or of bodies, (such as rarity and density, fluidity and firmness, putrefaction and fermentation, may seem to be,) as also the more operative qualities, such as heat, cold, gravity, &c. the laws of local motion among the parts of matter, and the present fabrick of the universe, and especially that of our terrestrial globe and its effluviums; to which might be added other things in nature, that are not properly bodies in the usual sense of that word, but may be called things corporeal as they belong to bodies, and intirely depend on them. In favour of this advertisement it were easy for me to suggest to you such a multitude of particulars, that reserving some few for the last essay, I here purposely forbear to mention any at all, to avoid being enticed or engaged to enter upon a subject, that could not be otherwise than very lamely handled, without enormously swelling an essay, that does already exceed its just dimensions.

T R A C T S.

OF

A Difcovery of the ADMIRABLE RAREFACTION of the AIR.

New Obfervations about the DURATION of the SPRING of the AIR.

New Experiments touching the CONDENSATION of the AIR by mere COLD; and its COMPRESSION without Mechanical engines.

The admirably DIFFERING EXTENSION of the fame Quantity of AIR rarified and compreffed.

ADVERTISEMENT.

THE Author of the following papers fuppofeth his readers to have learned, either from the books he hath publifhed, or from what hath been borrowed thence by other writers, the ftructure and more familiar ufes of a pneumatical engine of his, metioned by feveral authors under the name of Machina Boyliana; with whofe defcription therefore thofe are defired to acquaint themfelves, that fhall think it worth the while to underftand, as well as read, the following papers; about which it might be further taken notice of, that the firft of them was indeed written to a learned friend, though his name be not now annexed (for certain reafons;) prefently after which the three others were thought fit to be fubjoined. As for the omitting of the compliments and forms, ufual at the clofe of epiftles, the author did it, as well to fpare the reader, as himfelf; who hopes he may be excufed, if the tranfitions from one difcourfe to another, and even the ftile and method of them, be not fo fmooth and regular, in regard the enfuing writings were traced, when he was afflicted with a great fit of ficknefs, that kept him from fo much as once reading over himfelf, what he had indited.

A DISCOVERY OF THE ADMIRABLE RAREFACTION OF AIR, (EVEN WITHOUT HEAT) IMPARTED In a LETTER to a FRIEND.

DO not imagine, Sir, that I did at all wonder to see you yesternight so much admire, to hear me talk with so much seeming extravagancy about the rarefaction and condensation of the air; for I confess, that I did deliver something on that occasion, that might easily, at first, sight appear so near impossible, as to be utterly improbable.

And though you were pleased, even on such an occasion, to express a very favourable opinion of my veracity, yet thinking it fit, that such an obligation should not divert, but engage me, to endeavour to justify you to yourself, by confirming what I said to you; I have already sought and found among papers, many years since laid aside, some, that will enable me to make good more, than what the diffidence of my memory allowed me to say in the very boldest part of my yesternight's discourse. For now that I luckily find not only the originals of the relations, whereof this paper contains copies, but that my engine is in good order; I am so far qualified to countenance a discourse, wherein I kept somewhat within compass, that though it will perhaps cost me much pains and trouble, to make *ex tempore* experiments fully equal to the enclosed; yet if any just doubt should require it, I presume, I can make ocular proof of, at least, as much, as I last night told you.

And now it is time, after having contrary to my custom, raised in you a high expectation, that I endeavour in some measure to answer it, which I hope I shall the more easily do, because the agreement, you have often had occasion to observe between the relations registered in my adversaria, and the phænomena of the experiments they describe, will, I presume, make it needless to persuade you, that the ensuing trials, being transcribed thence, may be safely credited. Wherefore I shall proceed to annex them, as soon as I have premised a few historical lines, by way of manuduction to them.

It is now many years since, that having a desire to reduce the air to a degree of rarefaction, that appeared to be considerable, upon surer grounds than slight conjectures, I attempted to do it by the help of heat, and particularly by that of an œolipile, which I have mentioned in another tract *: but finding, that the diligent *Mersennus* had, if there be no mistake in his account, been able to rarify air that way, full as much, or more than I could, I betook me to try, whether I could not, by the spring of the air (without heat) procure a greater expansion of it? I found (as I have long since elsewhere † related) that in the pneumatical engine, which has been since called Machina Boyliana, I could increase the expansion of air, till the body attained to about one hundred fifty-two times its former and usual dimensions. But this expansion, though it were above twice as great as the utmost procured by *Mersennus*, did not yet satisfy me, but put me, (according to what I there intimate) upon another contrivance, which though put in practice eight or nine years ago, (as the date of one of the trials may inform you) had the relation of its successes laid aside among those of others, made in the same engine, which yet lie by me unpublished. So that I may now proceed to give you the transcripts of the trials themselves, as they were hastily and inelegantly, but very faithfully, set down among my Pneumatical Collections. And this I am ready to do, as soon as I shall have intimated to you, that in that noble collection of experiments, that has about two years since appeared in publick, as the first-fruits of the justly famous Florentine Academy, I find, that those virtuosi had, according to their sagacity, so advanced the extent of the air, as without the help of heat to bring the dilatation to exceed one hundred seventy-three times its former dimensions; and that, which made their improvement the more considerable, and consequently

* New Physico-Mechanical Experiments, Exper. VI. † Ibid.

quently the more worthy of them, is, that they procured this great rarefaction, as well as I had done mine, by the air's own spring; and had surpassed without the help of my engine, what I was then at first able to do by the conveniencies that it afforded me. Whereupon, remembering what I had performed in that kind several years before, I sought among my papers for the trials I had then made, and found those notes, whereof, I now, at length, think it high time to give you the promised copies in the following terms.

EXPERIMENT I.

We took a round glass-egg (as they call them) of clear metal furnished with a pipe, or shank, of some inches in length; this we filled with water, and conveyed both it and a vial with water in it, into a receiver of a convenient size, and by pumping the air out of it, we made the bubbles both in the egg and the vial to disclose themselves in great numbers; so as to make the liquor in the glass-egg seem to boil, and to make all that was in the shank really to run over. When we thought the water was sufficiently freed from air, which it was not quickly brought to be, we took out the glasses and filled up the pipe of the egg with water taken out of the vial, and inverted it into more of the same water, in such manner, that the egg was quite full, shank and all, excepting a small bubble of air, that we purposely left to gain the top of the egg; where, the glass being transparent, with a pair of compasses we measured as accurately as we could, and found it to be a tenth, and less than two centesms of an inch. Then putting the glasses again into a receiver, we set the pump at work, and the little bubble, after a while, began to expand itself, which when it had once done, it did at each suck strangely increase, till at length it drove all the water out of the round part of the glass. And lest it might be objected, that it was only the subsiding of the water upon the withdrawing of the outward air, that before kept it up to the top of the glass, we caused the pumping to be so continued, till the expanded air had several times driven the water in the pipe of the egg, a pretty way beneath the level of the external and surrounding water in the other glass. This done, we let in the air by degrees, with a design to observe, what bubble we should find at the top of the egg, when the water should be again driven up into its cavity. But the expanded air had forced over so much water, that there remained not enough to fill the globulous part of the egg: wherefore we tried the experiment again, and when we had proceeded thus far, we compared the above-mentioned diameter of the small bubble, with that of the spherical part of the glass, which we took with a pair of Callaper compasses: and though we found it to be somewhat more than 20 times as great, yet being willing rather to disfavour than flatter the experiment, we supposed the two diameters to be as 1 to 20, and consequently, since, as *Euclid* demonstrates, the proportion between spheres is triplicate to that of the diameters, El. 12. Propos. ult. and in our case, the cube of the lesser diameter being one, is also but one, the cube of 20, the greater diameter, must be 8000; and so the air appears to have, by expanding itself, acquired a place 8000 times as big as it possessed before. Nor was it overseen by us, that the globulous part of such glasses as we used is scarce ever made spherical. But not only I, but Dr. *Wallis*, who was pleased to assist at the experiment, concluded, that the cavity of the shank, which the expanded air drove the water from, but which, we did not compute, would make abundant compensation for the two above-mentioned particulars. After this, for further satisfaction, we took water, laboriously freed from air, and putting it into the same glass-egg, we inverted it as before, but left not any bubble in it. This we did, that in case we could make the water subside, the experiment might prevent a suspicion, that some air latitant in the water might increase the bubble that was formerly left in it; having then exhausted the receiver as much as before, and if we mistook not, more, the water in the egg did not all subside; but at length, with obstinate pumping, a bubble disclosed itself, and drove all the water clear out of the round part of the glass; and though by reason of some small leaks, that we could not find or stop, we were not able, as before, to make the expanded air depress the water in the shank, beneath the surface of the external water, yet we wanted very little of it; and then out of weariness giving over, we found, that when the water was impelled up again into the egg, there was at the top of it a bubble, whose diameter we measured as faithfully as we could, and found it to be to the diameter of the globular part of the glass, as 1 to 14; so that, though the little bubble had been a perfect sphere, yet spheres being, as was lately noted, in triplicate proportions to their diameters, the bubble when expanded, must have been 2744 times as big as the bubble unexpanded. But Dr. *Wallis*, who will be allowed to be a very competent judge in these matters, observing (what we all took notice of) the great thinness of the bubble, positively and constantly affirmed, that he could not estimate it to be at most any bigger than the third part of a perfect sphere of that diameter; by which estimate the expansion of the bubble must have reached to 8232 times its natural dimensions.

N.B. By letting in water into the receiver, as much as it would admit, we found, that by reason of some secret leak, we had not been able so to exhaust it, but that there remained some air.

EXPERIMENT II.

A SMALL and almost inconspicuous bubble June 2, 1662. expanded itself, when the ambient air was pretty well exhausted, to more than 10,000 times its former extent. The manner thus: we took a small bolt-head, blown by a lamp, which

which contained in all about 80 grains of water, and inverting the ſmall neck into a jar of water, it was included in the receiver, and the ambient air being exhauſted, ſtore of bubbles roſe out of the water, and expanding itſelf, quickly drove all the water out of the bolt-head. Then re-admitting the outward air, the bolt-head was preſently almoſt filled, and all the expanded air ſhrunk into a bubble, little bigger than a ſmall pin's head; then taking the bolt-head out of the water, and inverting it, that the bubble might get out at the neck, we carefully filled it up with the water, that had been freed from air, and then inverting it as before into the jar with water, we again included it, and after ſome exſuctions found, that there was gotten out of the water into the neck a very conſpicuous bubble, which, upon the admitting of the air, ſhrunk almoſt into an inviſible one, and aſcended into the head of the glaſs. Then again exhauſting the receiver very well, we found it expand itſelf, ſo as to fill all the capacity of the bolt-head, and to drive out almoſt all the water. And upon the re-admitting of the air, it again ſhrunk into a bubble, whoſe diameter (according to our beſt eſtimate) was not bigger than one two and twentieth part of the diameter of the head of the above-mentioned glaſs; ſo that to fill the whole cavity of the head only, it expanded itſelf 10648 times: but becauſe it filled likewiſe the greateſt part of the neck, we found by weighing the water that filled that part, and the water that filled the head, that the capacity of that part of the neck, was almoſt a third of the capacity of the head, being as 141 to 481: if therefore 481, the capacity of the head, contained it 10648 times; 141, the capacity of the neck, muſt contain it 3121 $\frac{167}{481}$ times; ſo that in all, the ſmall bubble of air was expanded to above 13769 times its former bulk.

THE diameter of the ſmall bubble retracted was $\frac{1}{27}$ of an inch.

THE diameter of the outſide of the head of the glaſs was $\frac{29}{36}$ of an inch.

THE water that filled the head only weighed 60 $\frac{1}{2}$ grains.

THE water, that filled the head, and as much of the neck as the air had before expanded itſelf into, weighed 78 $\frac{1}{8}$ grains; ſo that that part of the neck weighed 17 $\frac{5}{8}$ grains.

THE bolt-head itſelf weighed 15 grains.

I might have ſet down this ſecond experiment unaccompanied either with the firſt, or with that I am going to ſubjoin; becauſe the expanſion produced by neither of them was, at leaſt by meaſure, ſo vaſt, as that produced by the trial newly mentioned: but this was ſo ſtupendous, that I thought it not ſo fit to preſent it to you by itſelf alone, but rather accompanied with other experiments, the leaſt proſperous of which produced a dilatation of air ſufficient for my preſent purpoſe, and ſuch as may not a little confirm, that what is recited in the ſecond experiment, was neither a lucky chance, or miſtake. And that may be enough for my preſent purpoſe; for as for the little abatements, that ſome will perhaps think fit to be made upon the ſcore of the unequal thickneſs of glaſs or ſome ſuch circumſtances, they are not conſiderable enough to deſerve to be now ſolicitouſly debated, nor to hinder the expanſion, that muſt be granted from proving what they are alledged for: wherefore I will proceed to what follows.

EXPERIMENT III.

WE tried this experiment again, and found a ſmall bubble, much about $\frac{1}{12}$ of an inch in diameter, filled not only the ball at the end of the bolt-head (which was 1 $\frac{1}{2}$ of an inch in diameter) but the whole neck, which contained near as much water as the head, and beat down the ſurface of the water within the pipe, much below that of the water without the pipe.

THESE experiments already found among my old papers will, I hope, without ſeeking for more, ſuffice to manifeſt, that the expanſion, which the air may be reduced to without heat, is indeed admirable; for if we make an eſtimate of it but according to the experiment, which had the moſt moderate ſucceſs, it appeared, that one ſpace poſſeſſed, though not adæquately filled, by a portion of air, may have its air extended to at leaſt 2744 ſpaces equal to it; I ſay, at leaſt, becauſe very probably it was above twice as great: and if we make our eſtimate according to the moſt proſperous of our trials, we muſt allow the air to be rarefiable at leaſt 13000 times; I ſay again at leaſt, becauſe I am not ſure, that in that trial it was reduced (not fully, though perhaps very near) to the uttermoſt degree of rarefaction attainable in our engine: ſo that I preſume you will now grant, that I ſpoke warily and much within compaſs, when I mentioned but an expanſion from one to a thouſand.

AND now having performed the promiſe I made you, it remains only, that I take notice of the requeſt, that you made me, about communicating theſe experiments to the curious. But this deſire of yours is oppoſed by no ſmall inconveniencies, that would reſiſt my compliance with it. For it would oblige me by tearing out theſe papers, to diſmember a collection long ago in making, and wherein they were placed to be much otherwiſe diſpoſed of, and not only make a great gap in it, but ſtrip or deprive it of ſome things, that were the likelieſt to recommend it. Beſides that theſe appearing before the reſt are odd enough to make theſe ſeem far leſs uncommon, than perhaps otherwiſe they would. Yet all this notwithſtanding, I find it uneaſy to refuſe, what you, and thoſe friends, that concur with you on this occaſion, deſire, that if after having once more peruſed theſe papers, you perſiſt in the ſame earneſtneſs you expreſſed yeſterday, when you had not yet ſeen them, I ſhall not refuſe you the diſpoſal of them, both for the reaſon now given, and becauſe I have been informed as well by you as by other means, that the rarefaction of the air is at preſent the ſubject, that buſies the diſquiſitions of ſeveral eminent virtuoſi, both domeſtick and foreign, to whom I pay so much respect, that I shall think it a happiness, if it may be acceptable to them, not only because it will be seasonable, but because, that though the engine, that most of the attempts were made in, has not been thought altogether barren, yet these trials will probably pass for one of the least inconsiderable productions of it: and these two services I hope this short writing may do several ingenious readers; the one, that it will invite and accustom them to take notice of, and consider the great subtlety of nature, and the scarce imaginable smallness of those aerial instruments, that she employs even about visible operations: the other, that these relations will excite the more curious and piercing wits to debate, and I hope help them to solve the two problems here proposed to them; what figures and motions may be assigned to the particles of the air, to explicate it's so wonderful rarefiableness, and that perhaps without quite losing its durable spring, and how the air comes to be rarefiable so many times more without heat, than hitherto we have found it to be by heat. To which might be added, as a third, what might be reasonably conjectured about that part of the cavity of an exactly closed glass, where, though the eye discovers no visible substance harboured in it, it appears not, that the common air does adæquately fill so much as the ten thousandth part?

NEW OBSERVATIONS ABOUT THE DURATION OF THE SPRING OF EXPANDED AIR,

(Subjoined by way of APPENDIX to the foregoing EPISTLE.)

FORASMUCH as reviewing the former paper about the Rarefaction of the air, I took notice in the close of it of an expression (viz. and that perhaps without quite losing its durable spring) which I fear may, to some readers, seem to need explication; it will not be improper on this occasion to subjoin something by way of appendix about it.

FIRST, then the reason, why, in this short intimation, I thought fit to employ the diffident term *perhaps*, was, because I had not (nor yet have) been taught by trial, whether and how far the utmost expansion of the air actually produced in my engine, or otherwise procurable, and its retaining a sensible spring, are consistent. I express my self thus, to insinate, that I thought of other instruments and methods, whereby the dilatation of the air may not improbably be measured and promoted; as by making the Torricellian experiment in a glass with a very capacious head or globulous part, and applying the aerial particles, that will ascend out of the subsiding mercury together with a bubble of other air, if it be needful, to the use we have been speaking. Something also may be done, to some purpose, with very fine and large fish-bladders; but I shall not insist on these, or the other expedients, that came into my thoughts, contenting my self to have intimated, and thereby acknowledged, that there may be other means besides the Machina Boyliana, to bring air to a very great expansion. But whether any of them will surpass what has been actually attained in that engine, time must declare; till when, we shall be content to make use of the experiments it has already actually furnished us with.

WHEREFORE to come the second or other remaining part of it; whereas in the mentioning of the spring of the expanded air, I employed the attribute of durable, you may easily gather the reason, from what I am now going to annex.

I had observed, not without some wonder, in the enquirers into the nature of the air, that they have not, that I know of, so much as attempted to discover, whether the air, either in the utmost, or in the intermediate degrees we can bring it to, does retain a constant and durable elasticity?

FOR, first, men have not determined, whether a portion of our common air being exactly shut up in an hermetically sealed glass, or some other exactly closed vessel, will constantly and

and uniformly, for a moderate time at leaſt, retain the degree of elaſticity it had when it was ſhut up: And whether it will not ſometimes vary its preſſure, as we ſee, that the atmoſpherical (though I think upon peculiar grounds) is, by the help of our baroſcopes, obſerved to do? Next, it does not appear, whether included air, in caſe it retain an uniform elaſticity for a moderate time, will retain it for a very long one. Nay, whether it would not at length come not to have a weaker ſpring, but perhaps to have no ſenſible ſpring at all, as we ſee in happen it ſword-blades and divers other ſpringy bodies, which, after having ſtood too long bent, will continue ſo, and loſe their former power of ſelf-reſtitution, as they call it.

THIRDLY, Men have not yet determined any thing about the degrees of the air's elaſticity, whether the durableneſs and uniformity, or varying of its ſtrength, may not depend upon the differing degree it had, when it was firſt ſhut up.

FOURTHLY, Much leſs have we yet attempted to diſcover, whether the ſpring of an encloſed portion of air may be ſometimes weakened, and ſometimes ſtrengthened by the changes, as to gravity, of the outward atmoſpherical air, the new and full moon? To which I might add divers other external accidents, which, as yet, we ſcarce ſuſpect. And to theſe I might add ſome other doubts and enquiries, that may not be impertinently ſuggeſted, but here would, I fear, paſs for a digreſſion.

WHEREFORE I ſhall proceed to tell you, that having taking notice of it, as an omiſſion among the inquirers into the nature of the air, in whoſe negligence I was too long a ſharer, that we have not, that I know of, ſo much as attempted to diſcover this itſelf: whether the air, either in the utmoſt, or in the intermediate degrees of rarefaction we might bring it to, would for a conſiderably long time retain its elaſticity, or at leaſt, ſome determinate degree of it, or loſe it by determinate and regular decrements, I thought fit to make ſome trials about this matter, but cannot brag of the ſucceſs of my intentions, having been hindered either by want of inſtruments, or by removes, or by ſickneſs, or by unlucky accidents, or by one unwelcome thing or other, from accompliſhing what I had chiefly deſigned, and partly alſo made ſome progreſs in it; but yet to give you ſome hints, as well as ſome occaſion to more proſperous experiments, I ſhall not ſtick to annex, what I readily call to mind about my attempts on that occaſion.

I remember then, that when I firſt began to try ſomething in order to my deſign, being deſtitute of fit accommodations, I was fain to content myſelf, by cauſing a good bubble of glaſs with a ſtem to be ſo blown at the flame of a lamp, that whilſt the ball was yet exceeding hot, and conſequently contained none but highly rarefied air, the ſtem was very nimbly clapped into the flame of a candle, that was purpoſely kept ready at hand; ſo that being ſlender, it was in a trice ſealed up, and the air within remained as much expanded, as the great heat, it had been expoſed to, had brought it to be. This bubble many months after I inverted into a baſon of water, and having broken off the ſeal under the ſurface of it, the liquor was violently impelled into the cavity, but yet was not able to fill it, a conſiderable part being defended from the further aſcenſion of the water by the ſpring of the remaining air, which, for all the long ſtretch it had been put to, had not loſt any thing of its ſpring, that we could take notice of. But this was a trial, in which I could by no means acquieſce; and therefore when I was a little more befriended by opportunity, I tried another way, partly to give a ſomewhat pleaſing ſurpriſe to unaccuſtomed beholders, and partly, becauſe though it could not ſhew all, that I deſired, yet it might plainly ſhew, that the air, even at a very conſiderable extenſion, would hold out for a conſiderable time. Wherefore leaving a very ſmall proportion of air in the folds of a fine limber bladder, whoſe neck was very cloſely tied, I cauſed it to be, by the help of the Machina Boyliana, ſo expanded, that at length it ſo dilated it ſelf, as to ſeem to fill the whole bladder, and reduce it to the extent it had juſt before it was emptied; and the bladder, by a peculiar contrivance, was ſo included in another veſſel, that being protected from all intruſion of the outward air, it maintained its plump and tumid figure, and in that unwrinkled ſtate I ſhewed it, many months ſince, to ſome virtuoſi, now here in *London*, after it had continued ſo, if I miſtake not, near two years. Since the writing of this, I did, at length, find the newly mentioned veſſel, and ſhewed it to ſome curious [r] ſpectators, who with me took notice, that the included bladder, inſtead of being wrinkled or ſhrunk, appeared to be plump and full, as well blown bladders are wont to be. So that many months, perhaps a dozen, may be added to the freſhly mentioned duration of the expanded air.

BUT this way ſatisfying me neither as to ſome of the particulars I deſired my attempts ſhould diſcover, I deviſed a little inſtrument, whoſe contrivance, though it ſeemed very ſimple, promiſed, and for ſome time gave me a far more accurate account of what I expected. The inſtrument, if you deſire it, I can eaſily ſhew you, having lately been forced to make a new one, which is now by me: but it may ſuffice to tell you, that is is ſo framed, that it is fit to diſcover, beſides divers other things, whether, and how long air brought to the greateſt expanſion I could conveniently reduce it to in my engine, will retain its ſpring; and by what degrees or ſtages and periods of time, the decrement, if any be, is made? But of the iſſue of the trial made in it, I can give you but a very imperfect account, in regard, that, though I made it about three years ago, yet having left the inſtrument in a place, where it is ſo lodged, that I cannot have it without returning thither, till I ſee it again my ſelf, I dare not venture to judge of the ſucceſs of the experiment: only this I remember, that I took no notice of any obſervable diminution in the air's elaſticity though, it were preſſed,

and,

and, as it were, clogged with a weight, that one would wonder how it could, when it was so highly rarefied, support for one minute*.

* See the postscript.

THERE is also another way, that I contrived, wherein the air in a little portable instrument, which I can shew you, being expanded, as one may guess, to five or six hundred times (perhaps a thousand times) its wonted extent, has not only for a long preserved its spring, but satisfies me also about one of my chief queries, which was, whether the air, very much dilated without heat, would be considerably sensible of external heat? which it plainly appears to be in this instrument, where, notwithstanding the great rarity it has already attained and seems likely to preserve, the heat of one's hand applied to the out-side of the vessel has a quick and very manifest operation; and upon the withdrawing of it, the sensible air quickly returns to its former dimensions, as well as temper; so that one may employ it as a kind of weather-glass, and perhaps make some discoveries by long comparing it therewith.

BUT hitherto I have been doing, what I do not love to do, and very rarely have done, when I mention my own experiments, that is, I have not punctually specified any determinate quantities and proportions of the things spoken of; but one of my former trials I have newly found out registred in a loose note, and therefore the quantities being annexed, I hope it may both give some countenance to what I have been saying, and give some, though not an entire, satisfaction about the thing itself.

March 18 A glass, as cylindrical as we could get it blown at our lamp, and having a long stem coming out at the unsealed end, was quite filled with water and inverted into water placed at the bottom of a large pipe sealed at one end, and of three or four foot in length. This external pipe, so called for distinction sake, was exhausted, till the air, that disclosed it self in the water of the internal pipe, had drawn out the water in the cylindrical pipe, as low as the upper part of the stem; at which great expansion of the air the external pipe being speedily and securely closed by a certain contrivance, the air thus rarefied was kept sometimes in my own chamber, that was warmer, sometimes in an under room; and after it had been kept from first to last about eleven weeks or three months, if I mis-remember not, without any other remarkable variation, than that in the cold room the water ascended, as I guess'd, about an eighth, or near a fourth at that part of the internal pipe; where the lower end of the cylinder gradually lessened itself into the slender stem. Yesterday I invited doctor *Wallis* to be present at the breaking of the glass, and to favour me with his assistance, for the better estimating the expansion of the air upon the breaking of the closed apex. The water was but leisurely (because of the slenderness of the orifice, that was made for the air to get into it) impelled up into the formerly deserted cavity of the cylinder, which it filled all, save a little bubble, which was exceeding shallow. We made use of our eyes at a fit distance, and of compasses both ordinary and callaper, to obtain these measures. The cylindrical part of the internal pipe was three inches in length, and three fifths of an inch, or less, in diameter on the outside. The bubble was two tenths in diameter, and about two centesms in depth: From all which, by the doctor's calculation, the bubble, to the space it possessed unexpanded, was at one to one thousand three hundred and fifty.

NEW

NEW EXPERIMENTS

TOUCHING THE

CONDENSATION OF THE AIR

BY MERE COLD,

AND

Its COMPRESSION without MECHANICAL ENGINES.

BECAUSE it is as truly, as commonly, faid, that *contraria juxta fe pofita magis elucefcunt*, and becaufe what I am now going to interpofe, is little lefs than neceffary to be premifed, to clear the way to what follows, and to connect the paft writing to that which is to enfue; it will not be improper, to add fomething in this place touching the condenfation and compreffion of the air.

AND here I cannot but a little wonder, that among fo many, that have had occafion to confider the nature of cold, and the condenfation of the air by it, I have not yet met with any, that have had the curiofity to meafure that condenfation; wherefore I long fince attempted to do it, as I have related in another difcourfe; but not having that by me at prefent, and remembring in general, that I did it in winter, when it may be objected, that the air, being already præ-affected with the coldnefs of the feafon, was not capable of being fo confiderably contracted by an additional cold, as it would be at a time of year, when it is wont to be in a ftate of greater laxity; I thought fit to make the experiment about the beginning of autumn, without tying my felf to make it with the fame circumftances, that I had done before the event of this trial I find regiftered as follows.

AFTER the midft of *September*, on a funfhiny day, and about noon (which circumftances we made choice of, that the air might be the more rare and expanded) we took a bolt-head or round vial furnifhed with a long ftem, and placed in a frame purpofely provided, fo that the ftem was perpendicular to the horizon, and the globulous part was fupported by fuch a veffel, that thorough a hole, purpofely made at its middle, the fhank reached downwards, till the orifice of it was a little immerfed beneath the furface of a glafs full of water, that was placed at the bottom of the frame. This done, we took a good proportion of ice, and having beaten it in a mortar, and mixed with it a due quantity of bay-falt, we not only laid it round about the lower part of the ball, but the veffel contiguous to that part, being purpofely made with turned-up brims, we were enabled to heap up the frigorifick mixture, fo as to bury the whole globulous part of the glafs in it, and cover the very top of it therewith to a confiderable thicknefs; upon which occafion, the air within being exceedingly refrigerated, the water, into which the fhank terminated, was made to afcend fomewhat faft along the cavity of the fhank, till we perceived it would reach no higher, but after a while began to fubfide again; which nick of time being carefully watched, we made a mark at the higheft ftation of the water, and then taking out the bolt-head we filled it with water, making allowance for that fmall part of the ftem, which was immerfed at the beginning of the operation. This water we weighed, and found it amount to nineteen ounces and fix drachms, then weighing as much water, as fufficed to fill the fhank up to the mark newly mentioned, we found that to be one ounce and three drachms, by which number the former being divided, the quotient was fourteen drachms, four elevenths, fo that the proportion of the two quantities of water being as eleven to one hundred fifty-eight, the fpace, into which the air was condenfed by refrigeration, was, to the fpace it poffeffed in its former ftate of laxity, as one hundred forty-feven to one hundred fifty-eight, and confequently the greateft condenfation, that fuch a time of the year and in fuch weather, fo high a refrigeration could bring the air to, made it lofe but $\frac{11}{158}$ of its former extent.

NB. FIRST, the ftem of the glafs ought to be of a confiderable length, left by the great contraction made of the air in the ball by its high refrigeration, the water fhould afcend into the cavity of the ball itfelf, and thereby become exceeding difficult to be meafured.

SECONDLY, if one would be nice, one may take notice, that the height, to which the water afcended in the ftem, was about two foot; which cylinder of water, by its weight or tendency downward, might fomewhat hinder the liquor from afcending quite fo high as it would, and confequently keep the condenfation of the air from appearing fully fo great as it was, but fo light a cylinder as that of the fufpended water would fcarce be very confiderable.

THIRDLY, when the water was afcended near as high in the fhank as it would rife, there was obferved in it an odd kind of fubfultus, or rifing and falling alternatively, almoft like the mercury in the Torricellian experiment, before the

the mercury comes to settle after its first subsidence. [But the consideration of this phænomenon belongs not to this place; for which reason I insist not on this, and forbear mentioning some others.]

FOURTHLY, that though it appears not by this experiment, whether the cold thus produced is equal to that of frosty weather in winter, and consequently capable of contracting the air as much as that season is wont to do; yet by preceeding trials, made with fit instruments, I had found, that by such an application of ice and salt as we had made in the late experiment, a greater degree of cold, and that in a warmer season, might be produced, than had been found necessary to make frosty weather in winter. The way of experimenting for brevity sake I omit, but if you please you may command it.

BUT it is not chiefly to acquaint you with the condensation, that nature uses to make of air, that I have been entertaining you with these memorials; for that, which makes it very pertinent to my present purpose, is, that it will shew you, that as to the condensation or compression of air, that I am to recite, though cold were employed about it, yet it was not really produced by cold, which could not contract the air to so much as half that degree, you will find it was reduced to by our operation, presently to be mentioned; wherein the frigorifick mixture did not primarily or immediately compress the included air, but only so affected the water, that was shut up within the same vessel, as to make it swell, and consequently crowd the aerial particles into less room: wherefore it now remains, that we proceed to the experiment itself, a short account of which be pleased to take in the ensuing transcript.

[To convince some strangers, we took a new glass bolthead, with a neck not long, and filled it so far with common water, that being hermetically sealed, the liquor reached within three inches of the top, as near as we could guess by measuring it with a ruler, and making an estimate of the sharp end, made so for the conveniency of sealing up the glass, which sharp end we guessed to be about a quarter of an inch in length, then applying snow and salt to the lower part of the bolt-head, we readily drove out the water further and further into the neck, till at length it was got up to the basis of the sharp and conical end, where the glass was sealed, and then just as I was looking upon it, the glass flew with a noise about my ears, being broke into many pieces, which argued the compression of the air to have been very great. And Doctor *Wallis*, who was present, and measured it from time to time, desired me to register the experiment, with his estimate of the compression, which was, that the air was reduced into the fortieth part of its former extension.]

I know so great a condensation of air will seem strange to those, that have taken notice, that some of the best mathematicians of our age, that have made use of wind guns, and other forcible engines to crowd the air into as narrow room as possibly they could, confess themselves not to have been able, with all their strength and industry, to force the air into less than the fifteenth part of its usual extent; and besides, that this was done in countries, where the air may * well be supposed more lax and rare than in *England*. I confess I saw no trials made with wind-guns, that convinced me, that the condensation was so great as that newly specified: (about which *Mersennus* himself somewhat hesitates, seeming to doubt, whether the air were indeed restrained into a fifteenth, or but into one eighth part of its former room.) And he, that hath observed and considered, as I have done, that in wind-fountains, as they call them, of glass, the air will seem to be notably compressed, whilst, indeed, we could not find it compressed into much less than its third part, will be the less unapt to be diffident of the great things, that are said of the compression of the air: but because experience has informed us, that our English air may in peculiar instruments be forcibly crowded into a tenth, twelfth, or perhaps a fifteenth part of its former extent, I am content to take it for granted, what is related about the compression of the air, into the fifteenth part of its usual dimensions; and yet our experiment will be a considerabler instance of the great compressibility, if I may so speak of the air; for, according to the estimate delivered in the foregoing narrative, our compression, which was without mechanical instruments or engines, reduced the air into the fortieth part of the space it had lately possessed; and how great a force is requisite, when the air is once considerably condensed, to surmount, though but a little, its great ressistance to further condensation, may be gathered from the observations about the gradual renitency of the air to compression, which we many years since made with mercury, and afterwards published in another treatise: but though upon the recited grounds, that great compression of the air produced by our experiment may, as I was saying, seem very strange, yet it would not seem incredible, if I should here borrow those experiments and observations from my already published history, and some unpublished papers about cold, that would countenance what I have been delivering, and especially if I should stay to communicate to you the way, I not unsuccessfully made use of, to estimate by weight the great force of the expansion of water upon its freezing. But since an account of this contrivance is not here necessary, and would require more leisure than I can spare at this time, it remains only, that by way of corollaries from what has been hitherto delivered in this and the two precedent writings we rather point at, than discourse of some observations, that it suggested to us, in the ensuing paper.

Defence against *Linus*.

* See *Mersen.* Phæn. Pneum. Prop. 32.

OF

OF THE ADMIRABLY

DIFFERING EXTENSION

OF THE

Same QUANTITY of AIR,

RAREFIED and COMPRESSED.

HAVING already declared, that what I pretend to in the close, is but to set down some observations, that result from, or are suggested by what hath been already delivered, I presume I need not trouble you or myself, with any other preface to what follows.

THAT then, which seems first worth taking notice of, is the differing alterations, that the air is subjected to by cold and heat: for whereas we could not find in this our climate, that cold would reduce the air into near the twentieth part of its former extension by condensation, heat would advance it to near seventy times its usual laxity by rarefaction.

NEXT, we may observe, that by engines and other artificial instruments, the air may be two or three times as much compressed, as nature is wont to condense it by cold; even in frosty weather; and so on the other side, the air may by the intervention of art and instruments be much more rarefied and expanded, than it has been yet found to be by the bare application of external heat, though it were that of an intense fire itself.

See as to the utmost expansion by heat *Mersen.* in Cog. Phys. Mathem.

FURTHERMORE, it may seem worth while to observe, how much the utmost degree of rarefaction by heat, that experiment hath shewn us of the air falls short of the degree of expansion, to which it has been advanced in our pneumatical engine, the proportion betwixt these two expansions being that of one to seventy, or thereabout.

BUT, perhaps, it will not be necessary to conclude, that the air is so much more rarefiable than compressible, as most readers will be prone to infer, by comparing the greatest compression and expansion of it, that are mentioned in these experiments; since, if I mistake not, it ought to be considered, that the air, we made our trials with, upon the surface of the earth, was not (no more than is the air we commonly breath) properly in a true natural consistence, as they speak; or, if you please, in a free and indifferent state in reference to rarefaction and condensation, but was already highly compressed by the weight of the atmospherical pillar, that leaned upon it, so that is had already a very strong renitency to further compression; whereas the air, that was to be rarefied, had, by virtue of its spring, (strongly bent by the weight of the incumbent air) a strong propension or tendency to dilate itself; which difference I must content my self to have intimated, and leave you to consider, whether and how much it may alter the case.

FOURTHLY, To some perhaps it will seem more fit to consider, than easy to resolve, how, since the corpuscles of the air are acknowledged to be heavy, and those, that remain, must be so wonderful thinly dispersed in the cavity of the receiver, they come to be supported, and kept, as it were, swimming therein, and do not appear to subside by their own weight, the *Materia subtilis* (though the presence of that should be admitted) not appearing to have gravity, wherewith to sustain them; and the vacuum (if that be supposed wherever the aerial particles are not) being too near a-kin to nothing, to be able to oppose their descent: but though something may be suggested about the solution of this doubt, my haste obliges me to leave it as such.

FIFTHLY, I will not make it my business to make mention in this place of the wonder, that may be justly excited in those, that when they look on one of our well exhausted receivers, attentively consider, how small a proportion the common aerial corpuscles, which are very sparingly dispersed there, bear to the whole cavity of the vessel, which, before it was exhausted, was thought to be replenished with air alone. This, I say, I shall not sollicitously observe, because I think I need not; for I little doubt, the thing will be observed and laid hold of, both by the *Cartesians* and *Epicurians*; the former of which will endeavour thereby to establish the necessity of their *Materia subtilis*, to maintain the plenitude of the world, and the circle they attribute to moving bodies; and the latter will here triumphantly pretend to have a more illustrious instance than ever, of their *vacuum coacervatum* within the world, since there is an impenetrable vessel, out of which it is manifest, that an almost incredible proportion of aerial substance hath been manifestly made to issue; whereas it is

no

no ways manifest to any of our senses, that any other body has got in to succeed in its room: wherefore leaving them to debate, what it is, that is contained in that far greatest part of the vessel, that the air pumped out of our receiver has deserted, I take notice,

SIXTHLY, That to conclude with what was the main drift of this, and the foregoing papers, we are here invited to observe, with wonder, the stupendious mutability of the air, as to rarity and density, whereby the same quantity of air, being sometimes compressed, sometimes dilated, may change its dimensions to a degree, that seems almost to transcend the power of nature and art, and by consequence might probably be rejected as increble, if it were abruptly and nakedly proposed: and therefore it will be convenient to do, though very briefly, these two things.

FIRST, To consider, what we have upon experience delivered in our defence against the learned *Linus*, touching the condensation and rarefaction of the air, as it is exposed to a greater or smaller pressure; without the intervention of either external heat or elaborate engines. For from these experiments, (that may be found in the lately mentioned defence*) eminent mathematicians have inferred, that one can scarce safely put determinate limits to the stupendous rarity, which the upper part of the atmosphere, being almost totally uncompressed by incumbent particles of air, may be supposed to have by nature, un-assisted by art.

AND this is the first of the two things, I above desired to have taken notice of. But the other (which though it be but the second, is much the more considerable) is to confer together the smallest extent; to which we have reduced it by condensation, and the greatest, to which we have advanced it by rarefaction, after having taken notice, that according to the least estimate of any recited in the foregoing experiments, the extension of the same air, is as 1 to 2744, or thereabouts; and if instead of the moderatest, we take the greatest expansion of the air, being (leaving out the odd hundreds to make the rounder number) as 13000 to 1, when the uncompressed air was highly rarified, that number being multiplied by 40, because of the fore-mentioned compression of the air, will amount to 520000, for the number of times, by which the air at one time exceeds the same portion of air at another time; which is a difference of expansion so great, that I hope it will keep you from thinking the title of the foregoing epistle, where the expansion of the air is called admirable, immodest, especially since I have forborn to mention, what probable arguments might be offered, to prove it at least possible, that the industry of men, and perhaps our own, may find means to make both the condensation and rarefaction of the air to exceed the uttermost, whereto we have yet been able to bring them.

* Chap. V. Whose Title is, Two New Experiments touching the Measure of the Force of the Spring of the Air compressed and dilated.

POSTSCRIPT.

Touching an Observation to be inserted above, (Page 208) *immediately after the Mark* *.

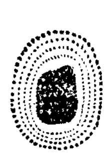
SINCE the writing of this, the author chanced to find one of the lately mentioned instruments of a considerable bigness, which was presumed to have miscarried; and comparing it with a memorial made, when it was first compleated, to keep in memory the heights, dimensions, &c. of the inclosed mercury and air; we found, that in about ten weeks, there was not any considerable variation of them; and the little shrinking of the air, which was discoverable by an attentive eye, was not such, but that it might be probably ascribed to the change of the weather to a far greater coldness which might be supposed, a little (and it did it but very little) to weaken the spring of the included air, and consequently abate of its full resistance to the pressure of the mercury in the longer leg of the syphon.

AN OBSERVATION OF A SPOT IN THE SUN.

First Printed in the PHILOSOPHICAL TRANSACTIONS, N° 74, p. 2216, for *April* the 27th, 1671.

Friday, April 27, 1660.

"ABOUT eight of the clock in the morning, there appeared a ſpot in the lower limb of the ſun, a little towards the ſouth of its æquator, which was entered about $\frac{1}{40}$ of the diameter of the ſun, itſelf being about $\frac{1}{160}$, in its ſhorteſt diameter, of that of the ſun; its longeſt, about $\frac{1}{40}$ of the ſame. It diſappeared upon Wedneſday morning, May 9th, though we ſaw it the day before, about ten in the morning, to be near about the ſame diſtance from the weſtward limb a little ſouth of its æquator, that it firſt appeared to be from the eaſtward-limb, a little ſouth alſo of its æquator. It ſeemed to move faſter in the middle of the ſun than towards the limb. It was a very dark ſpot almoſt of a quadrangular form, and was incloſed round with a kind of duſkiſh cloud, much in this form, and in this proportion to the ſpot.



"WE firſt obſerved this very ſame ſpot, both for figure, colour and bulk, to be re-entered the ſun, May 25th, when it ſeemed to be in a part of the ſame line it had formerly traced; and was entered about $\frac{4}{33}$ of its diameter about ſeven of the clock in the afternoon. At the ſame time there appeared another ſpot, which was juſt entered, and appeared to be entered not above $\frac{1}{132}$ part of the ſun's diameter. It appeared to be longeſt towards the north and ſouth, and ſhorteſt towards the eaſt and weſt. There ſeemed to be diſperſed about it divers ſmall clouds here and there."

[THESE obſervations were made, as the noble obſerver told us, with an excellent teleſcope, in the preſence of divers curious and ingenious perſons, one of whom was Mr. *Hook*. And diſcourſing of thoughts he had entertained touching the effects of ſuch ſpots, he ſuggeſted this inquiry, whether they might not cauſe a conſiderable alteration both in the body of the ſun itſelf, and in our air, and the bodies in it upon their diſſipation?]

AN

AN ESSAY ABOUT THE ORIGIN AND VIRTUES OF GEMS.

Wherein are propofed and hiftorically illuftrated fome Conjectures about the Confiftence of the Matter of PRECIOUS STONES, and the Subjects, wherein their chiefeft Virtues refide.

The PUBLISHER *to the* READER.

THE philofophy and origin of gems, as well as their ufefulnefs and virtues, will, I am perfuaded, be found, upon the attentive perufal of this effay itfelf, fo rationally and warily delivered therein, that there will need nothing to be faid in the praife of the compofure thereof. I dare venture, notwithftanding the noble author's modefty, to prefent it to the moft critical tafte, without hanging out a bufh to it.

ALL I have to fay in the publifhing thereof fhall be the fame, that was alledged by the Englifh interpreter of the learned *Steno*'s Prodromus to an intended differtation of his, concerning folids naturally contained within folids, printed the laft year by *Mofes Pitt* in *Little Britain*; where in the Englifh preface occur paffages to this effect, *viz.*

" THAT the honourable author of this ef-" fay, before he would fee or hear any thing " of that Prodromus of *Steno*, did upon occa-" fion folemnly declare to the author of that " Englifh verfion (who there protefts, that " he fpeaks it *bona fide*,) the fum and fub-" ftance of what is deduced at large in this " tract; the manufcript whereof the faid " interpreter then faw, and received it into " his cuftody for publication: which fum was " this; firft, that the generality of tranfparent " gems have been once liquid fubftances, " and many of them, whilft they were either " fluid, or at leaft foft, have been imbued " with mineral tinctures, that con-coagulated " with them; whence he conceives, that di-" vers of the real qualities and virtues of " gems may be probably derived.

" SECONDLY, as for the opacous gems, " and other medical ftones, as blood-ftones, " jafpers, magnets, emery, &c. he efteems " them to have, for the moft part, been earth " (perhaps in fome cafes very much diluted " and foft) impregnated with the more copious " proportion of fine metalline or other mine-" ral juices or particles, all which were after-" wards reduced into the form of ftone by the " fupervenience (or the exalted action) of " fome already inexiftent petrefcent liquor, or " petrific fpirit, which he fuppofeth may " fometimes afcend in the form of fteams; " from whence may be probably deduced, " not only divers of the medical virtues of " fuch ftones, but fome of their other qualities, " as colour, weight, &c. and alfo explained, " how it may happen, what he hath (which " he doubts not but others have done alfo) " obferved of ftones of another kind, or mar-" chafites, or even vegetable and animal fub-" ftances, that have been found inclofed in " folid ftones; forafmuch as thefe fubftances " may eafily be conceived to have been lodg-" ed in the earth, whilft it was but mineral " earth or mud, and afterwards to have been, " as it were, cafed up by the fupervenient " petrific agents, that pervaded it.

" Nor are thefe petrefcent liquors the only " ones, to which he fuppofes, that many fof-" fils

" fils may owe their origin, fince he thinks, " there may be both metallefcent and minera- " lefcent juices in the bowels of the earth, and " that fometimes they may there exift and ope- " rate under the fame fpirits and fteams."

So far the preface to that tranflation; which is here repeated, to do right to this noble author, in the matter of the theory relating to the origin both of precious and other Stones. Which done, I fhall keep the curious reader no longer from the contentment, which he will doubtlefs find in the perufal of this effay.

The PREFACE.

THAT the fcarcity, the luftre, and precioufnefs of gems have made them in all ages to be reckoned among the fineft and choiceft of nature's productions, is generally granted. But whether the books, that have been divulged of them, be anfwerable to the noblenefs of the fubject, feems not to me fo unqueftionable. For, as for the origin of gems; to fay with *Ariftotle*, towards the clofe of his third book of *Meteors*, that a dry exhalation, ξηρὰ ἀναθυμίασις, (whether) fiery or firing, (ἐκπυρῦσα,) makes, among other foffils, the feveral kinds of unfufible ftones: or to tell us, according to the more received doctrine, that gems are made of earth and water finely incorporated and hardened by cold; this, I fay, is to put us off with too remote and indefinite generalities, and to found an explication upon principles, which are partly precarious, and partly infufficient, and perhaps alfo untrue. And as to the hiftory of gems, that has been fo fabuloufly delivered, that efpecially among the moderns, many learned men, philofophers and phyficians, have, for the fake of fo many improbable, and fometimes impoffible virtues, that have been afcribed to gems, been induced to deny them any virtues at all. It is true, that I am not altogether fo fevere, and that the efteem, that I find made by learned men of the inquifitive emperor *Rudolfus*'s phyfician *Boetius de Boot*, makes me difcriminate him and two or three modern authors, that in books profeffedly made on other fubjects, have written incidentally of fome gems, from fuch notorioufly fabulous writers, as *Mizaldus*, *Albertus Magnus*, (if his name be not injured by the imputation of a fpurious book) *Baptifta Porta*, *Kirannides* (and fome others, that I forbear to name) from whofe learning one would expect more warinefs and judgment. But though, for reafons elfewhere mentioned, I do not unrefervedly think, that precious ftones, efpecially opacous ones, can have no medical virtues at all; yet when I confidered, how difficult it was to affign any thing, that is poffible and intelligible, (which I do not take a fubftantial form to be,) whence their virtues may probably be derived, without giving fome fuch account of the origin of gems themfelves, as was not to be expected from the followers of the peripatetick, that is, the received philofophy; I could not but wifh, that fomething were attempted on that fubject according to the principles of the Corpufcularian.

THESE things made me the lefs backward to comply with the curiofity of my friends, *which put me upon the following difcourfe*, wherein I was content to try, what, without ranfacking the authors, that had profeffedly written *de Gemmis*, the confideration of the fubject to be treated of, my natural propenfity to take notice of nature's productions, and the trials whereto thefe confiderations and obfervations lead me, would fuggeft to my pen.

WHETHER my conjectures and ratiocinations be as new to others, as to thofe I chiefly wrote for, it is not my part to determine: only I defigned to fuit my difcourfe to the phænomena of nature, without being follicitous, with whom I difagreed or complied. And *therefore, though it fhould happen, that fome* conjectures of *mine* fhould, unknown to me, be coincident with the opinion of fome claffic writer about gems; yet I prefume, the whole fubfequent hypothefis, and the arguments it is founded upon, will appear to have been fuggefted to me by the nature of the *thing itfelf*, and *my way of confidering it: not to* mention, that fometimes one may meet with a good particular conjecture in an author, that underftands not the importance of it himfelf, and knows not how to make ufe of it, but builds it on fome fuch fabulous relation, or erroneous principle, as is apt to difcredit it with wary readers, unlefs they be fuch, to whom its compliance with the opinions, they have on better grounds already entertained, happen to recommend it. I know, it may be thought ftrange, that I have been fo very fparing in the citation of thofe authors, that have writ whole books about gems; but I have this to fay for myfelf, that I had neither them, nor fo much as my own papers about the origin of minerals at hand, when I writ the following effay. Which I was the lefs troubled at, upon two diftinct accounts; the firft, becaufe I remembered, that feveral paffages, that I had met with about the virtues of gems, cited out of divers of thofe authors, were fuch, as I fhould have much fcrupled to vouch; fome of them being fuch as I knew to be falfe; others, that I fhrewdly fufpected not to be true; and others, that appeared to me altogether incredible: and the fecond, becaufe, to forbear tranfcribing what my friends might probably have met with in authors already, would beft comply, both with their defires, which was to know my particular thoughts; and with my defign, which was partly to fee, how

how far I could make out those thoughts, by my own arguments and observations, assisted only by some very few historical passages, that I lighted on in writers not classic; and partly, to take this occasion to prosecute divers matters of fact relating to the subject I was treating of, which probably would otherwise have been quite lost. And I doubted not, but if this first draught of my conceptions were by my friends thought worthy of being inlarged, it would not be difficult for me, when I should come at my books and papers again, to inrich this tract with many histories borrowed from famous writers; if that should be thought necessary by persons, that were possibly less diffident of me than of them. In short, I proposed this discourse but as a conjectural hypothesis, wherein I attempted to derive the origin of gems, and one of the main causes, (I do not say, the only cause) of their qualities and virtues, from principles less remote, and more intelligible than those of the peripateticks; and having delivered divers observations and experiments of my own about the phænomena of gems, to explicate some of them by intelligible principles, and illustrate others by resembling things, that may be really observed in nature, or easily performed by art. Which way of handling my subject permitted me to hope, that, whether or no I should be thought a lucky conjecturer about the subject I attempted, I should, at least in some measure, prove a benefactor to what is, perhaps, preferable even to lucky conjectures themselves, the natural and experimental history of such noble subjects as gems.

AN ESSAY ABOUT THE ORIGIN AND VIRTUES OF GEMS.

SECTION I.

THOUGH it will not perchance prove very difficult to propose to you my conjecture about the causes of the virtues of precious stones; yet I fear it will not be easy for me to acquaint you fully with the grounds of it. For unless I should transcribe for you my whole discourse of the origin of minerals in general (of which you know stones make a part) I cannot well lay before you all the considerations, by which I have been induced to take up the conjecture or hypothesis I am about to propound: and consequently, I cannot well comply with your curiosity about gems, without either omitting several things, which might much countenance the following discourse, or proposing (without amply proving them,) some things, that I confess seem not clear, nor some of them so much as probable, by their own light. But since you will have it so; I will, rather than disobey you, present you in one discourse several things concerning gems, whereof some belong to others of my little tracts about the origin of minerals from fluid, or at least soft bodies; though some indeed were more directly written concerning gems: notwithstanding that they were delivered not as an entire tract about that subject, but as corollaries, that might be drawn from, and applications, that might be made of, what had been in a more general way discoursed about the origination of stones and other minerals. And therefore presuming, that you will suppose with me in this discourse some few particulars, that, I think, I have elsewhere made probable, and might perhaps do so from some of the phænomena mentioned in this writing itself, I would immediately address myself to the subject of it, if I did not think a previous admonition very requisite.

FOR, I must, at the very entrance of this discourse, desire you to take notice, that when I propose my conjectures about the virtues of gems, I do not suppose the truth of all, or so much as the tenth part of those wonderful properties, that men have been pleased to ascribe to them. For not only some of the writers of natural magick, but men of note, who should be more cautious and sober, have delivered in their writings many things concerning gems, which are so unfit to be credited, and some of them perhaps so impossible to be true, that I hope the believers of them will, among the votaries to philosophy, be as great rarities, as gems themselves are among stones.

And those, that can admit such unlikely fables, will be as much despised by the judicious, as jewels can be prized by the rich.

For my part, I never saw any great feats performed by those hard and costly stones, (as diamonds, rubies, saphires,) that are wont to be worn in rings. But yet, because physicians have for so many ages thought fit to receive the fragments of precious stones into some of their most celebrated cordial compositions; because also divers eminent men of that profession, some of them famous writers, and some virtuosi of my own acquaintance, have, by their writings, or by word of mouth, informed me of very considerable effects of some gems, (especially crystal,) upon their own particular observations; and lastly, because, that (as I shall shew anon) I find no impossibility, that at least some costly, and less hard, (though indeed more valuable) gems, may have considerable operations upon human bodies, some few of which I have had opportunity to be convinced of; I will not indiscriminately reject all the medicinal virtues, that tradition and the writers about precious stones have ascribed to those noble minerals; contenting myself to declare in a word, that suspecting most of them to be fabulous, my conjectures aim only at giving one of the causes of those virtues ascribed to gems, which experience warrants to be real and true.

Having thus explained in what sense my conjecture about the virtues of precious stones is to be understood; it follows, that I propose the conjecture or hypothesis itself; the substance of which may be comprized in these two particulars: first, that many of these gems, and medical stones, either were once fluid bodies, as the transparent ones; or in part made up of such substances, as were once fluid: and secondly, that many of the real virtues of such stones may be probably derived from the mixture of metalline and other mineral substances, which (though unsuspectedly) are usually incorporated with them: and the greatness of the variety and efficacy of those virtues, may be attributed to some happy concurrent circumstances of that commixture. The first of these heads relates properly to the origin of gems. The second, partly to that, and partly to the kinds and degrees of their virtues.

But that any gems, especially the hardest sorts of them, should have a later beginning, than that of the earth itself, will probably be thought to relish of a paradox; and I doubt not, it will pass with many for a great one, that some of these hardest of solid bodies should have been once fluid ones, or liquors: wherefore I shall endeavour to countenance this hypothesis by the following considerations.

1. And first the diaphaneity of diamonds, rubies, saphires, and many other gems, agrees very well with this conjecture, and thereby seems to favour it. For it is not so likely, that bodies, that were never fluid, should have that arrangement of their constituent parts, that is requisite to transparency, as those that were once in a liquid form, during which it was easy for the beams of light to make themselves passages every way, and dispose the solid corpuscles after the manner requisite to the the constitution of a transparent body. Therefore we see, that silver in aqua fortis, or lead in spirit of vineger, having by that solution had their particles reduced into a fluid form, those particles, though before opacous, are so disposed of, as to make, not only a diaphanous solution, but, if one pleases, transparent crystals. And what chemists usually try with those metals, I have had the curiosity to try with several stones, which I may hereafter have occasion to name to you. But this argument I bring rather to confirm than evince my conjecture.

Secondly, the origin assigned to gems may be also countenanced by the external figuration of divers of them. For we plainly see, that the corpuscles of nitre, allom, vitriol, and even common salt, being suffered to coagulate in the liquors they swam in before, will convene into crystals of curious and determinate shapes. And the like I have tried in several metalline bodies dissolved in several menstruums. But unless a concreting stone, or other like body, be either surrounded with, or in good part contiguous to a fluid, it is not easy to conceive, how it should acquire a curious angular and determinate shape. For concrescent bodies, as I may so speak, if they have not room enough in an ambient fluid for the most congruous ranging of their parts, cannot cast themselves into fine and regular shapes, such as I shall presently show, that divers gems seems to affect; but the matter they consist of must conform to the figures of the cavity, that contains it, and which in this case has not so much the nature of a womb, as of a mold. And so we see, that salt-petre, and divers other salts, if the water, they were dissolved in, be much too far boiled away before they are suffered to shoot, will, if the liquor fill the glass, sometimes coagulate into a mass, fashioned like the inside of the containing vessel, or if a pretty quantity of liquor remains after the coagulation, that part of the nitrous mass, that was reduced to be concreted next the glass, will have the shape of the internal surface of it, whatever that be; but those crystals, that are contiguous to the remaining liquor, having a fluid ambient to shoot in, will have those parts of their bodies, that are contiguous to the liquor, curiously formed into such prismatical shapes as are proper to nitre.

To apply this now to gems; that divers kinds of them have geometrical and determinate shapes, though it be not vulgarly observed, because we are wont to see them when they are cut, if not also set in rings and jewels; yet I have often had the opportunity to take notice of it, by having had the curiosity to look upon many of them rough as nature has produced them, and the good fortune to take divers of them out of their wombs. For I remember, I have taken a good number of Indian granats out of a lump of heterogeneous matter, whose distinct cavities, like so many cells, contained stones, on some of whose surfaces you might

might see triangles, parallelograms, &c. And being once near the rock, whence those stones are chiefly fetched, that are commonly called Bristol-stones, I remember, I rid thither and procured a workman or two to dig me up a number of them, divers of which I found to be curiously and determinately shaped, much like some crystals of nitre, that I have taken pleasure to compare with them. And the like figuration I have also observed in divers Cornish diamonds, and in a fair and large one, which one, that knew not what it was, found growing, with many lesser, in *Ireland*, and presented me. And to let you see, that it is not only in these softer gems, that this curious figuration is to be met with, I shall add, that I found among many stones I had, and took to be rubies (and those the jewellers will tell you are exceeding hard) a considerable number, whose shapes, though not the same with those of the Cornish and Irish stones, were yet fine and geometrical. And the like I have observed even in those hardest of bodies, diamonds themselves; of which remembering, that in my collection of minerals, I had a pretty large one, that was rough, I perceived, that the surface of it consisted of several triangular planes, which were not exactly flat, but had, as it were, smaller triangles within them, that for the most part met at a point, and did seem to constitute, as it were, a very obtuse solid angle: encouraged by this, I examined several other rough diamonds, and found the most of them to have angular and determinate shapes, not unlike that newly mentioned. And having thereupon consulted an expert jeweller, that was also a traveller, though he could not name to me the shapes of the uncut diamonds, he had met with; yet he told me, he generally found them to be shaped like that I shewed him; insomuch, that such a shape was a mark, by which he usually judged a stone to be a right diamond, if he had not the opportunity to examine it by the hardness.

AND this I shall add in favour of the comparison, I lately intimated betwixt the coagulation of petre and that of gems, that having once made an odd menstruum, wherein I was able to dissolve some precious stones, there shot in the liquor, crystals pretty large, and so transparent and well shaped, that they might well have passed for crystals of nitre; and yet, if I much misremember not, they were insipid. And I have divers times taken notice in such stones, as the Bristol diamonds, that though that part, which may be looked upon as the upper part of the stone, were curiously shaped, having six smooth sides, which at the top were, as it were, cut off sloping, so as to make six triangles, that terminated like those of a pyramid in a vertex; yet that, which may be looked upon as the root or lower part of the stone, was much less transparent (if not opacous) and devoid of any figuration; of which the reason seems to be, that this being the part whereby the stone adhered to its womb, it was sullied by the muddiness of it, and reduced to conform it self to whatever shape the contiguous part of the cavity chanced to be of; whereas the upper part of the stone was not only formed of the clearer part of the lapidescent juice before the waterish vehicle was exhaled, but had room and opportunity to shoot into the curious figure belonging to its nature. And this is much more conspicuous, where many of these crystals grow, as it were, in clusters out of one mineral cake or lump; as I have seen not only in those soft, but yet transparent concretions, which some of the later mineralists (for the ancients seem scarce to have known them) call fluores, and particularly in a very fine mineral lump, that I had once the honour to have shewed me by a great prince, and no less great a virtuoso, to whom it was then newly presented. For this mass consisted of two flat parallel cakes, that seem composed of a dirty kind of crystalline substance, and out of each cake there grew, towards the other, a great number of stones, some of which, by their cohesion, kept the two cakes together; and most of these stones, having each of them a little void space about it, wherein it had room to shoot regularly, were geometrically shaped, and, which looked very prettily, were coloured like a German amethist. And I have my self a pretty large stone, taken up here in *England* by a gentleman of my acquaintance, which consists, as it were, of four parts; the lowermost is a thin and broad flake of coarse stone, only adorned here and there with very minute glistering particles, as if they were, (as probably they may be) of a metalline nature; over this is spread another thin white, but opacous, bed, which is so inclosed between the first named bed, and the two others, that, without defacing the stone, I cannot well examine it: the third consists of a congeries of minute crystals exceedingly thick set, which therefore look whitish, having little or no tincture of their own; and this part, no more than either of the former, is not much thicker than a barley-corn. The fourth and uppermost part, which, yet seems in great part to be the same crystals, which, as they grow higher and spread, acquire a deeper colour, is made up of a great number of amethists, some paler, and some highly tincted, which are of very differing figures, and bignesses, according (as one may guess) as they had conveniency to shoot; these at one end of the stone lying in a flat bed, as it were, and scarce exceeding a barley corn in length; whereas those at the other end shoot up to a good height into figured crystals, some of them as big as the top of my little finger, and those are the most deeply coloured, being also of a good hardness, since I found, that they would easily grave lines upon glass.

I remember also, that going to visit a famous quarry, that was not very far from a spring, which had somewhat of a petrescent faculty in it, I caused divers solid pieces of rough and opacous stones to be broken, out of hope I had to find in them some finer juice coagulated into some finer substances; and accordingly I found, that in divers places, the solid and massy stone had cavities in it, within which, all about the sides, there grew concretions

tions, which by being transparent, like cryſtals, and very curiouſly ſhaped, ſeemed to have been ſome finer lapideſcent juice, that by a kind of percolation through the ſubſtance, that groſſer ſtone was made of, had at length arrived at thoſe cavities, and upon the evaporation of the ſuperfluous and aqueous parts, or by their being ſoaked up by the neighbouring ſtone, had opportunity to ſhoot into theſe fine cryſtals, which were ſo numerous, as quite to overlay the ſides of the cavities, as I can ſhew you in ſome large cluſters of them, that I brought from thence. And enquiring of an ancient digger, whether he had not ſometimes met with greater quantity of them? he told me, that he had, and preſented me a great lump or maſs made up of a numerous congeries of ſoft cryſtals, (but nothing ſo colourleſs as theſe other newly mentioned) ſticking to one another, but not any of them to any part of the rock: ſo that they ſeemed to have been haſtily coagulated in ſome cleft or cavity, as it were in a mold, where meeting and mingling before concretion, with ſome looſe particles of clay, the maſs may thereby be diſcoloured.

OUR argument, drawn from the figuration of tranſparent ſtones, may be much ſtrengthened by the coalition I have ſometimes obſerved of two or more of ſuch ſtones, and the congruity in the ſhape of ſome of them to the figures of thoſe parts of the others, that were contiguous to them, and ſeemed to have been formed after them. But though this phænomenon be conſiderable to the ſcope of my diſcourſe, yet perceiving, that I ſhall have occaſion to inſiſt on it hereafter, I ſhall not do it now.

THIRDLY, nor is it only the external figuration of theſe gems, but the internal texture, that favours our hypotheſis, ſome of them ſeeming much to imitate in their coagulation ſeveral of thoſe ſubſtances, which I have obſerved to have once been fluid. That common ſalt may be made up of ſmall ſaline particles, that by a convenient juxta-poſition may be aſſociated into great lumps, divers of which are cubically ſhaped, is an obſervation eaſy enough to be made. And that ſuch coalitions of particles may conſtitute ſolid and conſiderably hard bodies, I have tried by breaking ſome of the larger cubes of ſal gem, and the lumps of the iſle of *Mayo* ſalt, whereof the firſt is foſſil, the other marine, and both natural. I have likewiſe found by trial, that, though ſilver diſſolved in aqua fortis appears uſually to ſhoot, if it be taken notice of, into flat and exceeding thin flakes; yet it is very poſſible ſo to order the coagulation, that many of theſe thin plates ſhall, in their convention, have their flat ſides ſo placed over one another, as to make up pretty large and thick cryſtals, whoſe very outſides will be finely ſhaped, as being ſome peculiar kind of vitriol. Nor are theſe the only fluid bodies, which I have reduced to coagulate into conventions, of ſuch a flaky texture; wherefore I began to ſuſpect, that divers tranſparent minerals may have the like; and in ſome diaphanous kinds of talk, whoſe outſides were mathematically figured, I found encouragement to try, whether even ſome gems themſelves, notwithſtanding their hardneſs, might not have ſuch an internal figuration. Nor was I deterred by conſidering, that it is taken for granted, that gems are of an uniform texture, and that there muſt be a ſtrange thinneſs in the plates, that make up tranſparent ſtones, ſince no ſuch thing has been noted by the moſt curious eye, but men have taken it for granted, that the texture of all gems is uniform, without any grain or fibres, no more than there is in gold. But as to the thinneſs of the plates, I remember, I have ſeveral times taken pleaſure to hold a piece of good Muſcovia glaſs againſt the light, when it was of ſuch a thinneſs, that the ſpectators, though provoked to look with curious eyes, could ſcarce ſee the plate itſelf, and would by no means be brought to think, that it was poſſible to ſplit it, till I did actually do it; and ſometimes I then ſubdivided it beyond even my own expectation. But to examine this conjecture, I took ſome ſtones, that had geometrical figures on part of their ſurfaces, and which I had other grounds to think to have been once fluid ſubſtances, and having diligently ſurveyed ſome of them, which ſeemed likelieſt to give me ſatisfaction, I manifeſtly enough perceived, not only with my aſſiſted, but with my naked eyes, divers parallel commiſſures, which ſeemed plainly to be made by the contiguous edges of little thin plates of ſtone, that appeared to lie one over another, almoſt like the leaves of a book, that is a little opened.

I remember, that holding a large and rough grizolette (as artificers call hard gems, of a blueiſh colour, brought them from *Eaſt-India,)* againſt the light, and curiouſly obſerving it, I have ſometimes diſcerned a grain, as they call it, in the ſtone, and was anſwered by a ſkilful artiſt, that uſed to make ſeals of them, that ſuch ſtones would uſually ſplit, according to the ductus of their grain. I will not urge, that in ſome other precious ſtones, that were cut and poliſhed, as particularly the hyacinth, and even the ſaphire, by obverting them ſeveral ways to the light, I have been able to obſerve, as it were, commiſſures, which were ſo fine, as not to hinder, or call in queſtion, the intireneſs of the ſtone, for the lapidary's purpoſe. This, I ſay, I forbear inſiſting on, becauſe the phænomenon is far leſs conſiderable, than what I have ſeveral times obſerved in New-Engliſh granats, wherein, eſpecially when they are broken, the edges and commiſſures of the thin plates or flakes, whereof they conſiſted, were very eaſily diſcernable. And to try, whether this obſervation would hold even in the hardeſt ſtones, I had recourſe to a pretty big diamond unwrought, which being placed in a miſcroſcope, ſhewed me the commiſſures of the flakes I looked for, whoſe edges were not ſo exactly diſpoſed into a plain, but that ſome of them were ſenſibly extant like little ridges, but broad at the top above the level of the reſt. And theſe parallel flakes together with their commiſſures, I could in a ſomewhat large diamond plainly enough diſcern even with my unaſſiſted eyes. And for further ſatisfaction, I went to a couple of perſons, whereof the one was

was an eminent jeweller, and the other an artificer, whose trade was to cut and polish diamonds, and they both assured me upon their repeated and constant experience, and as a known thing in their art, that it was almost impossible, (though not to break, yet) to split diamonds, or cleave them smoothly cross the grain, if I may so speak, but not very difficult to do it at one stroke with a steeled tool, when once they had found out from what part of the stone, and towards what part the splitting instrument was to be impelled: by which it is evident, that diamonds themselves have a grain, or a flaky contexture, not unlike the fissility, as the schools call it, in wood; which you will easily grant to consist of assimilated water or juices; which having been once fluid bodies, were fit to have their particles so ranged or disposed, as to constitute a body far more easy to be cleft according to the ductus of the fibres, or planes, than otherwise. And I remember, that having, as I thought, observed in a rough diamond, which I purposely examined, that the flakes, whose edges were terminated in one plain, were far enough from being parallel to those, whose edges composed another plain, (I speak of physical planes of the same stone,) I imagined, that if this diamond were to be cleft, it would not be smoothly split into two pieces, because the commissures did probably make angles in the body of the stone; and accordingly I learned of the ancientest of these diamond cutters, that sometimes he met with stones, that eluded all his skill, and would by no means be split like others into two parts, but, before they were cleft quite through, would break in pieces; which was a defect in the stone he could not certainly foresee, but was fain to learn from the unwelcome event.

Fourthly, it seems not unprobable, that the colours of divers gems, (for I do not say of all,) are adventitious, and were imparted to them, either by some coloured mineral juice, or some tinging mineral exhalation, whilst the gem or medical stone was either in *solutis principiis*, or of a texture open enough to be penetrable by mineral fumes. Which argument's considerableness makes me hold it unfit to be lightly touched in this place; though I cannot discourse any thing fully of it in few words, because it not only suggests divers observations and other particulars, but requires also the mention of some of the chief of them; which therefore I shall now subjoin.

1. And the first shall be, that many gems, not to say almost all of them, have been observed to be deprived of their colour, if having fallen, or been put into the fire they have lain too long there; insomuch, that I have found it affirmed upon the testimony of the learned and experienced *Boetius de Boot*, that all gems will lose their colour in the fire, except Bohemian granats. How far this may be true, I have not had opportunity thoroughly to examine. But I well remember, that having porposely exposed divers gems to the fire, though that were but moderate, and had a crucible interposed between it and them, some of them seemed to have their tincture much impaired, and others quite destroyed. But I must be so free as to admonish you, that if these trials be not warily made, they may easily impose upon us; especially if we do not consider the nature and cause of whiteness. For any diaphanous body, as far as I have yet observed, being divided into a multitude of very minute parts, and consequently acquiring a multitude of distinct superficies, which do briskly reflect the light every way outwards, will appear to have a white colour, that will be more or less vivid, as the particles are more or less numerous, minute, and otherwise fitted to scatter the incident beams of light; as you may see by reducing to powder fine Venice-glass, which will be white; and even red ink, if so shaken or beaten as to be brought to a froth, consisting of many minute bubbles, will seem to have put on a whiteness. So, that if by too hasty an ignition, or too hasty a cooling of the fired gems, they come to be flawed with innumerable little cracks, they may be thought to be made white, by having their tincture driven away, when their whiteness really proceeds from the multitude of those little flaws which are singly unperceived; and the rather, because the body may still retain its former shape or seeming intireness. To illustrate which, I have sometimes taken pleasure to heat a piece of crystal red hot in a crucible, and then quench it in cold water; for even when the parts did not fly or fall asunder, but the body retained its former shape, the multitude of little cracks, that were by this operation produced in it, made it quite lose its transparency, and appear a white body. In making which experiment, the multitude of produced flaws may be pretty well discovered to the incredulous, if, as I have sometimes done, the ignited crystal be warily and dextrously quenched not in water, but in a very deep solution of cochineel made with spirit of wine, in which operation, if it be well performed, but not otherwise, enough of the red particles of the solution will get into the cracks of the crystal, to give it a pleasing colour.

The other trials, that I have made about the reducing of whiteness or paleness in bodies, either transparent, or even semi-diaphanous only, belonging to another paper, I shall here forbear to mention them, having already said enough for my present purpose, which is not so much to affirm positively, that no proof at all can be drawn from the operation of fire upon the colour of gems, as to make you cautious, what proofs drawn from thence you admit. About Tincture of Coral.

2. Wherefore declining to say any thing more about the first, I shall now proceed to the next circumstance, that belongs to our argument, (which you may think to be more considerable than the former) namely, that the colours of several gems, when they are not destroyed by fire, will be altered thereby; which being a thing, that happens to divers fossil pigments (of which some I employ to tinge glass) and other bodies confessedly mineral, argues a commixture of mineral substances in those stones, whose colour receives some of the alterations

rations I speak of: which last words I add, because I would not impose upon you, by concealing, that there may be a great change of colour, produced by the fire, without any alteration of the tinging parts as such. For by flawing the heated gem in very many parts, a degree of whiteness or paleness emerging thereupon may somewhat change the former colour. But this alteration being but a kind of dilution, is not that, which I here mean. For I remember, I have taken Indian granats, and having in a crucible exposed them to the fire, I found they had exchanged their reddish colour for a dark and dirty one, like that of iron, that has been long kept in the air. And having taken some pieces of agate prettily enough adorned with waves of differing colours, and kept them a competent time (for they should not be kept too long) in the fire, I found, as I conjectured, that the greatest part of the agate seemed to be deprived of its tincture, being reduced to a pleasant whiteness. But in some places, where there were stains of a different kind from the rest, and where there ran little veins, that I guess to be of a metalline nature, there, I say, the colour was not destroyed, but changed, and the veins of pigment, thus coloured, acquired a deep redness, which they will retain, if let alone; though I was induced to think by some trials made on other pieces of Indian agate, that even these metalline tinctures were not so fixed, but, that a lastinger fire would drive them away, and leave the stones purely white. Such a change of colour, as I lately mentioned in the veins of agate, is likewise found in those of some other stones, as also in some pebbles, amongst divers of which, that lost only their transparency by ignition and extinction in water, one or two acquired so much deeper a colour, than it had before, that I thought it remarkable.

3. ANOTHER circumstance, that seems to favour our conjecture, may be this, that it has been observed not unfrequently, that near many of the places, where coloured gems are found, some mines or veins of metals are to be met with. And I think it not unlikely, that if search were skilfully made, many more discoveries would be made of veins either of metalline ore, or some other mineral, liquid or concreted, whence, by way of juices or fumes, the gems may be presumed to have received tinctures. But usually, where precious stones are found, men's industry and curiosity is too much confined to those rich minerals, and does not make them solicitous to look after inferior ones. Besides that in *East-India*, whose countries are best for the most gems, they are wonderfully unskilful at digging mines; as I have gathered from the answers of some, who purposely went to visit the diamond mines, as they call them. To this may be also referred, that gems are several times found in the metalline veins themselves, or very near them; as I can shew you divers amethysts, that an ingenious gentleman of my acquaintance took himself out of a piece of ground abounding with the ores of iron and tin, the latter of which was there plentifully dug up. And in those colder countries, such as *Germany* and *England*, where hard gems are more unfrequent, those soft ones, that mineralists call fluores, are often to be found in, or near metalline veins, so finely tincted by mineral juices, that, were it not for their softness, they might pass, at least among most men, for emeralds, rubies, sapphires, &c. as I have been informed, not only by some mineral writers of good credit, but also by eye witnesses, and partly by my own observation.

4. THE fourth circumstance, which may be alledged to the same purpose with the three foregoing, is, that it seems possible, from some gems, by menstruums, to obtain tinctures, that seem rather extractions, than dissolutions, strictly so called. I will not urge the chemical processes, that may be met with in some authors to this effect; because some circumstances in the things, and in the writers, made me so far suspect those I could try, (and those, that required undiscovered menstruums, as they may be true, so, for aught I know, they may not,) as to keep me from meddling with them. But I remember, I once made a menstruum, (I say once, because its preparation is so subject to casualty, that I have often failed in it) which being poured upon well coloured granats, not only not calcined, but entire, was in no long time beautified with a high and lovely tincture, which was admired by very skilful persons, to whom I shewed it, because the menstruum was not more corrosive than white-wine; and which yet I therefore took to be a genuine tincture, partly because it was drawn in the cold, partly because the liquor would not tinge it self by standing, if no body were put in it; and partly because it drew a tincture from antimony of a very differing colour from this we speak of. Nor are granats the only gems, which I have made the liquor work on, in the cold.

5. To these four circumstances I shall add this fifth, that some gems, which jewellers affirm, without scruple, to be rubies, sapphires, &c. either are colourless, or have other colours, than those, that are wont to belong to them. That famous goldsmith *Benvenuto Cellini*, in his little Italian tract of his own profession, admonishes his reader, that there are one kind Page 10. of rubies, that are naturally white, (and not made so by art) which he proves by the degrees of hardness peculiar to rubies. And the same author elsewhere tells us of beryls, topazes, and amethysts, that are white. And it seems, by what he says not far from that place, that the Italian jewellers did not look upon the tinctures of gems, as any thing near so essential to them, as they are commonly reputed, since they reckon topazes and sapphires, whereof one is blue and the other yellow, but both extremely hard in comparison of other gems than diamonds (and perhaps rubies,) to be of the same speeies. The degree of hardness of rubies and sapphires is oftentimes so equal, that I knew an expert English jeweller, who for that only reason (for he knew not whence the difference of colour might proceed) took rubies and sapphires to be of the same kind of stone.

And that gems, referred by lapidaries to the ſame kind, may be very differingly tinged, is a truth, whereof I have ſeen notable inſtances in diamonds themſelves; which I therefore prefer to other inſtances, becauſe the extreme hardneſs of diamonds is ſuch, as keeps jewellers from miſtaking any other ſtone for a true diamond, if they are permitted to put them on their rapidly moved wheels employed to cut them. Now, of true diamonds I have ſeen ſome, that were yellowiſh; others, that were more yellow; and, among the reſt, one that was ſo perfectly yellow, that I at firſt took it for a fair topaz, though it were a diamond valued at near three pound weight of gold. I have alſo ſeen diamonds, and thoſe rough, as they came directly out of the *Indies*, and were ſoon after bought by traders in diamonds for ſuch, which were either blueiſh or greeniſh. And I particularly contemplated one ſtone, which, if its ſhape and other things had not convinced me of the contrary, was ſo green, that I ſhould have taken it for an emerald.

I remember I had once occaſion to buy a conſiderable number of ſmall rubies, divers of which were very curiouſly ſhaped; and coming to look upon the whole parcel more leiſurely, than my haſte would permit me when I bought it, I found in a great number of other ſtones, one, and but one, that was devoid of any colour; but in any other reſpects was ſo like the reſt, as invited me to conclude, that it would have encreaſed their number, but that it was coagulated and hardened before the mineral pigment had tinged it of the ſame colour with the reſt. In which gueſs I was confirmed, when, having met with a gentleman, who had been in the chief places of the *Eaſt-Indies*, where rubies are found, and particularly at the river of *Siam*, or *Pegu*, near which he lived a good while, and where he frequently ſaw rubies taken out of the bottom of the water, and ſometimes took them out himſelf; I learned of him by enquiry, that he had there ſeen ſeveral ſtones, each of which was partly a ruby, and partly colourleſs: and ſometimes in the ſame ſtone, there would be two portions of one ſort, and the third, though lying betwixt them, of another; which has frequently obliged the jewellers conſiderably to leſſen the bulk of ſuch ſtones, by cutting off the untincted part. And, if my memory do not much deceive me, I ſaw, in a great and curious prince's cabinet, among other rarities, a ring, in which was ſet a ſtone of a moderate bigneſs, whereof only one half, or thereabouts, was well tincted, the other being colourleſs. In gems, that are leſs precious, and not ſo tranſparent, eſpecially in agats, and in opacous gems, I could eaſily give a multitude of inſtances of the differingly tincted parts of the ſame entire ſtone. And I uſually wear in a ring a ſmall ſardonix, that was once a great prince's, wherein there are three portions, one within another, the uppermoſt black, the middlemoſt of a kind of cheſnut colour, the other of a blue, almoſt like a turquois; each of which portions is exactly of a fine oval figure, and each of the two uttermoſt is throughout of a very uniform breadth as well as colour, and exactly parallel to the other. But it would not be here ſo proper, as it will be hereafter, to multiply inſtances of opacous gems: wherefore (having mentioned only the ſardonyx, becauſe it is not always opacous,) I ſhall add concerning tranſparent ones, that jewellers reckon among ſapphires not only that ſort of azure gems, which uſually paſs for ſuch, but alſo another ſort of ſtones, becauſe of their ſapphirine degree of hardneſs; though for their want of tincture, they call them white ſapphires.

6. The ſixth and laſt circumſtance belonging to the foregoing argument or conſideration is this, that ſometimes one may find gems, that are partly tincted and partly not; as if the tinging pigment mixing with one part of the matter, whereof the ſtone conſiſted, whilſt it was liquid or ſoft, were not copious enough to diffuſe itſelf to the whole, nor to give an equally intenſe colour to all that portion, that it tinges. It is true, that in ſome caſes, the diffuſion may be ſtopped by the petreſcent juices coagulating firſt in another part than that, with which the tincture was mixed. And perhaps, in ſome other caſes, the different colours may have belonged to differing portions of matter, coagulating upon or againſt each other, at different times, yet ſo as to ſeem one intire ſtone; as I may have hereafter occaſion to declare. Yet ſince, which ſoever of theſe explications be admitted, it will, if it belong not to this place, at leaſt, confirm our main hypotheſis (of the origin of gems from fluid or ſoft materials,) I ſhall return to what I was ſaying about gems, partly tincted, and partly colourleſs. And having only intimated upon the by, that in ſome hard ſemidiaphanous ſtones, European and Eaſt-Indian, I have obſerved a very unequal and irregular diffuſion of the tincture; I ſhall add to the things, that may be gathered in favour of the propoſed conjecture from ſome of the things before (as alſo ſince) related, theſe two particulars:

The one, that I have (as I think I elſewhere mentioned) ſeen in *Italy*, among rarities, a large piece of cryſtal about the bigneſs of my two fiſts, whereof the pyramidal part was of a tranſparent green, the vertex being richly tinged like an emerald; but the further the colour ſpread from the vertex, the fainter and paler it grew; ſo that, before it came near the baſe, it was quite ſpent, if I may ſo ſpeak, leaving the bigger part of the ſtone tranſparent, but colourleſs, like ordinary cryſtal. And by this, perhaps, we may explain an expreſſion of *Joſephus Acoſta*, where he ſays, that emeralds grow in ſtones like unto cryſtals; and that he had ſeen them in the ſame ſtone faſhioned like a vein; and they ſeem, adds he, by little and little to thicken and refine. And in the ſame place, this learned author has a memorable obſervation, that may confirm both what I have juſt now related, and what we mentioned a little above, about colourleſs gems: I have ſeen, ſays he, ſome, that were half

half white and half green; others all white, and some green and very perfect. And this is the first particular I was to mention.

THE other is afforded me by the way I have used, and elsewhere described, of giving to pieces of rock crystal passably good tinctures by mineral fumes. And supposing the thus coloured pieces to be as intire stones, as the beholders have generally believed them, the instance will be pertinent to our purpose in spite of an objection. For though the colours thus given are not wont to pervade them very deep, and have their penetration assisted by no faint degree of heat; yet it is to be considered on the other side, that these pieces of crystal had attained their full hardness, and after their colouration, are cut and polished like other crystals: whereas the gems, that our conjecture means, are supposed to have been tinged under ground, when they were yet fluid, or at least soft. That there are sometimes generated in the bowels of the earth, mineral exhalations capable of applying themselves to the stones they meet with there, I have, in another discourse, sufficiently declared. That also some hard and stony substances have been actually tinged with such mineral steams, I shall, in the subsequent part of this discourse, have occasion to take notice. And I remember too, that even in so hard a gem as a sapphire, I have observed the efficacy of these subterraneal fumes; having divers times seen one of those stones, wherein a fine seal was cut, which continued so oddly tinged, notwithstanding what had been taken off to reduce it to an exquisite shape, that having inquired of a skilful person of my acquaintance, by whom it had been engraven, he both assured me, that he had found it of the full hardness of a sapphire, and confessed to me, that the mineral fumes had so oddly tinged it, that in his opinion, it might, by the looks, pass rather for a Chalcedonian.

Of subterraneal fires, &c.

AND now, Sir, I fear I may need your pardon, for having been so prolix in discoursing of one of the particulars belonging to our argument; to excuse which, I have no other apology to make, but that I hope what hath been delivered will scarce seem impertinent, and that I might easily have made it more tedious, if, to decline doing so, I had not purposely made some omissions.

HAVING then said thus much about our fourth consideration, I proceed now to add, in the fifth place, on the behalf of the hypothesis hitherto favoured, an argument, which I presume you will not think inconsiderable; namely, that solid gems may include heterogeneous matter in them. Several instances of this sort, in opacous stones, I elsewhere recite upon my own observation; but in transparent ones they are very great rarities; and therefore it will not, I presume, be thought strange, if I mention but a few.

FIRST then, on this occasion I remember, that a very ingenious and qualified lady, who had accompanied her husband in an embassy to a great monarch, assured me, that she brought thence, among several rich presents and other rarities, (some whereof she shewed me,) a piece of crystal, in the midst of which there was a drop of water, which by its motion might be very easily observed, especially when the crystal was made to change its posture. And, if my memory deceive me not, I have, in some pieces of rock-crystal, taken notice of things, that seem to argue, that somewhat or other was intercepted within the body of the stone.

A curious person, that traded much, and was very skilful, in Indian gems, particularly grizolettes, which he got from the *Indies*, and whereof he shewed me the largest I have yet seen, being asked by me, whether he had ever found in them any heterogeneous substance; which something, I had observed, made me suspect, that some of them might harbour, notwithstanding their hardness; he averred to me, that among divers rough ones, that were brought from the *Indies*, he had with wonder seen one, that was about the bigness of a philbert, in the solid substance whereof there was a cavity with a certain liquor in it; which, by changing the posture of the stone, might be made to move to and fro in the cavity: and when the drop was settled, it was of the bigness of a round pearl, that he shewed me, which wanted somewhat of a moderate size for a neck-lace. And when he had answered the questions I proposed him, to clear my doubts, he added, that this rarity made the stone, which was otherwise of a small value, prized at an hundred pound. And I have myself seen a monstrous gem, if I may so call it, and little less a rarity then the former, that an acquaintance of mine had bought, (as I afterwards learnt,) from this relator; whose narrative about the grizolette I think the more credible, because that having had the curiosity to break a stone, that was brought as a rarity from the *East-Indies*, where gems are often harboured in such stones, I found in the solid substance of it (which was so hard as to strike fire, like a flint, and in its little flakes was at least semi-diaphanous) a cavity, wherein were coagulated very minute but polished and crystalline stones, which seemed to have their points inwards; which argued, that there had been some liquor, in which these glistering particles had shot, though in process of time the remaining and incoagulable part of it may have been imbibed by the ambient matter, if not have escaped thorough it, by virtue of some peculiar congruity of it with the pores of the stone. Which need not be thought impossible, since experience has assured us, that some solid stones, and even gems, may be, though slowly, penetrated, or have their texture altered by common water. Nor are these the only heterogeneous substances I found included in this stone.

AND if, as amber is reckoned among gems, and is sometimes of a greater hardness, than one would expect, so I could reckon it among true stones, it were easy for me to borrow thence a great confirmation of what I have been saying; and, however, it will afford me an illustration of it. For, not to mention many things, of what I elsewhere recite my self to have seen in amber, I have now by me a fine piece of clear and solid amber, (presented me by a person no less extraordinary than it) in which is included a large intire fly, in shape and size much like a grass-hopper, but variously and curiously coloured, with his wings displayed.

To these observations I shall add only this; that I have had my self, and shewn to others, one of that sort of pale amethysts, that some call white amethysts; which had been cut, to be set in a ring, or turned into a seal, and was, like that sort of gems, so hard, that I could readily cut glass with it; and yet in the body of this stone there appeared to be a considerable number of things, that looked just as if they had been hairs, some of them lying parallel, and others inclining to one another; and having contemplated them, as well by day-light as candle-light, and in divers positions in reference to the light and the eye, some of them seemed at times to be of a lovely reddish colour, but reflecting the light, as if they were well filled either with air or water: but for the most part they did, as I was saying, seem to be hairs of a brownish colour, which made the stone not a little wondered at, even by curious and skilful men. I leave you to judge, whether it will be fit here to add, that I have sometimes suspected, that even in diamonds themselves there may possibly be found intercepted, or mingled with a pure lapidescent substance, some particles of heterogeneous matter. And that in this suspicion I was somewhat confirmed, as by the odd clouds I had observed in an extraordinary diamond, and by some hydrostatical, and other observations I made about those stones, (some of which I found heavier than either crystal or white marble;) so by my having purposely demanded of an ancient cutter of diamonds of great practice and experience, whether he observed not a sensible difference of weight among diamonds of the same place: for to this he replied, that he had, especially in those, that were cloudy or foul; insomuch, that shewing me a diamond, that seemed to me to be about the bigness of two ordinary pease, or less, he affirmed, that he sometimes found in diamonds of that bigness about a carrat (which is by common estimation four grains) difference in point of weight.

Sixthly, the last argument I shall employ to shew, that the matter of divers gems may have once been fluid, may be taken from the proofs you will meet with (in the following part of this tract) of the second member of our hypothesis. For if it shall appear, that several even of the transparent gems have metalline or other extraneous mineral bodies mingled with them, *per minima*, it will be very agreeable to reason to suppose, that such a mixture was made, when the mingled bodies were in a fluid form; since, beside that one may well ask, how else the metalline corpuscles came to be conveyed into such compact and hard bodies as gems, it is very easy to conceive, if our hypothesis be admitted, and very hard otherwise to apprehend, how among bodies, that differ *toto genere*, as metals and stones, there should be made mixtures so exquisite, as many of these appear to be, partly by the uniform coloration of the gem, and partly by the diaphaneity retained notwithstanding this dispersion of mineral pigments through the whole mass; and in many instances also by the curious figuration, that we have lately been discoursing of.

POSTSCRIPT.

To all the foregoing circumstances, I can now add something, that I met with, since I thought to conclude with the last of them, and that tends highly to the confirmation of our hypothesis. In a tract, that makes part of a small book freshly published in French, principally to acquaint men with the ways of estimating gems, according to the rates of modern jewellers, the anonymous, but curious author, takes occasion, to give us, from the mouth, as he affirms, of the famous late travellers he conversed with in divers places, and whose relations are indeed the recentest I have seen in print, an account of the number, and names of the places, where diamonds and rubies are found in the *Indies*; adding some circumstances and particularities about the qualities of the soil in those places, that I have not elsewhere met with. This author, then, speaking of the first of those three diamond-mines, which he makes to be the only ones in the *East-Indies*, having told us, that the stones are there found, some in the ground, and some in the rock, subjoins, that those that are drawn from the rock, or the neighbouring parts, have ordinarily a good water; but for those, which are drawn out of the ground, their * water partakes of the colour or soil, wherein they are found. So that if the earth be clean and somewhat sandy, the diamonds will be of a good water; but if it be fat or black, or of another colour, they will have some tincture of it. Nay, he immediately annexes, that if there be some black or red sand among the earth, the diamond will also have some grain of it. And elsewhere mentioning the second mine of diamonds, which the natives call gems, he admonishes his reader, that in this, as in the mine of *Visapour*, which is that formerly mentioned, the stones partake of the quality of the soil, where they are found; so that if that be boggy or moist, the stone will incline to blackness, and if it be reddish, it will have an eye of that colour. Elsewhere he tells us, that of late years there were found in the kingdom of *Golconda* store of diamonds, which were brought to the Nababe, or first minister of state, who forbad the making any further search after them, finding not one in the whole number to have a good water, all of them being black or yellow. But by the way, whereas this author affirms it as a clear truth, that as gold is the heaviest

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* Que s'il y a quelque sable noir ou rouge parmi la terre, le Diamant aussi en aura quelqu'un. *P.* 9.

heavieſt and moſt precious of metals, ſo diamonds are the hardeſt, and heavieſt of all ſtones, he muſt excuſe me, if I declare, that what he aſſerts agrees not with my experience, who having tried the weight of an uncut diamond hydroſtatically, have taken ſuch a courſe to eſtimate its ſpecifick gravity, as I find not to have been yet taken by any other, and which you will eaſily grant to be more exact than any other of the known ways can be.

THE argument, that hath detained us all this while, compriſed ſo great a variety of matter, and may, I hope, perform ſo great a part of my taſk in this diſcourſe, that, though I ſhall not much apologize for having dwelt ſo long upon it, yet I ſhall think my ſelf obliged to make ſome amends for my paſt prolixity, by being ſuccinct in the remaining part of this treatiſe; and therefore, having left off with an intimated promiſe to ſhew more fully, that divers gems contain metalline, or other mineral ſubſtances, in them, I ſhould immediately connect thoſe arguments to what hath been lately ſaid, but that I think it altogether requiſite, to make way for what is to follow, by firſt taking notice of a main objection, that may be urged againſt the doctrine we have been propoſing.

THIS is taken from the figuration of ſome gems, and eſpecially the priſmatical one of cryſtal, and ſeems the more fit to be urged againſt us, becauſe we our ſelves have, in the ſecond of the above-recited arguments, given ſeveral inſtances of it. For it ſeems ſcarce poſſible, that ſo curious a ſhape ſhould be ſo uniformly produced in ſuch a multitude of cryſtals, great and ſmall, unleſs there were ſome ſeminal and plaſtick power to faſhion the matter after ſo regular and geometrical a manner.

BUT he, that ſhall attentively conſider, what I elſewhere ſay concerning the figuration of ſalts, and of metalline and other magiſteries diſſolved by, and concoagulated with ſalts, may be very much aſſiſted to diſcover the invalidity of this objection. But yet, becauſe I confeſs it is very ſpecious, if not important, I am content here to conſider it a little more particularly.

To this plauſible objection then I have two or three things to anſwer; firſt, that there is no abſurdity to conceive, that, if there be a ſeminal and plaſtick power in mineral bodies, it may be harboured in liquid principles, as well as otherwhere. For we ſee, that the ſeed of animals, which oftentimes, as in elephants, rhinoceroſes, &c. produces hard and ſolid bones, teeth, and horns, is at firſt but a liquid ſubſtance; and the formative power in ſome trees and their fruits does convert the alimental juice into woods, ſhells, and other bodies very ſolid and ponderous.

BUT ſecondly, I elſewhere * ſhew, that even in the figures of allom, vitriol, and other ſalts, that are ſo curiouſly and geometrically ſhaped, there is no neceſſity to fly to a diſtinct architectonick principle; but that thoſe bodies themſelves may receive their ſhapes from the coalition of ſuch ſingly inviſible corpuſcles, as by the motion of the fluid, wherein they did ſwim, and by divers aſſiſtant circumſtances, are determined to ſtick together rather in that manner, than in another. That this may be applyed alſo to other bodies, I ſhall need to ſhew in this place, by no other inſtance than that of the ſalt, that (in this or ſome other paper) I formerly told you I made of common ſalt, only by the help of oil of ſulphur, or of vitriol and water. For though it be manifeſtly a factitious body compounded of ſalt and ſulphur, and ſuch a body, that therein the ſea-ſalt, whereof it was chiefly made, has had its own nature deſtroyed; yet, by reaſon of the figure of the reſultant corpuſcles, and their fitneſs to convene, when diſſolved in water, into curiouſly ſhaped bodies, this factitious ſalt, when I have rightly prepared it, did ſundry times ſhoot into long cryſtals with points like diamonds, that did emulate native cryſtal as well in the regularneſs of the ſhape, as in the tranſparency of the ſubſtance. And to make it the more evident, that it was partly the figure, that happened to reſult from the operation of the oil of vitriol upon the ſea-ſalt, and partly other circumſtances, that determined the ſhape of the cryſtals; I ſhall add, that uſually, when the quality or proportion of the oil of vitriol was other than it ſhould have been, or an error was committed in ſome important circumſtance or other of the operation, the ſaline concretions, though they did not ſhoot at all like cubes, as the ſea-ſalt, which they were made of, would alone have done; yet they did not ſhoot any thing at all like rock-cryſtal, as did thoſe formerly mentioned; and for all this did, by reaſon of the curious ſhapes of the corpuſcles, they conſiſted of, ſhoot into cryſtals, for the moſt part, finely figured; though ſometimes of one ſhape, and ſometimes of another. And that you may not have any ſuſpicion, as if the regular figure, which ſea-ſalt is naturally of, is any way neceſſary to ſuch figurations, I will add an experiment, that I deviſed to ſhew, that even out of a petreſcent juice ſuch curiouſly figured bodies may be made. I took then ſome ſtony ſtiriæ, elſewhere mentioned to have been found in caves or grottos, where petreſcent liquors coagulated before they have time to fall down; and and having diſſolved them in ſpirit of verdigreaſe, I put the clear ſolution to evaporate in a digeſtive furnace, after the ordinary manner; by which means, though I made the experiment more than once, I had rather a coagulated maſs, than any thing like cryſtals. Whereby you may learn the truth of what I was ſaying, that a concourſe of divers circumſtances may be requiſite to determine the figuration of conſiſtent bodies, made out of fluid ones; ſince here, for want of time for making occurſions enough for the particles to concrete in after the moſt convenient manner, the experiment ſucceeded not: wherefore it being agreeable to my notions, that ſome ſorts of bodies may require a longer time to make ſuch a convention in, than others, I allowed many days to another ſolution of ſtiriæ made in the ſame men-

* See the Origin of Forms and Qualities, now publiſhed by the Author.

menstruum; after which there shot, as I desired, about the sides and bottom of the glass a number of distinct crystals, long, transparent, and curiously shaped, most of which, I think, I can yet shew you.

Perhaps it will be said, that the petrescent juice, when broken, does oftentimes appear to abound, within, with striæ, or narrow streaks like those of antimony; and that I myself observe some gems to be made up of thin flakes or plates; which internal figuration seems to be much more difficult to be accounted for, without a plastick form, than the external.

I will not reply to this, that, for aught I know, divers known salts would, when broken, appear to be geometrically figured, even in the lesser corpuscles, as well as they are evidently so in their entire bulk, if we had eyes quick enough to discern the shapes of the minuter, as well as of the bigger bodies. And we have great inducements to think, that whether or no *Cartesius* do rightly make the invisible particles, of which the smallest visible grains of sea-salt are made up, to be long and rigid like sticks; the minute visible concretions, of which the bigger grains of salt consist, are, as well as themselves, of a cubical figure; I will not, I say, insist on this reply, but proceed to alledge, that there are divers bodies so luckily shaped, that upon a slow coalition, they will convene into a multitude of manifest concretions; some of which will consist of streaks, and other be made up of flakes; as in the sal armoniack, commonly sold in the shops (for I speak not of the native, that is said to come from *Armenia*,) though it be avowedly a factitious body, you may often observe, upon breaking the bigger masses, great multitudes of streaks, like those we may usually observe in the broken striæ of petrifying water. And I have more than once seen, and also made, artificial concretions (of whose preparation I elsewhere speak) some of which consisted of salts alone, and others of salts and minerals, as stones or antimony, which look very like talk, being white bodies, made up of a multitude of very slender streaky particles lying long-ways one upon another, as in that mineral. And as I have taken out of earth many concretions, which, as they were for the most part outwardly shaped like rhombusses or lozenges, were composed of a multitude of flat and extremely thin plates; so I have sometimes taken pleasure to imitate such concretions by art. And though a solution of silver in putrified aqua fortis does usually afford only a great company of small, thin, and seemingly simple flakes, like scales of fish, because men have not any design like ours in procuring the concretion; yet having dissolved a good quantity of the metal together, and suffered it to shoot leisurely, and with due circumstances, I have obtained sundry crystals, which both were geometrically figured without, and consisted of a multitude of exceeding thin flakes orderly sticking to one another. And I remember, that whilst the objection, I am answering, was in my thoughts, I pitched upon a yet more pregnant experiment for the clearing of it. For considering how tin-glass, though a compact and ponderous body, does naturally consist of a multitude of shining polished flakes, (which may be easily perceived and distinguished by breaking a lump of it into three or four pieces;) I found by trial what I expected, that though a mass of this mineral were beaten to powder, yet if it were melted and suffered to cool of it self, the disposition of the component particles would determine them to stick to one another in broad and shining flakes, whereof many will be incumbent one upon another, and some cross to one another at various angles, according as the matter happened in its several portions to be diversely refrigerated. And some factitious bodies may afford us the like instances, as I have observed in some mixtures of copper, iron, and other minerals; and very conspicuously in good regulus martis stellatus, whose internal configuration may be found by breaking it; by which way I have observed with pleasure, that the regulus abounded with flat and shining flakes of an almost specular polish.

If it be urged, to confirm the former objection, that some lapidescent juices, even of those we mentioned in these discourses, do concrete, even whilst men are looking on; and yet our stony striæ, often mentioned (which probably may be also hastily coagulated) have in some places a streaky, and in other places an angular configuration of parts; I answer first, that I have seen divers of that kind of concretions, which, as far as the eye took notice of, were made up of parts confusedly jumbled together. And next, that (to consider now those, whose texture is more uniform) I have found by trials, that, if there be a due disposition in the component corpuscles of bodies to such configurations, they may be brought to concrete accordingly in a far shorter time, than almost any, that have not tryed, would expect not to say, believe. Having sometimes, for curiosity's sake, warmed six or seven ounces of aqua fortis, glutted with fine silver, 'till the mixture was all brought into a transparent liquor; and having then put the clear, but strong glass, that contained it, into cold water, that the menstruum might be the more hastily refrigerated, I observed, that when once the dissolved metal began to shoot, the coagulation into figured crystals proceeded so fast, that a naked eye could see the progress of it. And having sometimes put a quantity of salt and snow, or of some other strongly refrigerating mixture, into a convenient glass, and wetted the the outside with a strong solution of sal armoniack, or some urinous spirit, though in less than a minute of an hour it would be coagulated; yet the salt, into which it shot, had usually a curious and determinate figure, according to the nature of the liquor, that afforded it; as I have often shewn the curious.

Perhaps you will say, that these instances are taken from saline bodies, which are, for the most part, disposed to convene in smooth surfaces, and angular shapes, and easy enough to be wrought on by the external cold; and it may yet seem strange to philosophers themselves, what in some cases must have happened, if if our hypothesis be admitted, namely, that external cicumstances and accidents, such as the figure of a mold or womb, the coldness of the ambient, &c. should visibly, and sometimes not a little, diversify even the internal figuration of close and solid minerals and gems, without excluding all those, that are supposed to be of a quicker concretion.

WHEREFORE to clear this difficulty, it may not be amiss to subjoin an experiment, that I devised to shew, that if the corpuscles of a body be so shaped, as to be fitted by their coalition, to constitute smooth (and if I may so speak) glossy planes, though they be variously shuffled and discomposed, as to their pristine order, yet, if they be but a little kept in a state of fluidity, that they may the fitlier place themselves, or be placed by other agents, they will presently be brought to convene into smooth and shining planes, and the situation of those planes, in reference to one another, will be more uniform and regular, than almost any one would expect in a concretion so hastily made; notwithstanding which, their internal contexture will be much diversified by circumstances, as particularly the figure of the vessel or mold, wherein the fluid matter concretes.

CONSIDERING then, that, according to what I noted already, if we break tin-glass (taken for the bismuth of the ancient mineralists) as it is wont to be sold in lumps in the shops, it will discover a great many smooth and bright planes, larger, or lesser, according to the bigness of the lump; which sometimes meet, and sometimes cross one another at very differing angles: considering this, I say, I thought it probable, that a body, that had already been melted, and was apt to convene into such planes, not only would do so upon another fusion, but might have the order and bigness of those planes, diversified by the figure and capacity of the vessel, I should think fit for my purpose. Wherefore having beaten a sufficient quantity of it to powder, and, when it was well melted, cast it into a good pair of iron molds, whose cavity was an inch in diameter, we had a bullet, which being warily broken, did, as we expected, seem to be, as it were, made up of a multitude of little shining planes, so shaped and placed, that they seemed orderly to decrease more and more, as they were further and further removed from the superficies of the globe; and they were so ranked, that they seemed to consist of a multitude of these rows of planes reaching every way, almost like so many radius's of a sphere from the center or middle part, to the circumference: whereas, if we melt tin-glass in a crucible, and let it cool there, the matter being taken out and broken, will appear indeed full of smooth planes, but (as was lately intimated) very irregularly and confusedly associated or placed.

I will not now stay to enquire, whether the orderly composition of the planes in our bullet (which some curious persons, that I shewed it to, looked on, as a not unpleasant sight,) may be derived from this, that the matter was cooled first on the outside, by the contact of the cold iron mold, and the neighbourhood of the ambient air; and that the coagulation being once thus begun, the parts of the remaining fluid, as they happened to pass by this already cooled matter, with a motion, which, by reason of their removal from the fire, was now slackened, they were easily fastened against the already stable parts, (as may be illustrated by the concretion of dissolved nitre and allom, both about the injected sticks, and the grains, that first concrete against the sides of the vessel) and the refrigeration still reaching further inwards, till it came last of all to the middle of the globe, that being the remotest part from the refrigerating agents; the apposition was successively and orderly made, till the whole matter was concreted. But, (as I was saying) I must not now stay to inquire, whether the figuration of our bullet may be explained after this or some such way; or whether we are not to take in some subtle or pervading matter, or some other catholick agent? For though such points may be well worth discussing, and we may possibly elsewhere say something of them; yet here it may suffice to say, that we have varied the foregoing trial, by casting bullets of some other bodies, (and particularly the simple regulus of antimony) wherein it succeeded well enough, though the produced contexture were not so uniform as in tin-glass. And I also tried, that having cast melted sulphur itself into a globulous body of about five or six inches in diameter, and warily broken it, though one would think it an unlikely mineral to make any other, than a confused concretion, it presented me great fibres, almost like little straws, whose number, and, in great part, orderly situation, afforded me a much less unfit instance for my present purpose, than one would have lightly expected. But what I came from saying, may serve to make out what I propounded to my self; which having named already, I need not here repeat.

BUT one thing more there is, that may be pertinent on this occasion, namely, that I have broken divers marchasites of a peculiar sort, that were either of a roundish, or of an almost cylindrical figure, to observe their internal structure and qualifications; whereupon, I found in more than one of them (for I remember not, that I did in all) a great many rows of little planes or glistering corpuscles, reaching from the innermost to the external surface, and in those, that were somewhat cylindrically shaped on the out-side, these ranks of gold-coloured particles in the several planes of the broken mineral seemed like semi-diameters issuing out from a row of physical points, conceived to be placed on an imaginary line, lying almost like the axis of a cylinder between the opposite ends, (though I do not well remember, how near it reached to them;) as if the cavities of the chalk or clay, wherein these marchasites were found, had made the soil like a mold, wherein the matter of the marchasite being detained, whilst it was in a fluid form, did afterwards concrete much after the manner, that the bullets of tin-glass, regulus, &c. did in our molds. But the prosecution of this conjecture belongs to another discourse.

I

I shall therefore now proceed to a further answer to the formerly raised objection: wherefore, as to the exquisite uniformity of shape, which is so admired in gems, and is thought to demonstrate their being formed by a seminal and geometrizing principle; though I have, in the second of the above-mentioned arguments, ascribed to them such curious figures, as argue their having been generated after the way proposed in our hypothesis; and though also I willingly allow their shapes to deserve from us a delightful wonder at the curiousness of nature's, or rather her author's, workmanship; yet, upon a more attentive surveying of them, I do not find the uniformity to be near so great as is wont to be imagined; but have rather met with such diversities, as agree well with our hypothesis about their figuration.

In several transparent gems, it seemed manifest enough to me, (as I lately also noted) that the shape was, in great part, due to the figure of the womb, or mold, wherein the matter, whilst liquid or soft, happened to settle. In some other transparent and well figured gems of the same kind or denomination, and sometimes growing very near one another, by a diligent inspection I found a manifest and sometimes very considerable difference in their shapes, either as to the number, or the figures, or the bigness of the sides or planes, that made up the respective gems; or as to two, or all, of these; comparing these deviating particulars with what would have been in a stone of that kind or denomination, that were perfectly figured. This I had opportunity to take notice of, particularly in two sorts of stones; the first granats, of which I had a considerable number brought me out of *America* growing in one lump of matter; but in distinct parts of it, and without touching one another: among which I took notice of a manifest disparity of shape, and so I did in some African ones, that were presented me; as also in others, that were European, one of which, that was of an extraordinarily large size for a figured gem of a transparent kind, (for it weighed above eleven drachms and a half,) I considered with a particular attention, and found, that though it seemed to have been coagulated in a fluid medium, and to consist of twelve planes, at the concourse of two or three of which it seemed to have been broken off from the womb or root; yet it was very far from the dodecahedron of geometricians. For, whereas that consists of twelve æquilateral and æquiangled pentagons, almost all the planes, that made up our granat, were quadrilateral and very different from what regularly they should have been, not only in magnitude, but in shape: for one of them seemed to have five sides, and of the rest, some were most of kin to a rhombus, others to a rhomboides; but the most were but little better figured, than those, that the geometricians call the trapezia. And thus much for the first sort of gems, whose shapes I observed to be not regular. The second consists of those crystalline stones, which they call Cornish diamonds, and which are some of them much harder, than the Bristol diamonds, or perhaps, than rock crystal it self; it being easy to write upon glass with them. Of these stones having procured a good number (many of which I have yet by me) I took notice, by comparing them heedfully together, that though some of them were geometrically and curiously shaped like rock-crystal, having each six sides, whereof every two, that were opposite, were throughly like and equal enough to one another; and though the stone had a pyramidal termination, made up of several resembling and curiously figured planes, that terminated in a solid angle or apex; yet the greatest number, by much, of these titular diamonds was made up of stones, far from being so exactly and uniformly shaped, as those newly described. For though most of them had six long planes; yet oftentimes the opposite ones (besides that they were not so parallel to one another, as they should have been) were unlike and exceeding unequal; and those planes, that were to make up the apex, though apart, they were usually angular; yet being compared to one another, or to the regular patterns abovementioned, their figures, their bignesses, and their manner of concurring (which was sometimes not in a point or apex, but in a line,). was so remote from being uniform, that this great diversity and irregularity agreed far better with our hypothesis, than with its rival. And yet in these stones, the want of room to coagulate freely in, could not with probability be pretended; for they seemed to have been formed separately in a fluid ambient, save at the bottom, where they were fastened to the rock, as appeared by an opacous root, if I may so call it, which still adhered to most of them. And, if I much misremember not, I have more than once in diamonds, newly brought from the *Indies*, and some of them very fair ones, observed a great want of uniformity in the areas of the superficial planes, or in their figures, or both; and sometimes too in the very number, as well as situation, of their solid angles or corners: about which I hope to recover some notes. And so I have done with the first part of my answer to the abovementioned objection; whereby it may appear, that there is no such regular and constant uniformity in the shapes of gems, but that their real likeness may be reconciled to our hypothesis.

But now in the second part of my answer, I shall endeavour to shew, that the figuration of gems may not only consist with our conjectures, but confirm them. For I have, more than once, taken notice in the Cornish diamonds I have been mentioning, that sometimes a small stone of the same kind has made up, as it were, one body with a greater; so as that the lesser stone did not only adhere closely to the other, but was, if I may so speak, set or bedded in it. So that when the separation was made, there remained in the greater stone a cavity, whose figure did curiously answer that of as much of the smaller stone, as chanced to be harboured there. And, as sometimes I observed, that there was such an adnascency, (if you will pardon the word,) of a lesser stone to

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a much greater; so at other times, I met with the like of a greater to a much lesser, with a cavity in the lesser, answerable to that part of the greater, that had been lodged in it. Which, for aught I know, allows us with high probability to conjecture, that the stone, to which the other grew, was first formed and hardened; since it retained its own shape, and that, whilst this remained adherent to the rock or soil, some more liquor, either, that came afterwards by chance into the same cavity, or (in case it were there before,) that was less disposed to an early concretion, began to be coagulated by fastening itself against the solid body, that was already concreted: upon which account these two diamonds must stick close together, and yet be but contiguous, and a cavity, such as I freshly mentioned, must be left in the last concreted gem. Which may be illustrated by putting into a strong solution of pure nitre, or rock-allum, some little sticks of wood or any solid body, that may be kept steadily in the posture; for you will see many coagulations begin to be made against them, and the crystals thus concreted will necessarily have their figures incomplete, and have in them cavities correspondent to those parts of the stick, whereto the saline corpuscles fastened themseves. To which I shall only add, that though I have given instances of the adnascency of figured stones only in Cornish diamonds, yet they are not the only transparent minerals, wherein I have been able to observe it. And particularly I remember, that I observed among some minerals, left by a gold-smith to his widow, a fine transparent and neatly figured stone, which seemed to be pure crystal, but was coagulated about a kind of branching wire, whereof a good part was inclosed by the stone, that seemed to grow out of a piece of ore, that looked like silver-ore, and which the woman, that was a curious person, upon the strict enquiry, that I made, affirmed to be, together with the abovementioned branch, good silver, produced by nature in that form, (which I thought the more credible, because of the odd and almost hair-like shape, wherein I have seen silver-ore to have, as it were, grown;) which will excellently agree with the resemblance, I was just now proposing betwixt the coagulation of dissolved salts and the liquid matter of gems, about stable bodies partly immersed in those fluids.

THE very many circumstances belonging to our first argument, and the last answered objection, have so long detained us, that I doubt, you now think it more than time I should advance to, and dispatch the second of those grand considerations, whereon I at first intimated our hypothesis was founded; and this is built upon the weight of some gems, which being greater than that, which seems to belong to them, as hard and transparent stones, I think we may probably derive it from metalline or mineral mixtures.

I question not, but as you will think this allegation new, so you will be apt to question, how I come to know the truth of what I here deliver; since, though gems are wont to be estimated by lapidaries, as they weigh such or such a number of carrats, or of grains, yet they compare only the weight of this and that stone of the same kind in reference to one another, as the greater or lesser weight argues the greater or lesser bulk, without looking after, or knowing how to discover the specifick gravity of several gems, which depends not on the greater or lesser bulk; as (if you know it not already) you will gather from what I am now going to relate.

CONSIDERING then with my self, that for my purpose, it was requisite to have a gem as free as I could get from the metalline mixtures, that I suspected many precious stones to have; and remembring, that rock-crystal, as it is by mineralists reckoned among gems, so it is hard enough, as I tried, both to cut glass, and to strike fire; and that its having so great a transparency, and its being devoid of colour, makes it exceeding likely to be free from adventitious mixtures; I pitch'd upon it as the standard, whereby to make a probable estimate of the weight of gems; and having hydrostatically, and with a tender ballance examined the weight of it, first in the air, and then in water, I found its weight to be to that of water, of equal bulk, as two and almost two thirds to one: which, by the way, shews us, how groundlesly many learned men, as well ancient as modern, make crystal to be but ice extraordinarily hardened by a long and vehement cold; whereas ice is bulk for bulk lighter than water, (and therefore swims upon it) and (to add that objection against the vulgar error) *Madagascar* and other countries in the torrid zone abound with crystal.

HAVING thus found the ponderousness of crystal in reference to water, when I met with a coloured gem, whose specifick gravity I guessed to be sensibly greater; I sometimes gave my self the trouble (for a trouble 'tis) to weigh them in the air and in the water, and so discover, whether I conjectured aright. And if its specifick gravity did much exceed that of crystal, I thought it a probable argument, that there might be some metalline or mineral corpuscles mingled with the stony ones of the gems, and that also it may probably derive its tincture thence. I will not tell you, that I then found many sorts of transparent stones much heavier than crystal: for, besides that the tryals were troublesome enough to make, I chanced to fall upon them in a place, where I had not any store and variety of gems to examine. But one instance among those that occurred to me, I shall here set down, because, being so notable, it may suffice to shew, that, as to some gems at least, my opinion of their having an adventitious gravity, and consequently ingredient, is very probable. I had some American granats, which I had a great and peculiar reason to believe had been once liquid bodies, and therefore thought them the more worthy to be examined; and finding their colour to be so deep, that they were almost opacous, and judging by my hand, that they were much heavier than pieces of crystal of the same bulk would be, I weighed them in a pair of nice scales in the air, and in the water, and found them, as I expected, to be almost four times as heavy as water of the same bulk, and consequently heavier by about a third part than, pieces of crystal, equalling them in bigness, would be. Whence so great an accession of ponderousness proceeded, I shall tell you, when I come to my next argument; to which I shall advance, as soon as I have noted, that though, when coloured gems have a greater gravity than crystal, 'tis a probable argument, that they have some metalline pigment, or other mineral substance, mingled with them; yet, if such gems have no such surplusage of weight, it will not follow, that their colour cannot proceed from any mineral tincture; since 'tis not unreasonable to conceive, that a mineral substance may be present in a liquor (such as the lapidescent juice,) that we suppose gems to be made of, even when it adds no manifest weight to the body, that harbours it; since I have observed (what is odd,) that a mineral water, which by its taste, its effects, and the colour it would strike, appeared to be richly impregnated with iron, being carefully by me examined hydrostatically, did appear very little, if at all, sensibly heavier than common water.

The third and last argument, I shall now make use of, is taken from hence; that out of divers medicinal stones, and even out of some fine gems, real and corporeal metals, or other mineral substances, may be extracted.

Of this argument, I shall at present say the less, because the further prosecution of it will be more proper in the second part of this discourse, where I shall be obliged to handle it, with reference to opacous gems, in which its force will best appear. And therefore I shall desire you to take notice, when you arrive at that part of the subsequent discourse, of those particulars, that may serve to strengthen the newly proposed argument: and if it be objected, that the bodies, there treated of, are opacous stones, not gems, I have these things to answer.

First, that divers stones, that are reckoned amongst precious ones, are opacous too; as the turquoise, the onyx, the sardonyx, &c. not to mention divers others, as cats-eyes, opales, &c. which are as it were semi-opacous. Besides I much question, whether diaphaneity be absolutely necessary to the essence, though it be to the beauty, of those precious stones, wherein 'tis usually found. And I might here make it probable by discourse, that transparency and opacity oftentimes depend but upon the manner of the pigment's dispersion thorough the stony matter of the gem, and the convenient or inconvenient situation of the pores, in reference to the beams of light. But waving this speculative argument, I shall rather take notice, that several precious stones, and even diamonds themselves, have sometimes great clouds, which make them in those parts almost (if not quite) opacous, without being thereby hindered from being true diamonds or gems, of this or that kind, to which their hardness, colour, &c. makes them appertain: and not to mention cornelians, agats, and some other stones, that we may observe to be (as the tinging corpuscles happen to be, in a due or an over great proportion, mixed with the petrescent matter, and to be uniformly or inconveniently mingled with it,) some of them transparent, and some of them semi-diaphanous; I have seen worn in a ring a sardonyx itself, that was transparent, as unlikely a gem as that is to be so. And as for granats, though you know, that both of them are diaphanous, yet I have had some figured ones, that seemed quite opacous; and I have others by me of several countries, (whereof one very remarkable for its large size and geometrical shape,) that are in some places diaphanous, but, as to the main bulk of their bodies, appear at least almost as dark as ordinary stones.

I further add, that I little doubt, but that experiments, not unlike those I shall hereafter tell you, I try'd to obtain mineral or metalline substances from load-stones, native cinaber, blood-stones, &c. might succeed in several other of the more ponderous gems, if it were not, that the glassy nature, or exceeding compactness of many of them, makes the mineral corpuscles, that are harboured in the stony and insoluble parts, to be inaccessible to our common menstruums. And when the metalline and mineral ingredient is very abundant, and the tincture of the stony parts not so very close, I question not, but even from transparent gems the adventitious ingredient may, in part at least, be dissolved. And to satisfy you about this matter, I shall now inform you, that having by the ponderousness of the lately mentioned kind of granats been induced to conclude them impregnated with somewhat metalline, and for that reason to think it fit to try, whether I could separate it from them, or otherwise discover it in them; I kept some of them (in a crucible) for a competent time in the fire, and found, that they had exchanged their colour, for one not unlike that of unbrightned iron; and having reduced them to very fine powder, and digested some acid menstruums and particularly rectified spirit of salt upon them, they afforded me a rich tincture: encouraged by which, I hoped, that, without their being previously burnt, they would in aqua regis afford a tincture, and accordingly I obtained from crude granats, (only reduced to very fine powder) a rich solution, which though in colour it somewhat emulated a solution of gold; yet partly by the colour of the burdened granats and partly by the taste of this solution, I supposed, that another metal was likelier than gold to be the predominant mineral; and having gently evaporated part of that menstruum, I obtained from some of the rest certain crystals, whose shape, by reason of their smallness and disorderly coagulation, I could not well determine; and touching with the tip of my little finger the uncoagulated portion of the liquor, this part of a drop being put to a great many drops of the infusion of gall, did so immediately turn it into a substance, that seemed full as black, if not blacker than ink, as you would, I think, have been somewhat surprized to behold.

Which trial I made to examine the conjectures I had, that one mineral (for perhaps it it was not the only, that helped to constitute these granats, was of a martial nature; which, if it were, I supposed it would, like other bodies, that participate of iron, afford with galls and inky colour. I tried also with a parcel of small and red transparent stones, which some guessed to be granats, others, more probably, rubies, that being finely powdered, they would in an appropriated menstruum, (made extraordinary strong) give a colour like that of dissolved gold. And that there were really some parts of the gem dissolved in the menstruum, appeared not only by the above mentioned colour, but by these two indications; the one, that having put some of this liquor to some of the same solution of galls, I just now spoke of, it produced indeed, at the very first, a dark colour, but not near so black as that of the granats, and in a trice let fall a copious precipitate, that was almost white: the other, that I was able to precipitate from it, by an urinous spirit, a reddish substance, which being suffered to dry in air, seemed to grow into bodies, in shape not unlike moss, and here and there small mushrons, all of them prettily coloured. And from certain granats, that were in some places opacous, as well as in others diaphanous, I obtained a solution, from whence the superfluous liquor being abstracted, the residue, which was deeply coloured, did in the cold afford me a kind of saline concretions, which yet were not large enough to enable one to determine their figures.

AND on this occasion, I hold it not unfit to intimate, that perhaps, if men had curiosity enough to make trials, there would be other transparent minerals found capable of being wrought on by appropriated menstruums. For I do not think, that every seemingly glassy contexture of a mineral makes it unfit to be wrought on: for though the clear spar, which in most of our western lead mines in *England* is found next to the metalline veins, be at least semi-diaphanous, and be of so glassy a contexture, that it usually breaks into smooth and glossy superficies, and looks like a talk, and also, for the most part, is made up of, and presently reducible into geometrically figured bodies, shaped like rhombusses or rhomboides; yet some other trials, that I have made with this spar, inducing me to suspect, that it was not indeed a talk, but a body of a much more open texture, I found I could dissolve it in several liquors, and particularly in good spirit of salt, which would presently work upon it, even whilst it was in lumps, and that without the assistance of heat; which observation may perhaps give some encouragement to such a curiosity as yours.

BUT by what I have said of the usefulness of menstruums, I would not have you think, that they are the only instruments, where with something metalline may be obtained from some gems: for in an other paper of mine (to which such trials more properly belong) you may find an account of some attempts of that kind by fusions and appropriated additaments. And however such trials may succeed with you, that aim at separating from a gem a metalline or mineral body of a determinate species; I can teach you an easy way, whereby I have (by the help of fusion) more than once manifested in the general, that there may be substances, partaking of a metalline nature, in some kinds even of transparent gems. And partly by the same way, and partly by some others, I have been able to determine probably enough, in some cases, that the mineral substance is predominant in it.

AND here, before I dismiss the first part of our essay, I think, I may possibly somewhat illustrate our hypothesis, if I briefly mention to you an experiment, I remember I once made to that purpose. And it was this: I reduced to powder some of those striæ, that I have often spoken of, of water petrified, as it were, spontaneously. I also considered with my self, that I had found spirit of verdigrease, (which I make without the tedious preparations, that *Basilius* and others prescribe, by barely distilling, without additaments, good French verdigrease, and rectifying the obtained liquor) I had, I say, found this menstruum to be, not only, as I elsewhere observe, a good solvent for many bodies, but also to be distilable from many of them, without leaving near so much of itself behind, as other saline solvents are wont to do: considering this, I say, I dissolved the stony striæ in this liquor; and having suffered some of it to evaporate away, and put the rest into a cool place, I obtained, as I expected, store of small, but finely figured and transparent crystals, that shot much after the fashion of those of the purer sort of nitre. With some part also of the stony solution I mixed, in a convenient proportion, a high coloured solution of copper, made likewise in spirit of verdigrease; and these two solutions being made with the same menstruum, and warily enough put together, did not precipitate one another, but afforded me, upon the evaporation of the superfluous moisture, among divers crystals, that were transparent and colourless, some, that were richly adorned with a greenish blue tincture of the dissolved metal. What trials I made by this way, little varied, to imitate nature, by associating into transparent bodies stony and metalline substances, I cannot now give you a full account of; since I neither have by me the notes I set down about those trials, nor think it fit to make this first part of our discourse more prolix, than I now perceive it to be already.

SECTION II.

Containing a conjecture about the causes of the virtues of gems.

WHAT has been hitherto delivered in the first part of our discourse, will, I suppose, make it allowable for me to be more succinct in the second. I shall now, therefore, proceed to those other considerations, which, being assisted by what has been already said, may, I hope, suffice, to keep our conjecture about the cause of the virtues of gems from seeming unreasonable.

AND

And my first observation shall be, that not only there is in the earth a great number and variety of minerals, already known by particular names; but probably there are very many others, that are not yet known to us.

The former part of this proposition will not be doubted by those, that consider, how great a multitude of metalline ores, marchasites of several sorts, antimonies, tinned glass, fluores, talks of various kinds, spars, sulphurs, salts, bitumens, &c. are mentioned partly by chemists, and other mineralists, and partly by those, that have given us accounts of museums and other collections of natural rarities; insomuch, that of only one kind of fossils, the diligence of some modern writers hath reckoned up between two hundred and two hundred and fifty; besides animal stones, as lapis bezoar, lapis manati, oculus cancri, lapis porcinus, &c.

And as for the second part of our proposition or observation, you will scarce deny it, though you consider with me but these two things.

The first is the small and inconsiderable proportion, that the perpendicular depth, that the generality of mines bears to the semidiameter of the earth, reckoned to be above 3500 miles; so that, though our globe were inhabited by some hundreds of millions of men more than now it is, and they had curiosity enough to dig mines every where, and consequently there were millions of inquisitive and laborious men, more than really there are, their spades and pickaxes would, except here and there, penetrate so little a way into the earth, that a vast multitude of fossils might, by lying deeper in the bowels of it, continue undiscovered.

And to this first observation I shall subjoin this second; that, as far as I have observed, almost every region affords minerals of its own, differing from those, that are taken notice of in other regions. And in particular countries, as in some shires of *England*, a curious and heedful eye may, I doubt not, observe several, that are not taken notice of by the inhabitants themselves; especially, if well-made borers were diligently and skilfully employed to pierce the ground, and bring up samples of divers fossils, that lie hidden under it. But having elsewhere discoursed of this matter, I shall here only tell you, in general, that in some parts of *England*, where I had more opportunity than in others, to exercise some curiosity about minerals, I met sometimes, in a small compass of ground, with a much greater variety than I expected, and several of them undescribed, that I know of, by any writer; of which sort I have received divers others from several parts, both of the old world and the new.

In the next place, I consider, that nature has furnished the earth with menstruums, and other liquors of several sorts, and endowed it with divers qualities. This I have already manifested in the discourse of subterraneal menstruums, whereto I shall therefore refer you; only taking notice in this place, that whereas water is abundantly to be met with under ground, and, for the most part, very copiously in mines, by which it is capable to be variously impregnated; this liquor itself, especially being thus altered, may, in some cases, act the part of no despicable menstruum, and, on some occasions, otherwise concur to the production of mineral bodies.

I further observe, that the subterraneal liquors, upon one account or other, (for we need not now particularly determine it) are qualified to work, either as corrosive menstruums, or as other solvents, upon many of the medicinal earths, and other minerals they meet with under ground: which minerals, having never been exposed to our fires, have their texture more open, and their parts more soluble, than those, that have been melted by the violent heats of our furnaces.

And that even common water will suffice to dissolve and impregnate itself, both with the saline, and, oftentimes, with metalline parts, that it meets with in its passage, is obvious enough in the differing tastes, and other qualities of liquors, that all pass for common water, whereof some is found better, and some worse than others, to brew, some to wash linen, some to dye scarlet, or other determinate colours; some to temper steel, and some for other uses.

But others, unquestionably more eminent instances, are given us by the mineral springs, whether thermæ or acidulæ, as authors distinguish those, that are actually hot, (as at *Bath*) and those, that are saline, and, for the most part, sourish (like those at *Tunbridge*, and the *Yorkshire Spa*;) of which two sorts, good store are enumerated by physicians and geographers; and of which a far greater number would be discovered, if men wanted neither skill nor diligence. And here I shall desire you to take notice, that, though common water do the most readily dissolve the salts more properly so called, though not altogether pure, it meets with in the bowels of the earth, as we see it happens in those salt-springs that come not from the sea; yet there are also many other subterraneal bodies, which, upon the score of their abounding with saline particles, will be dissolved by water, though they be of a compounded nature, and contain very differing substances; as it is plain in those waters of *Hungary*, and other regions, which, by the evaporation of their superfluous moisture, will yield vitriol, a mineral not only compounded, but decompounded, as containing in it a saline, a sulphureous, a metalline, and an earthly part, (which, itself, I have found to be none of the simplest bodies;) every one of which may be made distinctly to appear.

Lastly, I consider, that the petrific juice or spirit coming to be in a sufficient proportion mingled with these impregnated waters, so as to coagulate them, and concoagulate with them; from their coalition may result those precious stones, that we call transparent gems. For it is certain, that bodies, that were a while before in the form of waters, may coagulate into stony stiriæ, of whose odorousness and

and reducibleneſs into lime, I have already given an account in my diſcourſes of lapideſcent juices; of which you may command a ſight. And that even diamonds themſelves, the hardeſt of gems, were once fluid ſubſtances, the firſt part of this diſcourſe has, I hope, evinced.

To which I ſhall now add, that procuring ſome petrified bodies to be brought me from a place in *England*, which I could not be admitted to, I found, that the petrific juice or ſpirit, that abounded in the earth of that ſpot of ground, was ſo penetrating, and ſo operative, that it made ſome of the vegetable ſubſtances, that were found in it, in their priſtine ſhape, and, for ought I could perceive, bigneſs, hard enough to cut glaſs, as well as grave on iron. And it was among theſe rarities (if I much miſ-remember not) that I picked up a (moderately) tranſparent body (which, I think, I have yet by me) that, by the ſhape, and other circumſtances, I judged to have been a diaphanous gum, belonging to one of the pieces of petrified wood, that had been brough me, and was hardened to a degree, that made it capable of ſcratching glaſs.

AND now to bring home theſe things to my preſent ſubject, I conceive, that ſome, at leaſt, of the real virtues of divers gems, may be derived from this, that whilſt they were in a fluid form, or, at leaſt, not yet hardened, the petreſcent ſubſtance was mingled with ſome mineral ſolution or tincture, or with ſome other impregnated liquor; and that theſe were afterwards concoagulated, or united and hardened into one gem, as a diamond, a ſapphire, a granat, an onyx, a blood-ſtone, &c. And as divers of the virtues of gems may be, in a general way, deduced from the commixture of theſe mineral corpuſcles; ſo the greatneſs of thoſe virtues, and the variety of thoſe properties, in particular, may be aſcribed to the peculiar nature of the impregnating liquors, to the diverſity of them, and to the greater and leſſer proportions, wherein they are mixed with the petreſcent juice.

To render this conjecture (for I propoſe it as no other) thus ſummarily and briefly expreſſed, the more probable, it will be fit to recall to mind the arguments, whereby we have already ſhewn, both that gems were once fluid or ſoft bodies, and that divers of them were not ſimple concretions of a petreſcent liquor, but conſiſted alſo of other mineral adventitious corpuſcles: which may appear, partly by the ſeparableneſs of ſuch ſubſtances from ſome gems, (as we exemplified in granats) partly by the ſpecific gravity of others, and partly by the differing tinctures (whereof one, at leaſt, may well be ſuppoſed adventitious) to be met with in gems of the ſame ſpecies, as rubies, ſapphires, granats, and even the hardeſt ſtones, that we yet know of, diamonds themſelves; of which (as is before noted) I have ſeen ſome yellow, and that to a great degree, ſome of other colours, but not ſo vivid; and ſome green, almoſt like emeralds.

Now ſince there may be in gems, and, in ſome of them, abundantly ſuch adventitious corpuſcles; and ſince there is cauſe to think, that ſome may be endowed with divers properties, and medicinal virtues; ſince alſo there is a great difference among theſe impregnating particles, and, probably, of a greater variety of them, than is known to us; ſince, laſtly, divers gems are not ſparingly, but richly impregnated with theſe ennobling corpuſcles, I ſee no ſufficient reaſon, why ſome of the virtues of divers gems are not more likely to proceed thence, than from thoſe unintelligible and precarious ſubſtantial forms, to which they are wont to be referred.

BUT becauſe there are ſome difficulties, that the objections of others, or my own thoughts, have ſuggeſted againſt our hypotheſis; though I neither have time, nor do think it very neceſſary, to diſcourſe amply of them: yet to clear the way for what I am afterwards to repreſent, I ſhall (though I can but briefly do it) ſay ſomething to each, that may, perhaps, appear no inſufficient anſwer; eſpecially after I have declared, as I here do once for all, that I ſpeak of the true and medical virtues, that belong to gems; and that, as to thoſe magical, and other extravagant properties, that either notoriouſly fabulous, or other credulous writers have made bold to deliver, I am ſo far from pretending to afford them an explication, that I do not allow them the leaſt degree of aſſent.

THIS premiſed, let us conſider the chief difficulties themſelves; among which, I doubt not, but it will be objected, that it is not credible, that the mineral ſubſtances, wherewith our hypotheſis would have gems to be impregnated, ſhould have any medical operation at all on the human body, in regard, that they are ſo locked up, that they can communicate nothing to it, eſpecially being indigeſtable and unconquerable by ſo ſmall a heat, as that of the ſtomach and other parts of the body.

BUT to this ſpecious objection I have ſeveral things to return by way of anſwer. And firſt of all; had there yet never been any actual trial made, whereby to know, whether a gem be capable of having any medical virtues, I confeſs I ſhould find probability enough in the objection to ſuſpend my judgment, till experience ſhould determine the queſtion. But ſince upon the very credible teſtimony of eminent phyſicians and patients themſelves of my own acquaintance, I find much leſs cauſe to diſbelieve, than to aſſent to ſome matters of fact about the operations of gems; and ſince ſuch matters of fact do ſtrongly argue, in the general, that a precious ſtone may have medical virtues; I think, the objection, as it is propoſed in general, is ſufficiently enervated by ſuch particular inſtances, and ought not to keep us from believing upon experience the poſſibility of the thing denied; eſpecially ſince there are other things beſides, that may be alledged in favour of our hypotheſis.

FOR it may be conſidered in the next place, that vigorous load-ſtones emit copious and very plentifully effluvia; and yet, beſides that ordinary magnets are uſually a very hard ſort of ſtones, I have met with ſome load-ſtones much

much harder than ordinary ones, and poſſibly than divers gems. And it is farther conſiderable, that there are load-ſtones, (ſome of which I can ſhew you) which do not only work upon iron, and other magnetical bodies, but have a manifeſt and inconvenient operation upon human bodies, by being worn in men's pockets, or long held in their hands; as thoſe, that have reſented ſuch operations themſelves, and obſerved them in others, have complained to me; which I might confirm by ſome analogous obſervations, if I had time to relate them.

But now I proceed to obſerve, that among tranſparent pebbles, ſome of which, you know, are, by being barely well cut and ſet, made to counterfeit diamonds, I have found ſeveral, that may be brought, in a trice, to emit copious, and even ſtrongly-ſcented ſteams. And if you allow the opinion of the generality of modern philoſophers, who aſcribe electrical attractions to the effluvia of bodies excited by rubbing, you will, I preſume, allow me to infer, that very light alterations may ſuffice to procure expirations, even from tranſparent gems: many of which are electrical, and ſo are the hardeſt of them, diamonds themſelves; one of which I keep by me, that, upon a little friction, attracts vigorouſly enough to be wondered at by the ſpectators.

And as to that part of the objection I am anſwering, which contends, that gems are not to be digeſted or conquered by the heat of the ſtomach; I will not ſtay to examine, whether, and how far, the digeſtion of things in the ſtomach be to be aſcribed to heat, contenting myſelf to ſay at preſent, that, to make the objection valid, it ſhould be firſt proved, that ſuch bodies cannot have any operation upon the human body as paſs thorough it, without any ſenſible change of bulk, figure, &c. as gems, that are ſwallowed down, are ſuppoſed to do. For, we know, that ſome chemiſts make bullets of the regulus of antimony (which we alſo have made, and obſerved ſomething odd about them) which they call pilulæ perpetuæ, becauſe, when they have performed their operation in the body, and have been ejected with the excrements, they are by ſome more thrifty, than cleanly perſons, waſhed, and employed again and again to the former purpoſes. Nor do we know, what analogy there may be between ſome juices in the body, and ſome of the mineral ſubſtances, that impregnate gems with their virtues.

For, though the oculus mundi be reckoned, by claſſic authors, among the rare gems (as indeed good ones may be juſtly accounted rarities;) yet, if one of the beſt ſort be but a while kept in common water, it will, as experience aſſures me, receive an alteration obvious to the eye. I might here alledge the concurrent authority of many, and the common practice of moſt phyſicians, who, in their publick diſpenſatories, as well as private preſcriptions, ordain the fragments of precious ſtones to be taken inwardly, upon the ſcore of the cordial, and other virtues, they aſcribe to them. But I ſhall rather make uſe of leſs queſtioned arguments, and, without inſiſting on the manifeſt operation, that the juices of the body have not only on the chalibeat preparations, where the metal is preſumed to be opened, but upon crude ſteel itſelf; or urging the examples of Lazarus vitri-vorax, or the devourers of ſtones, as being rare ἰδιοσυγκρασίαι; I ſhall proceed to acquaint you, that with a faint liquor, diſtilled from a vegetable ſubſtance, as temperately qualified, and as plentifully eaten as bread, I have obtained, and that without heat, from divers hard bodies, and amongſt them, from a tranſparent ſort of gems, a manifeſt tincture. And whether ſome juices of the body, aſſiſted by the natural heat of it, may not, in reference to ſome gems, ſerve for extracting menſtruums, though it may well be more than either I, or the objectors, certainly know, yet the inſtance, I come from alledging, favours our hypotheſis more than theirs.

And even the natural heat of a human ſtomach, nay, perhaps, the outward parts of the body may be able, though not to digeſt precious ſtones, yet to ſolicit out ſome of their virtues; ſince I am ſure, it makes a ſenſible alteration in the hardeſt ſort of them. For I have a diamond, whoſe electrical faculty may be excited not only by rubbing, but, without it, by a languid degree of adventitious heat. And I have had in my keeping a diamond, which, by water made a little more than lukewarm, I could bring to ſhine in the dark.

Object. If it be further alledged, that, though ſome virtues may be conceded to gems upon the account of the minerals, that impregnate them, yet it will be no way likely, that their virtues ſhould be ſo various and great, as even the modeſter ſort of authors pretend: if this, I ſay, be alledged, I ſhall readily acknowledge, that I do not think others, or myſelf, obliged to believe all the ſtrange things, that even ſome learned writers do ſometimes aſcribe to gems: And if any man will think, that ſome of them are fabulous, and more of them hyperbolical, he may ſooner find me his aſſociate, than his adverſary in that point. For the rarity of tranſparent gems, their luſtre, and the great value, which their ſcarceneſs, and men's folly ſets upon them, imboldens ſome to ſay, and inclines others to believe, that ſuch rare and noble productions of nature muſt be endowed with proportionable, and conſequently, with extraordinary qualities.

But this being freely granted, I anſwer to the objection; firſt, that 'tis not improbable, that there may be in the earth a much greater variety of minerals diſſoluble by the ſubterraneal menſtruums, and capable of concoagulation with petreſcent juices, than authors have yet taken notice of: to which conjecture divers ſubterraneal productions, that I have met with, do ſtrongly incline me. And from the number and various mixtures of theſe may proceed, not only a great variety of operative particles in precious ſtones, but a high degree of energy in ſome of them.

And next I conſider, that the efficacy of thoſe mineral tinctures or ſolutions, that are already

already known to us, and may be concoagulated with the petrescent juice, may be reasonably presumed to be much greater in some gems, whereof they became ingredients, whilst they were (as chemists speak) *in solutis principiis*, than may be expected in our shops, or laboratories, from the vulgar solutions of the same metals or minerals, after they have, by vehement fires, been reduced into gold, or silver, or lead, or antimony, &c. For whereas, in these vehement fusions, requisite to bring metalline or other ores into such substances, the volatile and spirituous parts are wont to be driven away, and the remaining body becomes more hard and compact, and has his virtues, as it were, locked up; in the state of fluidity those subtle and efficacious parts are preserved, and united to the other ingredients of the gems, whence some emanations of them may be easily enough drawn out; as in the instance I not long since mentioned, of the easy eduction of strongly scented streams from pebbles so hard, that I found them more disposed to strike fire, than flints themselves, that are used in guns. And from the greater or less plenty, and natural activity of the impregnating particles in this or that gem, may, probably, be deduced the difference in colour of some, and in virtue of other stones of the same denomination: of which we have, in a learned writer or two, eminent examples given us, of the great virtue of some, and the inefficacy of other, that experience has discovered, among those stones, that go under the title of lapis nephriticus. For, though they be not properly transparent gems, yet the analogy betwixt them, and those that are, seems sufficient to warrant the mentioning of them on this occasion.

See *Unzerus* de nephritide.

AND here we may subjoin two things, in favour of both the foregoing answers: the first, that for aught we know, the petrescent juices themselves may have all, that is requisite to make them such, and yet have distinct natures, and be endowed with peculiar qualities, abstracting from those, which they acquire upon the score of their coalitions with adventitious liquors. This I cannot stay to make probable by the differences I have observed in petrescent fluids, and therefore I hasten to the second.

THE next thing, which I would represent, is, that having observed petrific liquors, or spirits, to pervade and give a high degree of hardness to bodies, that chanced to lie within their reach, though one would have thought them sufficiently indisposed to receive such an induration; I see no absurdity in supposing, that, sometimes, such a liquor may invade, permeate, and subdue transparent minerals, abounding in saline, sulphureous, and bituminous particles; which consequently being duly excited, may be made to emit their more subtle and more active parts. And as I have cause to think, that subterraneal fires and menstruums do divers times make various compositions and decompositions in the earth, (as it were not hard for me to shew, if I had leisure;) so it is not impossible, but that the spirit, we have been speaking of, supervening, may mingle itself with such bodies, and petrify them together with itself into gems. On which occasion I remember, that I have had salt, made by nature in the bowels of the earth, just like that, which chemists compound by art on the surface of it. And I have sometimes made, by an easy operation, and a moderate degree of fire, a certain composition of volatile particles of salt and sulphurs (some of which I have yet by me) which, after distillation, did, in a fluid medium, shoot into crystals transparent, and more curiously figured, than I have seen divers natural gems to be. So that, if either beneath, or upon the surface of the earth, such kind of substance happen to be pervaded and subdued, by a clear petrifying liquor; we may well presume, that the resulting concretions may be indued with qualities, as well uncommon for the kind, as considerable for the degree.

Object. If it be yet objected, that it is very unlikely, that gems should part with any effluvia, or portions of themselves, since they lose not of their weight, and some of them are very little heavier than crystal itself, and consequently are not like to have much adventitious substance to part with; I might leave the answering of one part of the objection to physicians and chemists, who teach, that the antimonial glass and cup imbue wine, and other liquors, with a strong emetic quality, without any sensible loss of weight. But having elsewhere spoken of those things, I shall rather here demand, whether the objectors have tried the truth of what their argument supposes by any way sufficiently accurate? For I much doubt, that that has neither been attempted, nor would be found easy to be performed. And till due trial be made, let me represent, that though they will not allow common water to be a menstruum fit to draw any thing with, from such a body as mercury, which is wont to mock the chemists aqua fortis and aqua regis; yet both *Helmont*, and others, inform us, that mercury kept for a day or two in common water, or boiled a while in it, though it be taken out without any sensible diminution of weight, or bulk, will have imbued a considerable quantity of water with a virtue of killing worms; for which purpose, it is much used, and often with good success, in a great hospital in *London*, as the chief physician of it (a very judicious and experienced man,) has more than once informed me.

AND as for the lightness, that is objected against some gems, besides that it may safely be granted, that, *cæteris paribus*, such may have fewer or more languid virtues, than others of the same kind; it may also be answered, that the adventitious substance, that impregnates the petrescent juice, may be of so small specifick gravity, as not to make the gem at all heavier in specie, than crystal itself. For this, (as we have formerly observed,) being about two times and a half heavier than common water of the same bulk, I have hydrostatically found, that divers salts and some other mineral substances are of less specific gravity; and consequently, if they were concoagulated with the petrescent juice, that hardens into crystal, need not increase the ponderousness

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of it, and yet may embue it with considerable virtues: nor is it necessary (to add that *in transitu* on this occasion) that, not to alter even the colourlessness of crystal, or the colour of another gem, the adventitious substance should be purely saline: for I have divers times made bodies, which though transparent and colourless like crystal, and sometimes curiously and regularly figured, were yet of a compounded nature, and particularly abounded with an easily separable and strongly scented sulphur. But to give yet a farther and more direct answer to the objection; I shall add, that though when a gem has much more specifick gravity, than crystal, or will suffer an adventitious mineral to be separated from it, it is a very probable argument, that the petrescent juice is that body compounded with an adventitious substance; yet it will not necessarily follow, that, when neither of these signs appear, the gem is quite devoid of any such substance. For, (according to what I elsewhere declare,) the petrescent liquor, it mainly consists of, may be impregnated, not with the grosser substance, but with the finer and more spirituous part of the mineral, without having the specifick gravity sensibly encreased. Of which I remember, I shewed a notable instance to some curious persons, at a mineral spring, which many were then drinking of by the advice of learned physicians for several diseases. For, though this water, both by its inky taste, by its blacking the excrements of those, that drank it, and by other signs appeared to participate richly enough of iron; yet the ferruginous particles, it abounded with, were so light and spirituous, that not only they would, as I tried, be easily lost, if the liquor were kept too negligently stopped; but when I came, whilst the spirits were yet there, (it being but newly taken from the spring it self) to examine it hydrostatically with very good scales, and much diligence, I convinced the virtuosi, that assisted, that this ferruginous water was very little, if at all, heavier in specie than other water, which was brought as common water to be compared with it, and examined with the same scales, and after the same manner.

And now, if you recal to mind what I have elsewhere said, partly of the atmospheres of solid bodies, and partly of the great efficacy of effluviums; I hope you will not think it absurd to conjecture, both, that some precious stones may have medical virtues, and, that divers of these may be ascribed to the mineral substances, whereof they participate or consist; and especially to those, which are best fitted to exert their powers, by the copious effluxions of their more agile and subtle parts.

And by this time it may be seasonable to tell you, that though what I have hitherto discoursed do chiefly belong to transparent gems; yet divers of the things already delivered may, with no great alteration, be applied to opacous gems: of which I shall speak much more briefly, not only for the reason just now given, but because, if we have shewn, as I hope we have, that even diaphanous gems may be endowed with virtues by the mineral substances they contain or are in part, made up of; the arguments will hold more strongly as to opacous gems: both because these are, for the most part, much less hard than the others, and because it is far more easy to shew, by their specific gravity, and the compoundedness of divers of them, that the dark ones, than it is, that the clear ones, may partly, and sometimes plentifully, consist of mineral substances, embodied with, and hardned by petrescent juices, or petrific spirits.

In favour of this doctrine, I shall endeavour, in the first place, to shew, that what has been delivered is possible; and afterwards, set down some particulars to make it very probable.

The first part of my task might be easily performed, or, perhaps, would be needless, if I were sure you had no need to be told of any thing I have written about lapidescent juices. But for greater security I shall, in this place, briefly intimate, that, among the kinds of those liquors, I have observed a sort, that is of so fine a substance, and yet of so petrifying a virtue, that it will penetrate and petrify bodies of very differing kinds, and yet scarce, if at all, visibly encrease their bulk, or change their shape or colour. To which purpose, I remember, that I have seen divers animal and vegetable substances so petrified, as scarce, at all, to be taken notice of, by their appearance to have been altered by the operation of the petrescent liquor. I have, with pleasure, seen a thin cream-cheese turned into stone, where the size, shape, and colour, even of the wrinkles, and the blueish mold (which, it seems, it began to have, when the liquor invaded it) were so well preserved, that an hungry man would not have scrupled to have fallen upon it for a good bit. And as for the hardness, that this petrescent juice can give to the body, that it penetrates, I shall now only remind you of what I lately told you; that I have had, and, I think, yet have, in another place, a pretty quantity of wood petrified in *England*, which retaining its former figure, and grain, and scarce at all visibly encreased in bulk, was so very hard, that I could make impressions with it upon iron, and glass it self, and make it strike fire like an excellent flint. To which I shall here add, that the stony parts did not suffer the wood, which they had penetrated, to be reduced in the fire, either to ashes or charcoal. And I have by me a lump of mineral substances, wherein a petrescent liquor, that fills the large intervals between them, is transparent enough, and harder than most stones, as far as we could guess by some trial of it made by a skilful engraver of gems.

And to these instances might be added many others, if it did not by these few sufficiently appear, that petrifick agents may insinuate themselves into the pores of various bodies, and turn them into stone, without otherwise destroying their pristine nature, or so much as their former figure.

Wherefore having in general shewn our hypothesis to be possible, we may now descend

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to four or five particular arguments, that it is hoped may help to render it very probable. And these I shall fetch, partly from the great specific gravity of divers opacous and medicinal stones; partly from the fitness of our hypothesis to render a reason of divers phænomena relating thereunto, some of them scarce at all, and others much less probably to be accounted for without it; partly from the metalline substances to be manifestly separated or obtained from the stones we are treating of; and partly from the nature of the bodies, whereof medicinal stones seem to be compounded.

Arg. I. THAT the specific gravity of divers opacous stones, whereunto medicinal properties are ascribed, is very considerable, is a truth, which if those, that have written of such concretions, had been versed in hydrostaticks, and had had the curiosity to examine them that way, they might have easily discovered; as will quickly appear by particular examples: before the mention whereof, it will be fit for me to take notice to you, that considering with my self, that white marble is generally allowed to be a pure and solid stone, and upon the score of its whiteness is likelier, than most others, to be free from mineral mixtures; I thought, I might at least as well pitch upon that, as on any other, for the standard of the specifick gravity of opacous stones, as they are merely such. And accordingly having weighed a piece of white marble in air and water, I found it to be in weight to an equal bulk of that liquor very near 2 $\frac{72}{100}$ to 1, or, that the proportion with very little error may be the better remembered, as two and seven tenths to one. And to make trial in a stone uncoloured, but, because harder, supposed to be of a closer texture, we examined a fine white pebble, which we found to be to an equal magnitude of water, as two and above six tenths to one. This being determined, it was not difficult for me to think, both that divers bodies, that commonly passed for mere stones, are more ponderous than white marble of the same bulk; and that if there were any such great surplusage of specific weight, as I guessed, many will be found to have above that of marble, it might proceed from some metalline body, though not visibly, yet really, and perhaps plentifully mingled with the petrescent matter of these stones. The latter part of this conjecture will hereafter be confirmed in the third argument; which makes it unnecessary for me to give you now of the former more than a few instances: which I shall soon dispatch by telling you, that I quickly found by weighing the following minerals, first in the air, and then in the water, that a blood-stone (bought at the druggist) was in weight to water of the same bulk at 5 $\frac{7}{10}$ to 1; the loadstone, I then tried, (for all are not equally heavy in specie) as 4 and $\frac{6}{10}$, to 1; lapis calaminaris, used for rheums in the eyes, and to turn copper into brass, as 4 $\frac{7}{10}$ to one; lapis tutiæ, as they call it, which is also much employed in rheumatick eyes, as very near 5 to 1.

BUT here I must advertise you, that I have not found the proportion of each of these bodies and water to be any thing near constantly the same, but sometimes to differ very much in particular stones of the same kind; which agrees very well with our hypothesis. For, according to that, those particular stones, that happen to partake more plentifully of mineral substances, heavier in specie than stone, as such needs to be, ought to be more ponderous than others of the same kind, that are not so qualified: I said, heavier in specie than a stone, as such need to be, because there are substances, that are reckoned among minerals, and are capable of endowing the stony matter, wherewith they are coagulated, with medical virtues, and yet those substances may make the stone or aggregate, whereof they are made, not to be heavier, but lighter in specie. From jet, which in some parts of *Europe* being found in quarries of mines, is indeed a fossil, which is wont to be reckoned among stones, and by many worn as a gem, I obtained no inconsiderable proportion of oil; and having weighed choice jet itself in water, I found it to be, bulk for bulk, to that liquor, but as 1 $\frac{22}{100}$ to 1. And there are some other fossils, hard as stone, and polishable as marble, from which I have by distillation obtained two kinds of oil, whereof one was lighter than common water; which shews, that even bituminous and light substances may be ingredients of a stone: and that salts, which are most of them less heavy in specie than white marble, may plentifully concur to the making up of stones, I shall have occasion to manifest at the close of this discourse by those stones, whereof we in *England* use to make vitriol. The foregoing reflection I have here touched upon, because I would intimate to you, that stones, that are lighter in specie than white marble, may be compounded of fossils, whence they may derive peculiar qualities, at the same time, when I tell you, that in my opinion such stones, as are considerably more heavy in specie than marble, may afford us a strong presumption of their owing their gravity to the mixture of metalline or mineral substances. And this may suffice for our first argument.

Arg. II. THE next shall be taken from the consideration of some phænomena, (relating to medicinal stones) which agree very well with our hypothesis, and will scarcely be very well explicated without it.

AND, first, as to transparent gems themselves, I have learned by inquiry of travellers, that have visited those parts of the *East Indies*, where they grow, that sometimes one sort of gems, sometimes another, and sometimes also diamonds themselves, are found included in the rocks where they are digged for, or in the midst of hard loose stones, which must be broken in pieces, to take out the diamond, or other inclosed gem: which phænomenon will be hard to be accounted for, unless by our hypothesis; according to which it may rationally be supposed, that the gem was first formed either in earth, or some other soft and easily permeable substance, which being afterwards pervaded by some petrific juice or spirit, was turned into rock or loose stones, according as the

the earth, and other ambient matter, chanced to be an intire and coherent mass, or divided into clods and other portions. And I remember, that the governor of an American colony, having sent me among other rarities, digged up in his country, an odd kind of mineral, that seemed more ponderous than at first sight it promised, I had the curiosity to break it, and found in it, here and there, several gems, which by their figurarion and some other circumstances, were concluded to have been formed there, before the ambient mineral had obtained the nature it then appeared to be of. And in opacous stones, it may hence happen, that a great lump of medicinal earth may be invaded and petrified after the newly mentioned manner; so that it may not be thought incredible, that some of these medicinal stones should be very large in comparison of others: As I remember, that an ingenious physician told me of a spleen-stone, as they call them, in the hands of an acquaintance of his (where I might have seen it, if my occasions had permitted,) amounting to about fourscore pound weight. And on this occasion, I also remember, that even in a medicinal stone, much harder and heavier than marble, and whereof I have seen lumps far greater than I could lift; I remember, I say, that having had the curiosity to cause a pretty big piece, violently broken off from the mass, whereto it belonged, to be sawn asunder, that I might consider the internal textures, as far as it was visible; I found several empty cavities of differing sizes and figures in the solid substance of the stone, (which I think I have not yet lost:) which seems to argue, that this compact and ponderous body was made of a stony nature, by the supervening of some petrescent liquor or spirit upon porous earth, or some other consistent substance. For if it had been a mere liquor, wherein those cavities must have been so many aerial bubbles; it is not like, that some of them should have such irregular shapes, and that all should have continued, without emerging to the top.

Secondly, our hypothesis will also help to render a reason of what seems exceeding difficult to be explicated; namely, how some gems, that seem to be intire stones, are in part of one colour, and in that, which is contiguous to it, of a quite differing: of which sort we have the sardonyx, and some other opacous gems. And I have observed the like, though very rarely, in diaphanous ones. For, according to our hypothesis, it may be said, that a portion of matter, imbued with one of the tinctures of the parti-coloured gem, was first formed, and afterwards, some petrescent juice, endowed with another colour, came to settle contiguously to it, and so by accretion made up one stone with it. I might illustrate this by telling you, that though fire do make a far greater agitation of bodies melted by it, than need be supposed in cold petrescent liquors, yet I have found in making artificial gems, that by some mischance or error in the operation, the mineral pigment has richly tinged one part of the transparent mass, without at all imparting that colour to the very next part to it; so that if I should shew one of those I have yet by me, you would judge it to consist of two differing gems subtlely glewed or fastened together, unless you should in vain try, as others have done, to discover by the eye or otherwise, some naked commissure, which may keep those so differingly coloured bodies from making up one intire mass.

But let us leave these artificial gems, and add to what I was saying about our natural ones, that the union of parts in these resulting stones (if I may so call them) I was speaking of before, might be the more perfect, if the supervening matter found not the first formed stone to have attained to its full induration: though, for aught I know, even in this case, the apposition may be so close, and the two matters so near of kin, that both may pass for one stone, and be polished both together, without any blemishing discontinuity of surface at those parts, where one would expect commissures. For I have by me a lump, wherein there plainly appear stones of colours very different from each other, that were once distinct and incoherent; but by some petrescent liquor have had all their intervals so exquisitely filled up, that neither the touch, nor the artificers tool, the lump being now sawn asunder, discovered any commissures; but the whole mass bears an uniform polish, and is harder than divers gems, that are worn in rings, readily enough striking fire with a steel. And to confirm this the more, I shall add, that in a place, where a prying person of my acquaintance lighted on this portion of petrified matter, he found not only other lumps, but divers loose stones, that seemed altogether of the same nature with those, that by the supervention of the petrescent liquor were united into stony masses. I have also had a curious agat so formed, that it seemed highly probable, that the opacous parts of its matter had been some thin, but not altogether contiguous, beds of fine clay, or earth, lying almost parallel to each other (but not to the horizon) which by some petrescent liquor, that chanced to settle there, was reduced to coagulate with it into a partly opacous and partly diaphanous stone. And of such clays or mineral earths, I have sometimes with pleasure observed more than one or two, which, though distinct, and perhaps of differing colours, were so very thin, that the thickness of them all did scarce exceed an inch, nor did they always lie flat or horizontally, but in differing postures, both in reference to the horizon, and one another, and now and then the exterior ones did successively almost surround the interior: and of these thin couches or layers of earth, I remember, I have observed a considerable number within a very small compass of ground. I must not in this place stay to shew, how probable it is, that much after the same way may be explicated the production of divers other gems besides agats, as chalcedonians and jaspers, which are for the most part opacous, but oftentimes have some parts, that are not so. But I am content, before I go further, to mind you, on this occasion,

ſion, of what I elſewhere deliver, that by purpoſely calcining, without breaking, ſome of theſe ſtones, whoſe greater part was diaphanous, I found, that the tranſparent parts turned white; and that ſome of the thin layers, or couches of mineral earth, had retained their colour, as well as poſition, and had it much heightened; ſo that one of theſe layers, after calcination, was of a very rich and permanent red. And this difference of colours I obſerved not only in layers, but in the ſpecks, and irregularly ſhaped clouds, if I may ſo call them, of other colours, as greeniſh, blueiſh, &c. I might here add, that I have found ſhining marchaſites, not only in other ſolid ſtones, but in marbles; as alſo flints themſelves, incloſed in great maſſes of marble, and likewiſe wood; in ſtrong ſtones imployed to build a wall, and ſhells, at leaſt as was judged by their ſhapes and ſizes; in a great maſs of ſtone, that I met with almoſt on the top of a hill remote from the ſea, together with divers other ſuch phænomena, which I think may probably be accounted for by our hypotheſis, and ſcarce without it. But being willing to diſpatch this diſcourſe, and unwilling to intrench upon the diſcourſe of the effects of the petreſcent juice, to which the conſideration of theſe and divers other phænomena, to be met with about the generation of ſtones and petrified bodies, eſpecially in wombs or molds, more properly belongs; I ſhall in this place only point back to one obſervation, and anſwer one objection; becauſe both of them are pertinent to our preſent diſcourſe.

THE obſervation is this: that even in tranſparent gems, and which is more, of the ſelf-ſame ſpecies, I have ſometimes taken notice of ſuch an aggeneration, or accretion of ſtones, to one another, as argues their having been produced at ſeveral times. For proof of this, I need no more than refer you to what I have not long ſince related about thoſe Corniſh diamonds, wherein, ſometimes, a leſſer ſtone, though geometrically ſhaped, was found in good part incloſed in a greater, as well as in part alſo extant above it. Whence I argued, that the production of this aggregate of two cryſtalline bodies was not made all at once, but ſucceſſively, and that the leſſer was firſt formed, which I ſhall now confirm by this conſideration. That if the greater ſtone had been firſt hardened, the matter of the leſſer muſt only have exteriourly ſtuck to it, and been, as it were, imboſſed upon it; but could not have made itſelf, in the ſubſtance of the greater, a bed or mold, eſpecially of ſuch a geometrical figure as itſelf had not yet received.

AND though this ſucceſſive generation of the parts, of ſeemingly entire gems, may appear to you ſomewhat new and ſtrange; yet, that its fitneſs and requiſiteneſs to explain the foregoing phænomena, and others, to be hereafter mentioned, may the more recommend it to you; I ſhall add, that, perhaps, you may be aſſiſted to conceive, if not invited to admit it, by a mechanical illuſtration. For we ſee, in divers chymical ſolutions, as of ſalts, and other bodies, that there are certain ſtages, or periods, of coagulation; ſo that, when ſuch a quantity of the ſuperfluous moiſture is exhaled, eſpecially upon any conſiderable refrigeration, or other favourable circumſtance, thoſe particles, that are moſt diſpoſed to coagulation, will convene, and ſhoot into cryſtals; after which, no more will do ſo, till a farther and more conſiderable evaporation of the water, or other menſtruum, be made; upon which will enſue a new cryſtallization of the parts. And I can ſhew you the productions of a metalline, but uncommon ſolution, that I ſo made in an appropriated liquor; that the firſt ſhooting afforded me a layer, or bed of curiouſly figured cryſtals, and the following another layer of fine cryſtalline bodies, that have faſtened themſelves to the former, but differ notably from them, both in ſhape and poſture. And in this experiment, the diſſolved body was but one, as the menſtruum but one; but if there be a diverſity of nature in the liquors, that make up a menſtruum, or in the bodies, that are diſſolved in it, ſome of the corpuſcles may convene either a part with thoſe of the ſame nature, or mingled with thoſe of and differing nature; but yet at the ſame time, and ſo make up cryſtals of a compounded nature; and ſome of them may convene with homogeneous particles, but at differing times; and ſo miſs of ſuch uniformity as might elſe appear in their concretions. Which may be illuſtrated by what I have elſewhere related concerning the cryſtallizations of ſalt-petre, and ſea-ſalt, diſſolved together in ordinary water; where, moſt commonly, grains of ſalt of reſulting figures are produced; and alſo a conſiderable part of the ſea-ſalt coagulates in the form of imperfect cubes, about the bottom, before the nitrous corpuſcles ſhoot into cryſtals of their own almoſt priſmatical ſhape. And I might further add, that it matters not, whether the ſuperfluous water be waſted by exhalation, or by being drained by a body fit to ſoak it up; as we have had occaſion to obſerve in accelerating the cryſtallization of ſome bodies, where I was not willing to employ the heat of the fire, by placing, underneath the ſolution, dried earth, or ſome other porous and ſoaking body.

WITH ſome analogy to ſuch inſtances as theſe, we may conceive, that where there are petreſcent liquors, mingled with common water, there may, by divers accidents, and particularly an hot ſummer, a ſufficient diſcharge be made of the ſuperfluous moiſture, to make the more diſpoſed parts of the petreſcent liquor to coagulate; and afterwards the coagulation may be ſuſpended, either by the ſupervening of a colder ſeaſon, as winter; or even in ſummer itſelf, by a plentiful rain, or the effect of it, a land flood, which might check the progreſs of coalitions, by overmuch diluting the liquor, that might elſe have turned into ſtone. Not to mention, that trial hath aſſured me, that there are bodies, and thoſe of very differing kinds, which will, in tract of time, eſpecially if their coalition be furthered by cold weather, coagulate, after they have long remained in a fluid form, though the water, or other menſtruum, by being incloſed in ſtopped glaſſes, be kept from waſting. And ſince the earth harbours differing kinds of these liquors, as I have elsewhere shewn, and divers of them may be copiously impregnated, some of them with one sort of mineral, and some with another; we may conceive, that they may have distinct periods for their respective coalitions, and yet may stick close to one another; in regard that, though in our chemical crystallizations the artists are wont to take out of the vessel, what shoots the first time, before they make a fresh exhalation of the water for a new crystallization, and by this means, have the coagulated bodies, that they obtain at one time, more uniformly shaped; yet in the hollow receptacles, that the earth affords to petrescent liquors, the vessels continuing the same from first to last, the uniformity of the bodies produced by coalitions, made at several times, must be less regular; and the manifest accretions or aggregates of coalescent bodies must, in all likelihood, be more frequent. And accordingly having suffered the exhaling of some liquors to be continued in the same vessel, I had coalitions of very differing bodies at the bottom.

What I was not long since saying, makes me remember, that, in order to a satisfaction, which the event gave me, of the conjectures I had about the successive concretions of some solid fire-stones, that were not suspected to be other than intire and uniform masses, I caused two or three, that I thought likely, and of very different sizes and shapes, and brought from distant places, to be warily broken: which trial gave me the pleasure of observing, that the internal texture of the least of these minerals, which was almost spherical, was very differing from that of the more internal part of the substance of the stone; and that in the other, and greatest mineral, there was a little globulous stone, that manifestly was not of the same piece with the invironing mass, differing from it not only in texture, but here and there by a discernable commissure: though in most places, their adhesion was so strict, that we could not make any separation of the two minerals by the help of this commissure. The greatest part of this double fire-stone I keep by me, and shall say nothing of what I further observed in it, having mentioned what I said already but upon the by.

I might add, that in some circumstances, even in close vessels, and therefore without any manifest exhalation of the water, or other menstruum, and, sometimes, where the dissolved body was homogeneous, I have, in process of time, had coagulations, where the last formed crystals seemed plainly to have been generated by way of accretion to the first.

Difficulty. Having now done with my observation, I shall endeavour to clear a grand difficulty, which, I foresee, may be objected against our hypothesis; namely, that these aggenerations, if I may so call them, of medicinal, and other stones, are sometimes found in places, where there are no petrifying springs, and perhaps, no springs, or other waters at all; nay, little or nothing but quarries, or other masses of stone.

But to this I answer, first, that if we admit of the relations, that I elsewhere mention out of approved authors, concerning men and beasts turned into stone by a petrifying spirit, that suddenly invaded them, it will not be absolutely necessary, that there should be any petrescent springs, or other-like water, to produce such minerals, as we are now discoursing of.

Secondly, for aught has yet been shewn to the contrary, we may suppose, that rainwater does sometimes bring along with it such petrifying particles, as may serve our turn. In confirmation whereof, I shall add, that having, of a learned and judicious person, enquired after divers particulars relating to a famous bath, by him visited in *Hungary*, whose water abounds very much with petrescent particles, over which there is very high building erected, I learned by his answers, among other remarkable things, that to the roof, or upper part of this tall structure, there were fastened many long stony concretions, (like those wont to be employed to adorn grotto's) which he affirmed to be, from time to time, generated there; not, as I at first suspected, by the dashing up of any drops of water; (which he averred, could not reach any thing near so nigh) but by the copious petrific steams, that being there checked in their ascent, did, according to their natural propensity, coagulate into stone. Whether this relation may warrant me to guess, that, in some places, stones may be generated, without the help either of rain, or springs, by the ascent of petrific particles, in the form of exhalations, from some lower parts of the earth; which exhalations, suffering the lighter steams, that accompanied them to exhale, may operate upon some disposed materials, that they find in their way, and turn them into stone: whether, I say, this narrative may well suggest this conjecture, I shall not now stay to examine, though the earthy, and sometimes sulphureous sediments, that have been observed at the bottom of rain-waters, suffered to settle in clean vessels, may seem to favour it; and though also I might illustrate it by what I observed in a bottle of distilled liquor, whereof no part would naturally ascend in a dry form; for having kept this vial well stopped in a safe and quiet place for a year or two, I observed, that the ascending steams had quite pervaded the cork, and had formed, at the top of it, numerous whitish striæ, slender, but of a length, that surprized me.

Thirdly, there is no necessity, that in all soils, where petrific waters are to be met with, there should be petrifying springs, at least above ground. For I have caused to be digged store of figured and transparent stones in a certain earth, that lay upon the upper part of a rock, and seemed to be a very dry soil: perhaps you will allow me to tell you, that I have, by pouring a solution of stony striæ, made with spirit of verdigrease, on a convenient quantity of bolus Armenus, and suffering the soft mixture to remain in a glass in the open air, till the superfluous moisture was exhaled; I have, I say, by this means, imitated in a little,

little, what I have been now relating, and found small, but untinged and figured crystals dispersed through the little cavities of the red earth. But it will be more considerable to our present purpose to add, that the fairest and hardest petrifying wood, that I ever had, or tried, was taken up by an ingenious person I employed in a plot of sandy ground, where he could not find any petrifying, or so much as any other spring. To which I know not whether I should add, that supposing the ground to have been once moistened with a lapidescent liquor, whether brought thither by springs, or any other way; one may, in our hypothesis, well enough account for this difficult phænomenon, that now and then, not only in the surface of the ground, and perhaps upon rocks themselves, there are found aggregates of figured stones, that seem to grow upwards, as it were, from a root; which much puzzle men to know how they came there, and may incline them to their opinion, who ascribe vegetations to stones. But to this may be answered, that many of the concretions, we are speaking of, may have been formed in wombs, that lay, though not deep, yet under ground, or in shallow cavities in the surface of it; and that after their formation, the looser earth, that surrounded them, may have been washed off by rains, blown off by winds, or otherwise removed, leaving behind them these stones that adhered firmly to a solid body. Besides, if I had time, I think it were possible for me to shew, that stony concretions might be produced by the mechanical action of the air, upon the stony particles that successively apply themselves to the matter, that first begins to coagulate, when they are ready to be forsaken by the moisture, that accompanied those particles, and was necessary to their due application to the casual rudiments (which pass for roots) in imitation whereof, I have, more than once, obtained both from saline and stony solutions, dry tufts of prettily figured, and diaphanous or white, but very slender, stiriæ, if I may so call them, that seemed to grow out of the solid glass, and made men wonder how they came thither, no water, or other liquor, appearing near them.

FOURTHLY, It may very well happen, that the petrescent liquor may be so mingled and diluted with ordinary water, as not to be distinguished from it by the generality of men, nor to be capable of disclosing it self by its effects, till either by the copious exhalation of the common water, or by some peculiar advantages, it has to operate upon bodies, it has opportunity to discover it self. On which occasion I shall add, that there is a lake in the North of *Ireland*, wherein I could never hear but that fishes lived as well as in other lakes, and yet there are some rocks near the bottom of it, to which there fasten themselves divers masses and other pieces of a finely figured substance, and transparent as crystal; of which an eminent person, the chief owner of the lake, presented me with some, and promised me more. Now, if we suppose, that either by springs of petrescent water, or by rains, or by subterraneal steams, or otherwise, waters, resting in any hollow place, though upon the top of rocks and mountains, shall be sufficiently impregnated with petrifick particles; and that afterwards, in process of time, the merely aqueous parts shall be, by degrees, by the heat of the sun, the soaking of the grounds, the winds, or the continual action of the air, brought to exhale away in the form of vapours, the petrifick particles, which are not so volatile, will turn the soil beneath them, and on the sides of them, as far as the sphere of there activity reaches, into stone harder or softer, of this or that kind, according to the particular nature of the petrescent liquors, and the structure and other dispositions of the soil they invade: in which soil, if there chance to be lodged bodies heterogeneous to it, whether vegetable substances, as roots, pieces of wood, gums, &c. or the whole bodies of animals, as toads, frogs, serpents, fishes, &c. or their parts, as shells, bones, &c. or minerals of an open texture, as boles, unripe ores; or else gems or stones of another kind already formed; any of these things, or any other, that shall chance to be lodged there, must be found either petrified, or enclosed in stone, when this changed and hardened shall soil come to be broken up. Nor is it at all necessary, that this petrefaction of the extraneous bodies, and of the soil or bed, be made at once; for it may well be made successively at several times, according as some parts of the petrescent juice happen to be more copious and penetrant, and consequently more fit to be soaked in further than other. For, as the porousness happens to be greater in one part of the soil, than in another; or, as the texture and disposition of particular bodies, lodged in the earth, gives advantage to the petrifick particles to work on some of them, sooner, or in a differing manner, than in others; so the induration of the pervaded matters may be very unequally made in point of time, as well as in other circumstance. So that (to omit many other things explicable by it) we may, from what hath been already delivered, conceive, how it may happen, that medical stones of very differing colours, consistencies, and operations (of which I have several by me, that I had from the same mineral mass,) may be generated, and seem entire bodies, though (as in some, that I found) the difference is great, that so one part of the medical stone is dark, heavy, and opacous, and the other much lighter, transparent, and quite otherwise coloured. And upon the same principle may be explained, what I lately mentioned to you about the finding of diamonds enclosed in loose stones and even in rocks; of which we have credible testimony: which seems not more strange to me than a stone, which I have by me, which being a kind of pebble, contains in it a perfectly shaped serpent, coiled up, but without a head, which appears to have been formed before the stone, in regard, that in the upper and lower parts of the solid stone there are cavities left, which together make up one cavity, just of the size and shape of the contained body; to which, as it was easy for the matter of the stone, whilst it was yet a soft body, to accommodate it self exactly; so it is scarce conceivable, how, if the pebble had been first formed, the inclosed animal, if it were one, or the matter whereof the seeming animal afterwards was formed, should not only get in, but find a cavity so curiously shaped, and so fitted to its bulk. And that this variety was produced at several times, might be further argued from this, that the seeming serpent is plainly of another and clearer kind of stone, than that of the mold, that encompasses it; and of the mold itself, one part, contiguous to the included body, is whitish, and abounds in shining grains or flakes; in both which, it differs from the other and far greater part. And now it will be time to hasten to the

FIFTH consideration, which is, that, for aught we know, in those very places, where now there is nothing to be seen but loose stones, and perhaps beds of stones themselves, that in those very places, I say, there may in times past have been petrescent liquors, whether stagnant or running. For, I elsewhere * shew, (to another purpose) that earth-quakes, inundations of seas and rivers, sinkings of ground, encroachments of the land on the water, fiery eruptions and other such accidents, (some related by authentick authors, and others happening in our own times, in places, some of which I had the curiosity to see) have, among other odd effects, been able to dry or choke up pools and lakes, and to stop and quite divert the course, not only of springs, but of rivers, so as to leave no footsteps of them, where they plentifully flowed before. Upon the score of which transpositions of notable quantities of terrestrial matter, and other great changes of the structure and disposition of the soil in divers places, it may well be suspected, that the stony wombs or molds, wherein the above-mentioned bodies were found, were heretofore, at some time or other, of a muddy or earthy nature, and were receptacles of petrescent liquors, which, at several times, turned the whole mass of the soil into stone, before the springs, or other waters, containing the petrific liquors or spirits, were quite consumed, or had their course altogether diverted. But though I could say much more to confirm and apply this, and the preceding considerations, yet having spent so much of my time already, I shall not only leave all that unsaid, but, to make some amends for having staid so long in clearing this difficulty, I shall do little more than name the two remaining arguments.

Arg. III. It agrees very well with what we were formerly saying (in the first argument) about the great specific gravity of such, as the newly-mentioned stones, in comparison of that of white marble, or transparent pebbles, that it should be possible, out of those minerals, to extract some of that substance, whether metalline, or of kin to it; upon whose account, I told you, I supposed them to be so ponderous. And accordingly we have, by appropriated menstruums, obtained from the forementioned bodies, and not from those only, solutions or tinctures, which, besides that, by their colour or taste, they discover themselves, did, upon their being dropped upon a solution of galls, or some other convenient liquor, or upon their being examined by other proper ways, produce such changes of colour, or such determinate phænomena, as argued them to abound with metalline, or mineral particles, which, for the most part of them, I observed to be of a vitriolate nature; so I found, that the solution of a blood-stone, which tasted very rough upon the tongue, would, with the infusion of galls, make an inky mixture; and the like would also be made with load-stone, emery, marchasites, &c. opened with corrosive menstruums. But the solution of lapis calaminaris, which was of a golden colour, did not operate like the rest on the infusion of galls; but yet by its taste, as well as colour, sufficiently discovered itself to have copiously impregnated the menstruum. And now the mention of lapis calaminaris minds me, to take thence an instance of what I lately intimated, that there may be other ways, besides that of dissolutions in proper menstruums, to shew, that some medicinal stones participate of metalline and mineral substances. For it is by melting lapis calaminaris with copper, and keeping them together for a competent while in fusion, that brass is made; wherein the red colour of the copper is changed into a golden one, and the absolute weight (for I speak not of the specific gravity) considerably increased. Nor is this the only mineral stone, from which I have, by a way quite differing from those I have yet mentioned, namely, with running mercury, obtained a metalline substance. And though native cinabar, used by eminent physicians both inwardly and outwardly, be looked upon by the vulgar as only a red stone; yet it is known, in the quick-silver mines of *Friuli*, and some other places where it abounds, that it is a mercurial ore, whence, by vehement fires, they distil running mercury, which we, by moderate ones, have sometimes done.

BUT here perhaps it may not be improper to tell you, that though before any admonition given men of the expediency of examining stones hydrostatically, I could not receive from others, yet I made against myself the following objection, that there are some stones, to which useful qualities are ascribed, which are either not at all heavier in specie, than is requisite for a stone, as such, to be; or so little heavier, that it is no way likely, that metals, or any such ponderous minerals, should contribute either to their productions, or their virtues.

IN answer whereunto, I thought it may be said in the first place, that our hypothesis does no way oblige us to deny, that there may be such stones. For though it ascribes the virtues of

* In an examen of an experiment urged for the magnetism of the earth.

of most gems, and metalline stones, to the metalline and ponderous mineral substances they partake of, yet the concession agrees very well with our doctrine; which (as will in the fourth argument be more manifested) speaks in general, when it teaches, that the virtues of stones may, in many cases, depend upon their consisting not of a pure petrescent substance, but a substance impregnated with other minerals, which, though most commonly they prove specifically heavier than the petrescent matter, as such, without being the less, but rather, in some cases, the more operative and communicative of their virtues; yet, in divers stony concretions, the adventitious ingredients may be specifically lighter than the genuine matter of the stone; as may be easily gathered from some passages of the foregoing discourse. For, not here to urge, that divers bodies, that pass for stones, do abound in particles of salt, which may be much less heavy than pure stone of the like bulk, I have observed, that some other hard fossils abound with a kind of bitumen, which, when by distillation brought to an oil, is much less heavy than a stone of the same bulk. And, as I remember, I have had some portions of such oil, that would swim even upon common water; and, lest this should be ascribed to the subtilization, the bitumen received from the fire, I will add, that having hydrostatically weighed a piece of good asphaltum, we found it to be to water of the same bulk, but as 1, and somewhat less than $\frac{4}{10}$ to 1. Which was within a tenth of the proportion to water of a stony, though a bituminous fossil, commonly called in *England* Scots-coal. And because sulphur, as well as bitumen, is very apt, (and indeed, more apt, than before trial I expected) by even a moderate heat, or attrition to diffuse its steams, (usually rank-scented enough) I shall add, that there are variety of hard stones, which abound in sulphur: (witness, that in some places they obtain their common brimstone by sublimation thence) and yet having weighed a roll of brimstone in air and water, I found it to be but a fraction scarce worth mentioning above double its weight to the liquor; which shews it to be much lighter in specie than crystal itself.

AN improvement of this first answer may furnish me with the second. For hence we may argue, that it is not impossible, that the principal virtue of a light medical stone, should be due to some mixture of a metalline, or the like ponderous substance; since, if some of the ingredients, that are plentifully mixed with the true stony matter, be of the lighter sort, though there be also some metalline, or other heavy mineral particles mingled with the same matter, yet the specific levity of the one, in comparison of this matter, may compensate the specific gravity of the other; and they may all compose a stone, either less, or not more ponderous than white marble. On which occasion, I remember, not only that I found a blackish East-Indian flint, and likewise a black English one, to have to water not full the proportion of $2\frac{6}{10}$ to one, but that one of the first pieces of black marble, that I examined hydrostatically, was found, notwithstanding the darkness of its colour, to be to water of the same bulk, scarce any thing more than $2\frac{7}{10}$ to 1, which, you may remember, was the proportion I found between white marble and water, unless we should say, that this blackness of colour proceeded, not so much from any gross bituminous matter, imbodied with that of the stone, but from some mineral smoke that had pervaded it. And this puts me in mind of speaking something in this place about what might properly enough have been discoursed of long ago.

WHEREFORE I shall subjoin, in the third place, that it seems not impossible, that the matter, which medical stones are made of, may, before it comes to be hardened, derive various colours, and be imbued with virtues by subterraneal exhalations, and other steams. This, I fear, you will think somewhat strange, and therefore I shall briefly endeavour to confirm it by the mention of two or three particulars.

THAT then many places of the lower part of the earth emit copious exhalations into the upper, and even into the air itself, I presume you will grant, and I have elsewhere proved it. That also such subterraneal steams will easily mingle with liquors, and imbue them with their own qualities, may be inferred from the experiment of mixing the gas (as the Helmontians call it) or the scarce coagulable fumes of kindled and extinguished brimstone, with wine, which is thereby long preserved. And I have elsewhere mentioned, how I have incorporated this smoke with other liquors, wherein I observed its operations to be notable.

THAT beneath the surface of the earth there may be sulphureous, and other steams, that may be plentifully mixed with water, and there, in likelihood, with lapidescent liquors, I have also manifested in another * discourse.

THAT quick-silver may be in part resolved into fumes by less fires than many of those that burn under ground, will be readily acknowledged by chemists and gilders, and is obvious in the fumigations employed in the cure of the lues venerea. And that mercury may in the bowels of the earth be so disguised, and well mixed with stony matter, as to suffer the whole concretion to pass for stone, may be observed in some kind of native cinaber.

THAT sal armoniac, of which, in some places, there is to be dug up store, will, with a moderate fire, be made to ascend in form of exhalations, is vulgarly known, as to the factitious salt of that name; and I have found it to hold in the native. That common sal armoniac, sulphur, mercury, and tin, will be sublimed into a gold-like substance, that participates of most, if not of all the ingredients, may appear by the account I have elsewhere given of the way, I used, in making aurum musicum: and that even gold itself, the heaviest and fixedest of the bodies we know, may, by no great proportion of additament, and that with but a moderate fire, be made to ascend in the

* Of subterraneous steams.

the form of fumes, or even of flame, I have several times tried, by ways elsewhere delivered. And that mineral exhalations may be met with in the bowels of the earth, is witnessed by the relations of divers credible persons, conversant about minerals, that affirm themselves to testify what they write upon their own observation, to which some things, that I had seen myself, did the more incline me to give credit. And this copious ascension of mineral fumes, and even of metalline ones, may be much confirmed, not only by what is written by professed chemists, but by the learned and curious *Johannes Kentmannus*, who, in the useful catalogue of the *Misnian* fossils he had collected, amongst the pyritæ or fire-stones, reckons one, whose title is, *Pumicosus, & ab exhalatione ardenti nigro colore tinctus*; and another, whose inscription is, *Coloris argenti, qui ab exhalatione virosa colore cinereo est tinctus.* The same may be further confirmed by what I have somewhere met with, as related *in terminis* by the learned *Cabæus*, that he found in the territory of *Modena.*

To bring this home to our purpose, since there are mineral exhalations of very differing kinds, dispersed in divers places under ground, and since there are several volatile minerals, as arsenic, orpiment, sandarach, &c. that are very actively hurtful; there may be others endowed with medical qualities, and the exhalations of such minerals, either alone, or mixed with petrescent liquors, pervading duly-disposed earths, and bolusses, and other fluid, soft, or open substances, before their induration, may endow them with medicinal and other qualities.

Nay, when I recall to mind the old phænomena, that I have partly observed, and partly received from credible testimony, about the coalitions, mixtures, tinctures, and the emanations, as it were of those tinctures, in metalline, stony, and other fossile concretions; I dare not peremptorily deny, but that, even after subterraneal bodies have obtained a considerable degree of induration, and perhaps great enough to make them pass for stony ones, there may be subterraneal steams subtle enough to penetrate, tinge, and otherwise impregnate them. Which you would think the less impossible, if you reflect upon what I just now related out of *Kentman*; and especially, if I had time to add here, what I remember I elsewhere delivered about my trials to tinge native crystal with differing colours, by the fumes of volatile minerals. And that a very small proportion of a metalline substance, resolved into minute particles, may suffice to impart a tincture to a greater quantity of other matter duly disposed, may appear, by those factitious gems, wherein, with three or four grains of a skilfully calcined metal, or some such mineral pigment, we may give the colour of a natural gem to a whole ounce, or more, of vitrified matter. And I remember, that in subtiler fluids, I have made the instance by vast odds more conspicuous, having tinged with one grain, or less, of a prepared metal, as gold or copper, as much successively generatied phlegm, as, if it could have been all preserved, would have amounted to a bulky lump of deeply-coloured matter.

But your allowing the hesitancy I have expressed in this last paragraph is not necessary to my present purpose; wherefore I shall not borrow any thing to countenance it from another paper, but pass on to what remains.

Arg. IV. The last thing, that I shall represent to shew, that the virtues of opacous gems, and medicinal stones, may be more easily, than those of transparent ones, accounted for in our hypothesis, is this, that the main ingredients, whereof many such opacous stones consist, were complete mineral bodies before they became stones; some of them having been medicinal bolusses, or the like earths; some earths abounding with metalline or mineral juices; some, ores of metals, or minerals of kin to metals; and some, in fine bodies of other sorts, or natures, differing from these and one another. For all these several kinds of fossils may, by the supervening and pervasion of petrific spirits, be turned into stone; and consequently retain many of the virtues, they were indowed with by the mineral corpuscles, that had copiously, either under the form of liquors, or exhalations, impregnated them, whilst they were yet earths, or other bodies of a more open or penetrable texture.

I might illustrate this by the way I elsewhere mention, whereby I made such mixtures, even of stony and metalline ingredients, that, notwithstanding their coalition, were transparent; though you will grant that to be more difficult, than to compound such concretions when one is allowed to make them opacous.

But here I must obviate an objection, which I foresee may be made against our present fourth argument, unto which, even what I have been now saying, may afford a rise. For since it seems by our doctrine, that gems may be but magisteries, and consequently but such compositions, as, though made in the bowels of the earth, might be made or imitated by human skill, it may seem very improbable to many, that bodies so near of kin to artificial ones should be endowed with such peculiar, and, some of them, with such strange virtues, as are ascribed to divers gems, and are thought to be capable of flowing only from certain substantial forms, and those very noble ones too.

To this I might reply, that I admit not any such imaginary beings as the Peripatetic forms, which, I fear, they will never be able to demonstrate. But to avoid unnecessary disputes, I will rather answer in short, that such compositions, as are called artificial, may, for all that, be endowed with great virtues, and such as are called specific; witness the virtues of many chemical preparations, even of those, that are used by physicians of all sorts. And lest you should think, I need to fly to chemistry, of which some learned men are pleased to have a great distaste, I will name a couple of instances out of *Galen* himself; the one is the ashes of crayfish, to which, notwithstanding the destruction, that has been made of the pristine body by fire, he gives a greater commendation against the as strange, as fatal

poison

poison infused by the biting of a mad dog, than he does either to the fish itself unburned, or to any medicine of nature's own providing; and I hope, you will grant a virtue of that kind and degree, to be specific enough. My other instance shall be taken from treacle, which, though allowedly a factitious body, and consisting of I know not how many ingredients shuffled together, was yet, in the days of *Galen*, to whom a book is attributed about it, and ever since has been, the famousest antidote, in these parts of the world, and has been celebrated, not only for its alexipharmical virtues, which alone are sufficient to intitle it to specific ones, but for divers others, which are generally ascribed to it, some indeed upon the score of manifest, but others also upon that of occult qualities.

THE objection being thus dispatched, we may return to our medicinal stones, about which I shall venture to add, that, according to our way of explicating the production of them, a not impossible solution may be offered of this difficult phænomenon; that sometimes stones, that are thought, without scruple, to be of the same kind (as hath been particularly observed by learned men of the lapis nephriticus) are of such different qualifications, that some of them prove very considerable remedies in cases, where others prove almost utterly ineffectual. And I have observed also, though very rarely, that a medical stone may have virtues, that are taught to be the properties of stones of another kind. For, according to our hypothesis, when the stony matter is impregnated, as it ought to be, with those minerals, that in the ordinary course of nature belong to that species, its virtue will be such, as it should be for kind, but for degree may be very various, answerable to the plenty, purity, subtlety, &c. of the mineral, that impregnates it. But if the stony matter chance to be inbued with some other substance of a contrary nature, though, perhaps, the proportion of it may be so small, and the colour of it such, as not to make an alteration in the stone obvious to sense, and great enough to make it judged to be of another species; yet it may so vitiate the matter, wherein its expected quality resides, or check and infringe its operations, as not to leave the stone any considerable degree of virtue. And on the other side, if it happen, that the mineral corpuscles, that are wont to impart a certain virtue to the stony matter of one gem, should, by some lucky hit, be so united with that of another sort of gems (of which case I formerly gave an instance in green diamonds) though the quantity of this unusual ingredient may be but very small, yet, if its efficacy be great, it may innoble the stone with a notable degree of some such virtue, as is supposed not to belong to that species, but to another.

See *Unzerus* de nephritide.

AND on this occasion I shall add, that I know a gentleman, a professed scholar, who to the eye seems to be of a complexion extraordinarily sanguine: this person was for a long time so troubled with excessive bleedings at the nose, that, notwithstanding all the remedies he could procure in an academy of physick, where he lived, he was divers times brought to death's door; till at length his case growing very famous, there was sent him by an ancient gentlewoman a blood-stone, about the bigness of a pigeon's egg, with an assurance, that it had done scarce credible cures in his disease, by being worn about the patient's neck. Upon the use of this stone he quickly recovered his health, and had long enjoyed it, when I conversed with him, but yet so, that when he left it off any considerable time, his distemper would return. And when I seemed to suspect, that imagination might have an interest in the efficacy of this remedy, he answered, that he was very well satisfied of the negative; and particularly upon this trial, that he had, by the hands of a third person, that lived not far off, and whom he named to me, stopped a hæmorrhage in a neighbouring gentlewoman, whom the violence of the distemper kept from knowing, that any thing had been applied to her, till a pretty while after the blood was stanched. I shall not here mention other instances, though very remarkable, of the efficacy of this stone, which I had, both from the gentleman himself, and an intimate friend of his, who is a very learned man and a physician; because I have said enough to make it seasonable for me to tell you, that notwithstanding all the odd operations of this stone, when I came to look upon it, it was so differing in colour and texture from what I expected, that I should have taken it much rather for a gem of some other species, than a blood-stone.

TO confirm some of the particulars comprized in this our fourth argument, and shew the variety, and sometimes great plenty of mineral and other subterraneal matters, that may concur to the composition of bodies, that pass for stones; I shall observe, that the subtilty and penetrancy of some liquors, if duly considered, may evince it to be possible, that such bodies should be petrified by them, and with them, as may in part consist of animal and vegetable substances, as in petrified skulls, bones, and pieces of wood: and we see, that soft stone, which is plentifully found near *Naples*, and commonly called the lapis lyncurius, being rubbed a little and moistened with water, and then exposed to the sun in a due season of the year, will, in a very short time, (as eye-witnesses have assured me,) produce mushrooms fit to be eaten; as if even the seminal principles and rudiments of vegetables may be so preserved in a petrified earth, as to be able to disclose themselves, when they find an opportunity. To which agrees well what an eminent person, master of some of these stones, informs me, that they now and then find them of a vast bigness, as if whole masses of earth, pregnant with the prolific principles of mushrooms, were, by some supervening, but not very potently hardening petrescent liquor, turned into stone.

AND not only there may be boluses, sealed earths, and such like fossils, that are commonly known to be medicinal, hardened into stone,

ſtone by petrifying agents; but alſo other earths, ſubject to be petrified, may have medicinal and ſubtle particles of ſuch a kind in, them, as ſcarce any body would expect. But to omit inſtances, belonging to another paper, I have viſited a certain clay-pit in a waſte piece of ground, in which, at a conſiderable depth from the ſurface of the earth there lay a bed of clay, which by diſtillation yielded ſome acquaintances of mine a ſalt ſo volatile anp ſtrong, and ſo differing from other ſubterraneal ſalts, that my examens did not diſcover the manifeſt qualities of it without ſome wonder; and the owners of it (perſons curious and rich) did themſelves uſe it as well as give it in phyſick, and cryed it up for an excellent cordial, and a great opening and diaphoretic medicine.

THAT ſublimable ſalts, ſulphurs, bitumens, (bodies, that communicate enough of their virtues,) may be met with in the bowels of the earth, I have elſewhere ſhewn: and that ſuch ſubſtances may be found in bodies that paſs for ſtones, I have been induced to think by the chemical examen, that I purpoſely made of ſome ſuch concretions, particularly of that ſolid and heavy one, that is commonly called ſcotch-coal, from whence I obtained by diſtillation, (wherein I ſomewhat wondered, other mens curioſity did not, as far as I knew, prevent me,) a good proportion of oil or liquid bitumen, and no ſmall number of ſaline particles, that ſeemed to be of an uncommon nature.

THAT metalline particles may concur to make up a body, that paſſes for a medicinal ſtone, may appear by native ſulphur, which is it ſelf a compounded body, beſides a good proportion of mineral earth.

I had thoughts not to make an end of this diſcourſe, without mentioning to you ſome attempts, that I partly deſigned, and partly made, to illuſtrate ſome paſſages of it by purpoſely contrived experiments, whereof ſome were unproſperouſly, and others not altogether unſucceſsfully tried. But not having the minutes of them by me, and not daring to truſt my ſingle memory in experiments ſo nice, and ſo long ſince made, as thoſe were, I ſhall here put an end to your trouble; eſpecially ſince at length I perceive, that the forgetfulneſs of my firſt intended brevity has miſled me ſo far beyond the bounds of it into excurſions, whereinto the unforeſeen connexion of things unawares engaged me, that I ſtand in need both of your pardon and my own; of yours, for having exerciſed your patience with a prolix diſcourſe; and of my own, for having receded from my cuſtom, by contributing to that prolixity, and by expatiating upon conjectures; to which the more I conform to my own practice, the leſs I am indulgent: though theſe may be the more pardonable, becauſe I have propoſed them but as gueſſes, not peremptory aſſertions, much leſs phyſical demonſtrations. And if *Ariſtotle* himſelf, where he gives an account of phænomena appearing above the ſurface of the earth, ſcrupled not to think he had done enough, if he had ſhewn, how ſuch things may be produced; I hope it may be tolerable in me, who treat of things, that nature does privately in her dark and ſubterraneal receſſes, to have offered accounts, that are poſſible, if not probable. And yet I ſhould have ſpent much leſs of my diſcourſe upon conjectures, if I had not ſeen, that they gave me riſes to bring in more of natural hiſtory, than I could elſe decently do. But after all this I confeſs to you, (though you may think it a paradox) that one of the main cauſes of the prolixity of theſe papers was my haſte, and that experience hath taught me, on this occaſion (as well as on ſome others) that there may be more truth, than there is likelihood, in the genteel conceipt of a French ſecretary, that ſaid, he had written his friend a long letter, becauſe he had not leiſure to write him a ſhort one.

T R A C T S.

CONTAINING,

NEW EXPERIMENTS, touching the Relation betwixt FLAME and AIR. And about EXPLOSIONS.

An HYDROSTATICAL DISCOURSE, occasioned by some Objections of Dr. HENRY MORE against some Explications of NEW EXPERIMENTS made by the Author of these TRACTS:

TO WHICH IS ANNEXED

An HYDROSTATICAL LETTER, dilucidating an Experiment about a way of weighing Water in Water.

NEW EXPERIMENTS,

Of the POSITIVE or RELATIVE LEVITY of Bodies under Water,

Of the AIR'S SPRING on Bodies under Water,

About the DIFFERING PRESSURE of heavy SOLIDS and FLUIDS.

The PUBLISHER *to the* READER.

IT will, it is presumed, be altogether needless to preface any thing by way of commendation to the following Tracts; they will certainly commend themselves by their own worth to the intelligent and attentive reader, who might have seen them sooner, if the press had not detained them longer than was expected; since, to the publisher's knowledge, they were ready in the year 1671, except the hydrostatical discourse, and the explication of the author's experiment of weighing water in water, the former of which was finished in the beginning of this year 1672; though the latter could not be so till near the end of the same year, viz. the month of February, English stile, because the book of Mr. *George Sinclair*'s Hydrostaticks, in which it is excepted against, came not, I think, before that time to *London*; I am sure, not to the view of the honourable Author. Farewel.

NEW EXPERIMENTS

Touching the RELATION betwixt

FLAME AND AIR,

Sent in a LETTER

To the Learned Publisher of the PHILOSOPHICAL TRANSACTIONS.

SIR,

YOU may have observed, as well as I, that since the publishing of the experiments I sent you touching respiration, divers of our learned men have spent both thoughts and discourses in inquiring and disputing, whether there resides in the heart of animals such a fine and kindled, but mild substance, as they call a vital flame, to whose preservation, as to that of other flames, the air, (especially as it is taken in, and expelled again by respiration) is necessary. This among other considerations, makes me think it seasonable (though many avocations make it inconvenient) to complete the performance of the promise I made you, by adding to the experiments about respiration, which your commands have already obtained of me, those scattered notes, that I have been able to pick up about the relation betwixt flame and air. And though, I confess, they are very much inferior in number to the trials about respiration: and, that in making them it was not so much my design to complete an entire and distinct tract, though but a small one, of such experiments, as to gratify my own curiosity in the examining of a paradox or two, I had been writing about flame; yet the nobleness of the question now under debate, and their pertinency to it, will possibly keep them, as few as they are, from being useless. And that also they may be the better kept from being unwelcome, I have chosen to make my self a relator of matters of fact, without engaging with either of the litigant parties in a controversy, wherein I am the less tempted to be partial, because I have not formerly declared my opinion about it, and at present, I see, on either side, persons, for whom I have no small respect and kindness.

AND now, Sir, that you may not expect in the following papers such a number and variety of experiments, as I might perhaps be able to present you with, on some more tractable subject; I shall briefly mention to you some of the chief difficulties I met with in the making of these; which I do the rather, that, if you, and your ingenious friends have a mind to prosecute such trials, you may not be surprised with the difficulties I have met with; but provide at least against those foreseen ones, by which you will scarce fail to be encountered.

I shall then inform you, that the ensuing experiments were rendered uneasy and troublesome to me by this; that some of them could not be conveniently done at all seasons of the year, nor in any season in all weathers, but must be made not only in the day time, but in sunshine days. You will easily guess, that I speak of those experiments, that are to be made by the help of a burning-glass, casting the reflected or refracted beams of the sun upon the combustible matter placed in the exhausted receiver: for, by reason of the interposition of so thick a glass, whereby many of the incident beams of light are reflected, and others inconveniently refracted, there is ordinarily requisite a clear day, and a competent height of the sun above the horizon, and sometimes also a convenient time of the year, to bring such experiments, as we were speaking of, to a fair trial. Not to take notice, that in such attempts there usually intervene circumstantial difficulties, not so easy to be foreseen: and it not being summer, when I had occasion to make the following experiments, I could make but very few with the sun-beams; besides that there are divers others, which are not that way to be made so conveniently, if at all, as by the help of the fire.

BUT though the trials of this second sort had their conveniences, in regard they might be made in any weather, and as well by night as day; yet they were not unattended with peculiar inconveniencies; some of which you will easily discern by the mention of them, that was necessary to be made in some of the relations themselves. And, besides more particular and emergent difficulties, there was this in general, that rendered these experiments troublesome; that, whether I made them in larger receivers, or in small, or in middle-sized ones, each of these cases had its inconveniencies: for very large receivers, besides that it was very toilsome and tedious to empty them of air, required so much time for the exhaustion, that too frequently, by that time the operator

rator had done pumping, the included, or other heated body was grown too cold to perform the desired effect: and if the receiver were not considerably large, than the red-hot iron, or other included body, that was to burn the combustible matter, would much endanger the breaking of the over-heated glass, and not afford room enough for some phænomena to be fairly exhibited in; and, besides, create another difficulty, to which we found middle-sized receivers also obnoxious: for, several times, when the experiment required an intense heat within the receiver, then (especially if some casual obstacle hindered the quick exhaustion) the heat of the ignited iron, or some such other included body, would so melt or soften the cement, that fastened the receiver to the engine, that, when the glass was brought to be well exhausted, and sometimes also before, the external air would, by its pressure and fluidity, squeeze or thrust in somewhere or other the yielding cement, and thereby cause in the instrument a leak, that would much incommodate us, if not reduce us to begin the experiment again, insomuch, that, for some trials, we were fain to provide a cement on purpose; the least fusible, that we used on other occasions, being yet found too fusible on these.

NOR were those, I have already mentioned, the only difficulties and impediments I met with in making experiments about flame and air; but I shall not here trouble you with them in this place, where it may suffice for me to have mentioned those, that are of a more general nature, and are like the most frequently to occur.

BUT though I declined to name any other to you, than the foregoing difficulties in making the following experiments; yet I must not omit to take notice of one, that may occur to you about judging of them. For, in those trials, that require to have an ignited iron or any such thing included in the receiver, it would usually happen, that so much heat would rarefy the air shut up in the mercurial gage, and consequently inable it to depress the mercury, that lies under it, far beneath the mark it would have staid at, upon the meer account of so much ambient air pumped out: this would happen, I say, before the heated receiver was well exhausted; so that, if one be not aware of this, it will be obvious, by looking on the gage, to conclude the receiver to be well emptied, before it really is so. And therefore the safest way in these cases is, to continue to pump (without trusting to the ordinary marks) till you see, that the mercury will be no further depressed in the sealed leg of the gage; though otherwise, by concurring signs, one that is versed in those trials, may well enough judge, when he needs to pump no longer.

BUT perhaps you will here demand, whether, by our engine, we can competently withdraw the air out of a receiver; or whether, at least, that may not be much better done by the help of quick-silver, after the manner of the Torricellian experiment, in regard that ponderous liquor frees the glass, it deserts, from all the air at once, and exactly hinders the regress of it.

IN answer whereunto, I hope you do not expect, that I should contend for a favourabler judgment of the engine I employ, than the virtuosi (as well foreign as English) have been pleased to pass on it already: and therefore, to tell you freely my thoughts about the main part of the proposed question, I shall readily avow to you, that I think, there may be experiments (such as some of those, where the included body need be but small, and where the being suddenly produced is chiefly desired in the effect) wherein, by the help of the quick-silver, the exhaustion of the air may be dispatched with greater celerity, and consequently make the effect be more conspicuous, than, by our ordinary way of trying, it would be in our engine; since the fall of the mercury does, as the objection intimates, produce a vacuum (in our sense of that word) very nimbly, whereby the expansion of the air is presently effected, and the aereal particles, harboured in the pores of any body placed in this deserted cavity, will thereby have opportunity more suddenly to expand themselves. But, on the other side, I might answer in general, that when I have particular occasions to dispatch the exhaustion of the air, I can very much hasten it, by barely lessening, as I have several times done, the capacity of the receiver; insomuch, that I have sometimes employed so small an one, that in half a minute, or much less, after it was fitted on, we could considerably exhaust it, and thereby produce phænomena exceeding conspicuous. And as to the experiments of this little tract in particular, it may be said, that not to mention the troublesomeness, and other inconveniencies of needing to employ such an unwieldy weight of mercury, you will easily find, by the phænomena of divers of the ensuing trials, that most of them cannot be with any conveniency, and some of them not at all, made in the Torricellian tubes. As for the ground of the objection, that the air cannot be so well drawn out by our way, as by the subsiding of the mercury; though you may think that very clear, yet one, that were very jealous of the reputation of the instrument I employ, may perhaps reasonably enough question it. For the vacuum, that is produced in the Torricellian experiment, as it is made all at once, so it is made once for all; and therefore, if there were any aereal particles lurking in the mercury, as there will be pretty store, if the quantity of that liquor be great enough to make a considerable vacuum, which if it be not, it will be too small for very many of our trials; they will remain in the deserted cavity at the top of the glass, and, by their expansion there, much hinder the full operation of an ambient vacuum upon the bodies placed in it. Besides that almost all such bodies, if they be dry, will be so incongruous to mercury (which scarce sticks to any consistent bodies but metals,) that probably there will be no small number of aereal corpuscles intercepted between the mercury and

and those surfaces, to which it does not closely adhere: which aery corpuscles, when the subsiding mercury deserts them, will be left to encrease the number of those, that, as we were saying, will emerge from the mercury; from which, as also from the pores of the included bodies, will perhaps arise divers new ones, from time to time, for a pretty while after. And in case the vacuum be made by a cylinder of two or three and thirty foot of water, as for some experiments, that have been tried in *France* and *Italy*, hath been done, the emersion of bubbles may last a long time, as may be gathered from some observations of mine, elsewhere related.

ON the contrary, in our engine, though when the receivers are not very small, they are more slowly emptied; yet in recompence, we may continue the pumping out of the air as long, and renew it in the same experiment as often as we think fit: so that, if we perceive, that, after the first exhaustion of the glass, there happen any aereal particles to extricate themselves successively out of the included body, we can, by resuming the pump from time to time, whenever need requires, free the vacuum from these also; which, in some cases, I have found to be longer and more copiously emitted by the included bodies, than any thing but jealous trials could have convinced me of. And to confirm what I have been saying by something historical, I shall add; that though the excellent Florentine academians are thought to have prosecuted the experiments about the vacuum made with mercury the furthest of any, yet some eminent members of that illustrious society were pleased to confess to me, that they never were able, by the help of mercury, to bring a glass-bubble, sealed up with air in it, to burst of itself by the withdrawing of the external air; which yet I have often done with the engine I employ, and convinced them, that I could do so, by doing it in their presence.

YOU will, perhaps, think it somewhat strange, to find, that I set down some of the following narratives in such a way, as does not express me sollicitous to ascribe and vindicate to the air so absolute and equal a necessity to the production and conservation of all flames, as divers learned men have concluded from my former experiments. But I, that am content to be kind to the air, but not partial, shall not scruple to declare to you, that, as much as some may think me beholden to the air for any discoveries of itself, it may have vouchsafed me; yet, I think, a natural, as well as a civil historian, does, in his accounts of matters of fact, owe more to truth, than to gratitude itself. And though, whereever the air can challenge a clear, or, at least, a probable interest in a phænomenon, I am not only disposed, but glad to do it right; yet I would not easily assert to it a larger jurisdiction than I find nature to have assigned it; especially since, without partiality, that, I presume, may be shewn to be very large and considerable, and perhaps to reach to many things, wherewith men seem not to have yet taken notice, that it hath any thing to do at all.

WHAT hath been hitherto said, will not, I hope, seem impertinent or useless, whenever you shall fall upon the actual making of such experiments as you are about to read. But I fear, that to add any thing more, (which were not difficult for me to do to the preliminary part of this small tract) would make it too disproportionate to the historical; from which I shall therefore no longer detain you.

The FIRST TITLE.

Of the difficulty of producing FLAME without AIR.

EXPERIMENT I.

A way of kindling brimstone in vacuo Boyliano, *unsuccessfully tried.*

WE took a small earthen melting pot, of an almost cylindrical figure, and well glazed (when it was first baked) by the heat; and into this we put a small cylinder of iron, of about an inch in thickness, and half as much more in diameter, made red hot in the fire; and having hastily pumped out the air, to prevent the breaking of the glass; when this vessel seemed to be well emptied, we let down, by a turning key, a piece of paper, wherein was put a convenient quantity of flower of brimstone, under which the iron had been carefully placed; so that being let down, it might fall upon the heated metal; which as soon as it came to do, that vehement heat did, as we expected, presently destroy the contiguous paper; whence the included sulphur fell immediately upon the iron, whose upper part was a little concave, that it might contain the flowers when melted. But all the heat of the iron, though it made the paper and sulphur smoke, would not actually kindle either of them, that we could perceive.

EXPERIMENT II.

An ineffectual attempt to kindle sulphur in our vacuum another way.

ANOTHER way I thought of to examine the inflammability of sulphur without air; which, though it may prove somewhat hazardous to put it in practice, I resolved to try, and did so after the following manner:

INTO

INTO a glafs-bubble of a convenient fize, and furnifhed with a neck fit for our purpofe, we put a little flower of brimftone (as likely to be more pure and inflammable than common fulphur;) and having exhaufted the glafs, and fecured it againft the return of the air, we laid it upon burning coals, where it did not take fire, but rife all to the oppofite part of the glafs, in the form of a fine powder; and that part being turned downward and laid on coals, the brimftone, without kindling, rofe again in the form of an expanded fubftance, which (being removed from the fire) was, for the moft part, tranfparent, not unlike a yellow varnifh.

ADVERTISEMENT.

THOUGH thefe unfuccefsful attempts to kindle fulphur in our exhaufted receivers were made more difcouraging by fome more, that were made another way; yet judging that laft way to be rational enough, we perfifted fomewhat obftinately in our endeavours, and conjecturing, that there might be fome unperceived difference between minerals, that do all of them pafs, and are fold for common fulphur, I made trial, according to the way hereafter to be mentioned, with another parcel of brimftone, which differed not fo much from the former, as to make it worth while to fet down a defcription of it, that probably would not be ufeful.

BUT in this place, it may fuffice to have given a general intimation of the poffibility of the thing. The proof of it you will meet with under the third Title, when I come to tell you what ufe I endeavoured to make of our fulphurous flames.

EXPERIMENT III.

Shewing the efficacy of air in the production of flame, without any actually flaming or burning body.

HAVING hitherto examined by the prefence of the air, what intereft it has in kindling of flame; it will not be impertinent to add an experiment or two, that we tried to fhew the fame intereft of the air, by the effects of its admiffion into our vacuum. For I thought it might reafonably be fuppofed, that if fuch difpofitions were introduced into a body, as that there fhould not appear any thing wanting to turn it into flame but the prefence of the air, an actual accenfion of that body might be produced by the admitted air, without the intervention of any actual flame, or fire, or even heated fubftance; the warrantablenefs of which fuppofition may be judged by the two following experiments.

WHEN we had made the experiment, ere long to be related in its due place, (viz. Title II. Experiment the 2d) to examine the prefumption we had, that even when the iron was not hot enough to keep the melted brimftone in fuch a heat, as was requifite to make it burn without air, or with very little, it would yet be hot enough to kindle the fulphur, if the air had accefs to it: to examine this, I fay, we made two or three feveral trials, and found by them, that if fome little while after the flame was extinguifhed, the receiver were removed, the fulphur would prefently take fire again, and flame as vigoroufly as before. But I thought it might without abfurdity be doubted, whether or no the agency of the air in the production of the flame might not be fomewhat lefs, than thefe trials would perfuade; becaufe, that by taking off the receiver, the fulphur was not only expofed to frefh air, but alfo advantaged with a free fcope for the avolation of thofe fumes, which in a clofe veffel might be prefumed to have been unfriendly to the flame.

HOW far this doubt may, and how far it fhould, be admitted, we may be affifted to difcern by the fubjoined experiment, though made in great part for another purpofe; which you will perceive by the beginning of the memorial I made of it, that runs thus;

EXPERIMENT IV.

A differing experiment to the fame purpofe with the former.

HAVING a mind to try, at how great a degree of rarefaction of the air, it was poffible to make fulphur flame by the affiftance of an adventitious heat, we caufed fuch an experiment as the above-mentioned to be reiterated, and the pumping to be continued for fome time after the flame of the melted flowers of brimftone appeared to be quite extinguifhed, and the receiver was judged by thofe, that managed the pump (and that upon probable figns) to be very well exhaufted. Then, without ftirring the receiver, we let in at the ftop-cock very warily a little air, upon which, we could perceive, though not a conftant flame, yet divers little flafhes, as it were, which difclofed themfelves by their blue colour to be fulphurous flames; and yet the air, that had fufficed to re-kindle the fulphur, was fo little, that two exfuctions more drew it out again, and quite deprived us of the mentioned flafhes. And when a little air was cautioufly let in again at the ftop-cock, the like flafhes began again to appear, which, upon two exfuctions more, did again quite vanifh, though, upon the letting in a little frefh air the third time, they did once more re-appear.

WHETHER, and how far fuch experiments as thefe may conduce to explicate what is related of fires, fuddenly appearing in long undifclofed vaults or caves to thofe, that firft broke into them, I may perchance elfewhere confider, but fhall not here enquire, efpecially being not yet fully fatisfied of the truth of the matter of fact.

EXPERIMENT V.

About an endeavour to fire gun-powder in vacuo *with the fun-beams.*

WHATEVER hath been hitherto delivered, will not, I prefume, make it unreafonable to enquire, whether, what intereft foever the air appears to have in the production,

tion of those flames, that are to last for some time, there may not easily be produced a momentary flame or flash, without any assistance from the air. Wherefore I employed some endeavours to discover, whether there were the same need of air to the going off of gun-powder, as to the inflammation of other bodies. And though my first attempt of this nature being unprosperous, it was concluded by the learned of the by-standers, that I should never be able to make a successful one to kindle gun-powder in an exhausted receiver; yet this did not hinder me from prosecuting a design, for whose feasibility I considered, that it might be alledged *a priori* (as they use to speak) that brimstone, which is one of the ingredients of gun-powder, appears by several trials, to be sometimes capable of accension in our vacuum, and therefore probably may kindle the rest. But how far the firing of powder, without the help of air, is possible, will be best judged by the experiments you will meet with under the third title: and how far it is more difficult to be kindled in our exhausted receivers, than in the open air (which is an inquiry proper for this place) may be guessed by the subjoined trial; which, though it were made many years since (in the year 1660) before we had devised the mercurial gage, to examine how well the receiver was exhausted, I shall yet afford it a room in this place, because it was made in summer by the help of a burning-glass, which I could not employ to purpose in the winter-season, wherein the two following trials were made.

To give you then some account of that part of the experiment, which concerns our present inquiry, I will subjoin a transcript of what I find registered about it; which is to this purpose, and almost in these words: that, having conveniently placed three or four grains of gun-powder in the cavity of our receiver, and having carefully drawn out the air, we cast the sun-beams, united by a good burning-glass, upon the powder, and kept them there a pretty while to little purpose; till, at length, the powder, instead of taking fire, smoaking only, and melting like a metal, those spectators, that were of another opinion, than I was yet convinced of, would have me leave off. The further event of such trials more fully prosecuted you will find under the third title; all that will be pertinent to be here added being, that the newly recited experiment was not the single one, we made about that time, that discovered a great indisposition even in gunpowder to be fired in our vacuum.

EXPERIMENT VI.

An attempt to fire gun-powder in vacuo, *by means of a hot iron.*

WE took, by weight, what we judged a convenient quantity of gun-powder, that was extraordinarily strong and well made; and having in our receiver, that was capable of holding about sixteen pounds of water, placed the formerly mentioned iron first heated red-hot, when the air appeared by the mercurial gage to have been diligently pumped out, we let down, by help of the turning key, a small piece of thin paper, wherein the powder had been put, till we saw it reached the plate, by whose heat we hoped the paper would be destroyed, and the powder made to go off. But though both the one and the other had been purposely well dried near the fire, before they were put into the receiver; the desired explosion of the powder did not ensue. Yet there appeared upon the iron plate a pretty broad blue flame, like that of brimstone (whence it was judged the sulphureous ingredient of the gun-powder, that was kindled) which lasted so very long, as we could not but wonder at it. But, at length, the powder not going off and the still decaying heat of the iron forbidding us to wait any longer, we thought fit to take off the receiver, and found (as we expected) that the paper contiguous to the iron was, in part, destroyed by its heat; but most of the grains of the powder seemed not altered, and were found disposed enough to be fired, notwithstanding the consumption of the brimstone, that had burned away.

APPENDIX.

TO confirm the foregoing experiment, by shewing how great a disposition to take fire there may be in gun-powder, that yet would not do so without air, I shall subjoin this observation,

HAVING reiterated the newly mentioned experiment after the like manner, and with the same receiver, and iron-plate, as formerly, we did not find any explosion to be made for so long a time, that thinking it in vain to wait any farther, we let in the air, which might perhaps, by the help of the remaining heat of the iron, procure the operation we at first desired. The event was, that after nothing had ensued for a good while, and we scarce thought, that such a thing would happen; the powder suddenly went off with a great flash, and so shook the receiver, that was yet standing on the engine, as to endanger the throwing of it down. Which circumstance I mention, to give you a caution, that may prove useful, in case you try in close vessels experiments with gun-powder; since if they be not warily managed, they may sometimes (as I have had occasion to observe) prove dangerous enough; which will be the better discerned, if I add, that the powder, that had this operation on a receiver (large enough to contain two gallons of liquor) was weighed before it was put in, and amounted but to one grain, (though a greater quantity might perhaps have been well enough ventured upon, if it had been but common gun-powder.)

EXPERIMENT VII.

Reciting another way, whereby the firing of gun-powder in vacuo Boyliano *was attempted.*

TO diversify our ways of examining the indisposedness of gun-powder to be fired in our vacuum, we thought fit to add to the foregoing trials that, which followeth.

INTO

INTO a pretty large and ſtrong glaſs-bubble we put a few ſmall corns of gunpowder, and having carefully exhauſted it, and ſecured it againſt the return of the air, we put it upon a pretty quantity of live coals ſuperficially covered with aſhes; by whoſe heat the ſulphureous ingredient of the powder was in part kindled, and burned blue for a pretty while, and with a flame conſiderably great (in proportion to the powder;) upon whoſe ceaſing, the powder, which, when all was done, did not take fire, appeared to have ſent up, beſides the flame, a pretty deal of ſulphureous ſublimate, that ſtuck to the upper part of the glaſs, and being held againſt a candle we cauſed to be brought in, (for the experiment had been purpoſely made in a dark place) it exhibited divers vivid colours like thoſe of the rain-bow.

EXPERIMENT VIII.

About a trial made to fire gunpowder in our vacuum by the help of ſparks.

THOUGH in the fourteenth of the long ſince-publiſhed phyſico-mechanical experiments there is recited a trial made about kindling of gunpowder with a piſtol; yet I ſhall not forbear to ſubjoin the enſuing account, partly, becauſe the receiver, we then employed, being about four times, if I miſ-remember not, as big as that we laſt made uſe of, it was very difficult to exhauſt the one ſo well as the other; and partly, becauſe we wanted ſome accommodations, with which we ſince furniſhed ourſelves, and (having not then deviſed the mercurial gage we employed in the making this laſt experiment) we could not then judge ſo well, as we ſince could, of the degrees, to which the receiver was emptied. And, therefore, when in the relation of that fourteenth trial, there is mention made of one attempt, that did ſucceed, among divers, that did not; there is towards the cloſe an intimation given, that in ſpite of the great rarefaction, that had been made in the air, there might yet be ſome little portion of it remaining in the receiver. I proceed then to the promiſed relation, which I find thus ſet down:

TO proſecute the deſign of the foregoing experiment by a way ſomewhat differing from thoſe hitherto mentioned, we made, though not without difficulty, the enſuing trial; one of whoſe ſcopes you will find intimated at the cloſe of the relation.

WE took a ſmall and very ſhort piſtol, and having well faſtened it with ſtrings to a great weight, that was placed upon the iron-plate of our engine, we drew up the cock, and primed the pan with dry powder; then over both the weight and piſtol we whelmed a receiver, capable of containing two gallons of liquor, and having carefully cemented it on, we cauſed the air to be diligently pumped out; having before put in a mercurial gage, to help us to diſcern when it was exhauſted. Laſtly, ordering the pump to be plied in the mean while, for fear ſome air ſhould ſteal in, before the trial was compleated, we did, by the motion of the turning key, ſhorten a ſtring, that was tied both to it and the trigger of the piſtol, by which means we did as much as we could towards the firing of the powder in the pan; but though the pan were made to fly open, yet the powder did not go off: whereupon letting in the air, and cocking the piſtol again, without taking it off the weight it was tied to before, we drew out a little air, to be ſure, that the receiver was cloſely cemented on, (which care we took in reference to another experiment;) and then letting in the air at the top of the receiver, and ſtopping it in with the turning key, we did, by the help of that key, draw aſide the trigger again; whereupon, though there had been no new powder put into the pan, nor any left in it, but only ſome little, that remained after the late trial, yet that little readily took fire and flaſhed in the pan; which made it the more probable, that, in the former trial, ſparks of fire had been ſtruck out by the colliſion of the flint and ſteel: which was the more credible, becauſe in another trial, made the ſame hour in the ſame exhauſted receiver, two of the aſſiſtants plainly ſaw a ſpark or two fly out upon the falling of the cock, though I, that chanced to ſtand in an inconvenient place, did not then perceive it. But afterwards, having cauſed the experiment for my fuller ſatisfaction to be repeated, I freed my-ſelf from need of truſting others eyes: ſo that it appears, that notwithſtanding the great indiſpoſition of gunpowder itſelf to be reduced into flame in our vacuum, yet even ſolid matter is not uncapable of being ignited there, if it be put into a motion ſufficiently vehement.

IF this experiment had not been ſo very troubleſome to make, I ſhould have been invited to reiterate it, becauſe a not contemptible ſcruple may be prevented, if the trial can be made to ſucceed, in regard, that the going off of the whole gunpowder, by the falling of a ſpark or two only upon two or three of its grains, would argue, that the accenſion of the reſt was made by the propagation of flame from the kindled grains to the reſt; ſo ſmall a portion of ignited and ſuddenly vaniſhing matter, as is to be found in a ſpark or two, being not likely to be able in ſo very ſhort a time to impart a vehement, or ſo much as a ſenſible heat, to the whole aggregate of grains, or at leaſt a great part of them, as the focus of a burning-glaſs, held long enough upon them to make them melt, may well be ſuppoſed to do.

EXPERIMENT IX.

Two ways of making aurum fulminans go off in our exhauſted receiver.

BECAUSE it is wont to be ſuppoſed, how juſtly I here diſpute not, that aurum fulminans, as the chemiſts call it, is much of the nature of gunpowder, though by vaſt odds ſtronger than it; I thought it not unfit to make trial, whether it could be made to go off in our exhauſted receiver; and accordingly, about the time, that the other experiment of firing gunpowder by the ſun-beams was made, we alſo made trial of this; and that, as I remember, in the ſame receiver, and with the ſame burning-glaſs. The event was, that, though the air had been pumped out, the concentrated beams of the ſun made the aurum fulminans go off, and violently ſcatter about the cavity of the receiver a yellowiſh duſt or powder, which other trials in the free air made us look upon as particles of the gold, that was the main ingredient of this odd compoſition.

THIS experiment we reiterated a good while after in another place, and with other veſſels, and yet with the like ſucceſs.

BUT in regard theſe trials being made by the united ſun-beams, it was unavoidable, that our eyes would be before-hand affected with the vivid impreſſions of ſo glaring a light; it ſeemed not ſafe to determine, by the bare going off, or ſhattering of the aurum fulminans, whether or no it afforded any flame or light upon its exploſion: for, as we could not be ſure of the affirmative, becauſe our eyes could not diſcern any momentany flame or flaſh; ſo it ſeemed not ſafe to conclude the negative; ſince, though there had been ſuch a flame, yet, if it had not been ſtrong, it would not have been ſenſible to our eyes, whilſt pre-affected by a powerful light. Wherefore we reſolved to make this trial in the night with an iron heated, but not candent, (that its light might not eclipſe that, which the powder might afford;) and having, after the manner already often recited, exhauſted a pretty large receiver, and let down by a ſtring half a quarter of a grain (by weight) of good aurum fulminans, of our own preparing, looſely tied in a little piece of thin paper, (which paper, former trials to another purpoſe kept us from fearing, that no hotter an iron, than ours then was, would kindle) we found, as we expected, that after the powder had lain long enough upon the iron to be thoroughly heated, it went off all together, and, as the by-ſtanders affirmed, with a flaſh: but my face being accidentally turned to remove a light, that I feared might diſturb us, I could not ſee the flaſh myſelf, and therefore cauſed the experiment to be made once more, to ground my narrative upon my own obſervation; which quickly aſſured me, that the luminous flaſh, produced upon the exploſion, was not only ſenſible, but conſiderable.

The SECOND TITLE.

Of the difficulty of preſerving FLAME without AIR.

SINCE it is generally, and in moſt caſes juſtly, eſteemed to be more eaſy to preſerve flame in a body, that is already actually kindled, than to produce it there at firſt; we thought fit to try, whether, at leaſt, bodies already burning might not be kept in that ſtate without the concurrence of air. And though in ſome of our formerly publiſhed phyſico-mechanical experiments it happened, that actual flame would ſcarce laſt a minute or two in our large pneumatical receiver; yet, becauſe it ſeemed not improbable, that mineral bodies once kindled might afford a vigorous, and very durable flame, we thought fit to deviſe, and make the following trials: whence probably we might receive ſome new informations about the diverſities, and ſome other phænomena of flame, and the various degrees, wherein the air is neceſſary, or helpful to them.

EXPERIMENT I.

Reciting an attempt to preſerve the flame of brimſtone without air.

WE put upon a thick metalline place a convenient quantity of flowers of ſulphur; and having kindled them in the air, we nimbly conveyed them into a receiver, and made haſte to pump out ſome of the included air, partly for other reaſons, and partly that the cavity of the receiver might be the ſooner freed from ſmoak, which would, if plentiful, both injure the flame, and hinder our ſight. As ſoon as the pump began to be plied, or preſently after, the flame appeared to be ſenſibly decayed, and continued to be leſſened at every exſuction of the air; and in effect it expired, before the air was quite drawn out. Nor did it, upon the early removal of the receiver, do any more than afford, for a very little while, ſomewhat more of ſmoak in the open air, than it appeared to do before.

THE reiteration of this experiment preſently after afforded us nothing new, worth mentioning in this place.

EXPERIMENT II.

Relating a trial about the duration of the flame of ſulphur in vacuo Boyliano.

TO vary a little the foregoing experiment, and try to ſave ſome moments of time, which on theſe occaſions is to be huſbanded with the utmoſt care; having provided a cylinder of iron, larger than the former, that it might by its bulk, being once heated, both contribute to the accenſion of the ſulphur, and to the laſting of its flame, we made a trial, that I find regiſtred to this effect:

WE took a pretty big lump of brimſtone, and tied it to the turning-key; and having got what elſe was neceſſary in a readineſs, we cauſed the iron-plate to be haſtily brought red-hot from the fire, and put upon a pedeſtal, that the flame might be the more conſpicuous; and having nimbly cemented on the receiver, we we ſpeedily let down the ſuſpended brimſtone, till it reſted upon the red-hot iron, by which being kindled, it ſent up a great flame with copious fumes, which hindered us not from plying the pump, till we had, as we conjectured, emptied the receiver; which we could not do without withdrawing together with the air much ſulphureous ſmoke, that was offenſive enough both to the eyes and noſtrils. But notwithſtanding this pumping out of the air, though the flame did ſeem gradually to be ſomewhat impaired, yet it manifeſtly continued burning much longer, than by the ſhort duration of other flames in our receivers, when diligence is uſed to withdraw the air from them, one could have expected. And eſpecially one time, (for the experiment was made more than once,) the flame laſted, till the receiver was judged to be well exhauſted; and ſome thought it did ſo ſurvive the exhauſtion, that it went not out ſo much for want of air, as fuel; the brimſtone appearing, when we took off the receiver, either to have been conſumed by the fire, that fed on it, or to have caſually run off from the iron, whoſe heat had kept it conſtantly melted.

☞ IN caſe you ſhould have a mind to proſecute experiments of the nature of this and the precedent, it may not prove uſeleſs, if I intimate to you the following advertiſements. 1. For the red-hot iron above mentioned, we thought it not amiſs to provide, inſtead of the melting-pot employed in the firſt experiment, a pedeſtal, if I may ſo call it, made of a lump of dryed tobacco-pipe-clay, that the vehement heat of the iron might neither fill the receiver with the ſmoak of what it leaned on, nor injure the engine, if it ſhould reſt immediately upon that; and this pedeſtal ſhould be ſo placed, that the iron may be as far as you can from the ſides of the receiver, which elſe the exceſſive heat would endanger.

2. To the above-mentioned concave iron, that was to receive the brimſtone, we did for ſome occaſions cauſe to be fitted a thick convex piece of iron, ſhaped almoſt like a flattiſh button; which was not to be uſed conſtantly, but upon occaſion, that, being laid red-hot over the melted brimſtone, it might increaſe the heat, and keep the flame from having ſo broad a ſuperficies, whereby it would conſume its fuel too faſt.

3. WE ſometimes thought it expedient, for the clearer diſcerning of what ſhould happen in the receiver, to make the experiment by night, and remove the candles, when we were juſt about to pump, preſuming, that the flame would be conſpicuous enough by its own light; as indeed we found it to be, though its light were but dim, conſidering the greatneſs of the flame; whoſe colour, though it did not quite loſe its wonted blueiſhneſs, ſeemed yet to have received a great and ſomewhat odd alteration.

THERE is one great inconvenience, ſcarce avoidable in this experiment, *viz.* that the fumes aſcending very copiouſly do quickly much darken the receiver, and if the trial be long continued, line it with a kind of flower of brimſtone, which obſcures it much more, and therefore ought to be carefully wiped away, whenſoever the receiver is taken off; upon which account you will not, I preſume, wonder, if you ſhall find the phænomena of theſe experiments not always to be the very ſame with what you meet with in this paper; ſince, as it is very poſſible, that we may not have been able to obſerve things ſo accurately by reaſon of the newly mentioned fumes and flowers; ſo it is not impoſſible, that the difference, if there ſhall be any, of other men's obſervations from ours ſhould proceed from the ſame cauſe.

BEFORE we paſs from this ſecond experiment, it will not be amiſs to take notice, that though the flames of brimſtone may be allowed to be ſomewhat more durable than the flames of vegetables are wont to be; yet it is not ſafe to conclude, that it was meerly upon the account of their native vigour, that the flames above mentioned laſted ſo long in our receiver.

FOR we ſeemed to obſerve, that there was requiſite a very intenſe heat of the iron to make the ſulphur capable of flaming on it, when any conſiderable proportion of air was withdrawn. For which reaſon it ſeems expedient, according to what I lately intimated, that the iron, that is to keep it melted, be of a good thickneſs, that it may the longer retain a competent heat; and we thought it contributed to the ſucceſsfulleſt trials we made, that in them we uſed, beſides the concave iron, the convex one mentioned in the ſecond note.

EXPERIMENT III.

Of the laſting of the flame of a metalline ſubſtance in the ſame vacuum.

THOSE ſulphurs, that chemiſts call metalline, being ſuppoſed by many to be of a much more fixed nature than common ſulphur, and it being indeed probable enough, that in them good ſtore of very minute particles are crowded together, I thought fit to try, whether a body, wherein a vulgar chemiſt would think the ſulphur of a metal to be the main ingredient, would afford in our vacuum a more vigorous or laſting flame, than that of common ſulphur. And, though I will not here trouble you with my particular ſcruples about the chemiſts doctrine concerning metalline ſulphurs, nor with the grounds, on which I deviſed the following inflammable ſolution of Mars, (for I do not now give it a more determinate name) which ſome chemiſts will not perhaps diſlike; I ſhall here annex the enſuing tranſcript of the trial itſelf.

HAVING provided a ſaline ſpirit, which by an uncommon way of preparation was made exceeding ſharp and piercing, we put into a vial, capable of containing three or four ounces of water, a convenient quantity of filings of ſteel, which were not ſuch, as are commonly ſold in ſhops to chemiſts and apothecaries, (thoſe being uſually not free enough from ruſt) but ſuch as I had a while before cauſed to be purpoſely filed off from a piece of good ſteel. This metalline powder being moiſtened in the vial with a little of the menſtruum, was afterwards drenched with more; whereupon the mixture grew very hot, and belched up copious and ſtinking fumes; which, whether they conſiſted altogether of the volatile ſulphur of the Mars, or of metalline ſteams participating of a ſulphureous nature, and joined with the ſaline exhalations of the menſtruum, is not neceſſary to be here diſcuſſed. But whenceſoever this ſtinking ſmoke proceeded, ſo inflammable it was, that upon the approach of a lighted candle to it, it would readily enough take fire, and burn with a blueiſh, and ſomewhat greeniſh flame, at the mouth of the vial, for a good while together; and that, though with little light, yet with more ſtrength, than one would eaſily ſuſpect.

THIS flaming vial therefore we conveyed into a receiver, which he, who uſed to manage the pump, affirmed, that about ſix exſuctions would exhauſt. And the receiver being well cemented on, upon the firſt ſuck the flame ſuddenly appeared four or five times as great as before; which I aſcribed to this, that upon the withdrawing of the air, and conſequently the weakning of its preſſure, great ſtore of bubbles were produced in the menſtruum, which breaking could not but ſupply the neck of the vial with ſtore of inflammable ſteams, which, as we thought, took not fire without ſome noiſe: upon the ſecond exſuction of the air, the flame blazed out as before, and ſo it likewiſe did upon the third exſuction, but after that it went out; nor could we re-kindle any fire by haſtily removing the receiver; only we found, that there remained ſuch a diſpoſition in the ſmoke to inflammability, that holding a lighted candle to it, a flame was quickly rekindled.

EXPERIMENT IV.

Of the duration of the flame of ſpirit of wine impregnated with a metal in the exhauſted receiver.

BECAUSE it may, upon grounds not improbable, be thought, that well-dephlegmed ſpirit of wine being a pure æthereal liquor, which does not, like combuſtible ſulphurs (whether vulgar or metalline) emit any viſible ſmoak to ſtifle the flame (into which it may, in the free air, be totally reſolved;) if this ſpirituous, and thus qualified liquor, could be duely aſſociated with a metalline body, the reſulting flame might be more than ordinarily vigorous and durable; I reſolved to make an experiment of this ſort, and having by a way, that I delivered in another paper [in a Paradox about the fuel of Flames] ſo united highly rectified ſpirit of wine with a prepared metal, that they would both afford a conſpicuouſly tincted flame; we put this mixture into a ſmall glaſs-lamp, made on purpoſe, and furniſhed with a very ſlender wick, which the mixture would not burn, whilſt there was liquor enough to imbibe it well; and putting this lighted lamp into a convenient place of a receiver, that was not ſmall, ſince it was able to contain about two gallons, or ſixteen pounds of water, we made haſte to cement on the glaſs to the engine, and yet found not in two or three ſeveral trials, that after the pump began to be moved, ſo little a quantity of tincted flame in that capacious glaſs laſted much, if at all, more than half a minute of an hour, eſtimated by a minute watch.

AND becauſe the receiver, we then made uſe of, ſeemed to me, by reaſon of its ſize, and ſome accommodations, that belong to it, proper enough to be employed about other trials, concerning the relation between flame and air; I thought fit to try, with the ſame ſmall lamp and liquor, what other phænomena of that kind would be afforded by letting air in and out, according to the various exigencies of my particular aims.

BUT not having then, nor in ſome time after, the leiſure and opportunity of ſetting down things circumſtantially, I contented myſelf to take thoſe ſhort notes of the principal things, whereof I now ſubjoin the tranſcript.

WHEN the flame began to decay, the turning key being now and then drawn almoſt out, the tincted flame laſted once a minute and a half, and another time longer.

THE turning key being taken out in the beginning, the flame laſted two minutes or better.

A pipe bedded in the cement at the bottom of the glaſs, and having at each end an open orifice almoſt of the bigneſs of that filled by the turning key, which key was then removed from the top; the tincted ſpirit ſeemed to burn very conveniently, as if the flame would have burned very long, if we would have permitted it ſo to do.

THE orifice at the top being ſtopped with the turning key, though the pipe were left open at the bottom, it plainly, in a ſhort time, ſeemed much to decay, and ready to expire; whereupon I cauſed one to blow conſtantly, yet but very gently, in at the pipe with a pair of bellows, and by this means, though we did not keep the flame vigorous, yet we kept it alive for above four minutes; and then obſerving it to be manifeſtly ſtronger, than it was, when we began to refreſh it with the bellows, we ceaſed from blowing, and found, that though the glaſs-pipe was ſtill left open, yet within about one minute the flame was quite extinguiſhed.

EXPERIMENT V.

Of the conſervation of flame under water.

THE better to examine the neceſſity of air to flame, I thought fit not only to make the ſeveral trials mentioned in this paper, whether it would live in a medium much thinner than air; but alſo to try, whether it would be able to continue in a medium many hundred times thicker than air, namely in water.

I doubted, not but many would think this both an eaſy and a needleſs inquiry, ſince eminent writers, both ancient and modern, tell us without ſcruple, that Naptha and Camphire will

will burn under water; but I had never the good fortune to be able to make them do ſo; and may be allowed to doubt, whether theſe writers, notwithſtanding their confidence, deliver what they affirm, upon experience, not bare tradition. And though in celebrated authors I have met with divers receipts of making compoſitions, that will not only burn under water, but be kindled by it; yet I have found thoſe, I had occaſion to conſider, to be ſo lamely, or ſo darkly (and ſome of them I fear ſo falſely) ſet down, that by the following compoſition, how ſlight ſoever it may ſeem, I have been able to do more, than with things they ſpeak very promiſingly of; ſince, though it will not be kindled by water, yet being once kindled, it will continue to burn under water.

AND that there might be no ſuſpicion, that whilſt the mixture continued under water, it did only, as it were, vehemently ferment, or ſuffer a violent agitation of its parts without having them kindled, till in their aſcending they were actually fired by the contact of the air, incumbent on the ſurface of the water; to obviate this ſuſpicion (I ſay) we were careful to try the experiment, not only in other veſſels, but in a large glaſs, the tranſparency of whoſe ſides, as well as that of the contained water, would permit us to ſee, for a while, the burning of our compoſition, which was ſometimes with a weight detained, and ſometimes with a forceps held, till it was conſumed, a good way under the ſurface of the water.

THE way of making the experiment is this: we took of gunpowder three ounces, of well burned charcoal one drachm, of good ſulphur or flower of brimſtone a little leſs than half a drachm, of choice ſalt-petre near a drachm and a half: which ingredients being well reduced to powder, and diligently mingled without any liquor, either a large gooſe-quil, whoſe feathery part was cut off, or a piece of a tobacco-pipe, of two or three inches long, and well ſtopped at one end, had its cavity well filled with this mixture, (inſtead of which, beaten gunpowder alone might ſerve, if it did not operate too violently, or waſte too ſoon:) for the kindling whereof, the open orifice of the quil or pipe was carefully ſtopped with a convenient quantity of the ſame mixture, made up with as little chemical oil or water, as would bring it to a fit conſiſtence. This wild-fire was kindled in the air, and the quill or pipe, together with a weight, to which it was tied to keep it from aſcending, was ſlowly let down to a convenient depth under water, where it would continue to burn, as appeared by the great ſmoak it emitted, and other ſigns, as it did in the air; becauſe the ſhape of the quill or pipe kept the dry mixture from being acceſſible to the water (that would have diſordered and ſpoiled it) at any other part than the upper orifice; and there the ſtream of kindled matter iſſued out with ſuch violence, as did inceſſantly beat off the neighbouring water, and kept it from entering into the cavity, that contained the mixture, which therefore would continue burning, till it was conſumed.

It is probable, that moſt men will conclude from this experiment, that air is not ſo abſolutely neceſſary to the duration of flame, as ſome other of our trials ſeem to argue; and that there ought to be a difference made between ordinary flames, and thoſe, that burn with an extraordinary vehemency. But my deſign being, as I long ſince intimated, rather to relate trials, than debate hypotheſes, I ſhall only add, that it may be pretended on the behalf of the opinion, that this experiment ſeems to diſprove, that, not to mention the air, that may lurk in the pores of the water, or that, which may be intercepted between the little grains of powder, whereof the mixture conſiſts, the ſalt-petre itſelf may be ſuppoſed to be of ſuch a texture, that in its very formation the corpuſcles, that compoſe it, may intercept ſtore of little aereal particles between the very minute ſolid ones, which thoſe corpuſcles are made up of. And this inexiſtence of the air in nitre may be probably argued from the great windineſs of the flame, that is produced upon the deflagration of nitre. According to this ſurmiſe, though our mixture burns under water, yet it does not burn without air, being ſupplied with enough to ſerve the turn by the numerous eruptions of the aereal particles of the diſſipated nitre itſelf.

ON this occaſion I remember, that in another paper I relate, that for divers purpoſes, and among them to remove this ſuſpicion, I ſucceſsfully tried to reproduce nitre in *Vacuo Boyliano*, that there might not be any air, or at leaſt any quantity worth heeding, intercepted between the convening particles, that by their coalitions made up the nitrous corpuſcles, which, in favour of the neceſſity of air to flame, may be pretended to be but ſo many little empty bubbles cloſe ſtopped, whoſe moiſter parts may, by the fire, that kindles the nitre, be exceedingly rarified, and in that eſtate emulate air, and violently burſt their little priſons, and throw about the fragments of them with force, and in numbers enough to make their aggregate appear ſuch a flame, as is wont to be be made by unctuous and truly combuſtible bodies; and yet this rarified ſubſtance, that thus ſhatters the nitrous particles, may really be no true and laſting air, but only vehemently agitated vapours, which preſently, upon the ceſſation of the heat, return to liquor; as we ſee, that the vapours of an Æolipile, that iſſue out after the aereal particles have been expelled, though they make a great noiſe and a temporary wind near the hole they ſtream out at, and would perhaps, if that hole were cloſe ſtopped, break the Æolipile; yet are not true and permanent air, but at a ſmall diſtance off the inſtrument return into water.

BUT though I could ſuggeſt other ſuſpicions and conjectures about the incluſion of air between the particles of ſalt-petre, yet I forbear to mention them in a writing deſigned to be chiefly hiſtorical.

EXPERIMENT VI.

Relating an odd phænomenon about the flame of a metal in our vacuum.

TO the foregoing experiments made on purpose I shall add a phænomenon afforded us by chance, and yet not unworthy to accompany the rest.

Whilst we were trying to kindle something in our exhausted receiver, it happened by some accident or other, that the combustible substance, that was to be kindled, fell besides the iron, whereby our intended trial was defeated. But whilst we were considering what was to be done on this occasion, and had not yet let in the air, that had been pumped out, the lights also continuing yet removed; we were surprised to see something burn, like a pale blueish flame almost in the midst of the cavity of the receiver, and at first suspected it to be some illusion of the eyes; but all the by-standers perceiving it alike, and observing, that it grew very broad, we looked at it with great attention, and found it to last much longer, than I remember I have seen any flame do in an exhausted receiver. I should have suspected, it had proceeded from some brimstone, sticking, without our heeding it, to some part of the iron, which we had formerly employed to kindle sulphur in our receiver, had it not been, that, besides other things, I remembered, that we had just before kept it red-hot in the fire, and consequently must have burned away any little brimstone, if there were any, that adhered to it: but though we much wondered, whence this our flame proceeded, I would not let any thing be done, that might hasten its extinction; and at length, when it expired of its self, we let in the air, which had been till then kept out, and perceived upon the concave part of the iron (which we judged to be the place, where the flame had appeared) a piece of melted metal, which we concluded had been fastened to the string, that the fuel we designed to kindle had been tied to, in order to the letting it down the more easily: and this made us conceive, that the string happening to be burned by the excessive heat of the iron, the piece of metal fell into the cavity of it, and, by the same heat, the more combustible part, which the chemists call the sulphur, was melted and kept on fire, and continued burning so long as we have related. The piece of metal was judged to be lead, but having not formerly observed such a disposition in that metal to be inflamed, I considered it attentively, and perceived, that it was some fragment, that the operator had chanced to light on, of a mixture of lead and tin, that I had (a while before, for an experiment not at all belonging to our present subject) caused to be colliquated in a certain proportion. Upon whose account it seems, the mixture of the ingredients had acquired such a new texture, as, whether by making the bodies open one another, or by what other means soever, fitted the mass to afford us the phænomenon above recited. And though I made an unsuccessful trial with a mixture of lead and tin, to produce such a flame upon the heated iron in the open air; yet the newly related experiment may suffice to argue, that there may be flames of metalline sulphurs (as the chemists call them) that will be, at least, as easily produced without the concurrence of the air, as that of common sulphur, and continue to burn in our vacuum longer than it.

THE THIRD TITLE.

Of the strangely difficult Propagation of ACTUAL FLAME *in vacuo Boyliano.*

I Have more than once observed, that some bodies (whereof I make particular mention in another paper) though they will not be turned into flame by very intense heats, and those of very differing kinds, are yet very readily kindled by an actual flame. So that the propagation of flame to contiguous bodies, that, according to the hitherto observed, and unquestioned course of things, must thereby in a moment, as it were, be actually inflamed, seems to be not only very easy, but almost infallible: and yet, that this propagation is not easy, or is perhaps scarce possible to be performed without the assisting presence of the air, may be gathered from the next following experiments; at whose titles though you will probably be surprized, in regard, that by the two first experiments of the first title of this tract, it will scarce be expected, that sulphur should be kindled in our vacuum; yet I presume your wonder will cease, when I put you in mind, that I formerly took notice to you of my having sometimes met with such sulphur as would be kindled there; and it was whilst that well-disposed parcel of sulphur lasted, that I took the opportunity of making with the flame of it the trials, to which I now proceed.

EXPERIMENT I.

An ineffectual attempt to make flame kindle spunk in an exhausted receiver.

HAVING placed the often mention cylindrical plate of iron, first brought to be red-hot, in a receiver, capable of containing two gallons of water; and having also diligently pumped out the air, we kindled a little sulphur upon the heated plate, and then a piece of of dried ſpunk, tied to a ſtring, was, by the help of a turning key, let down to the flame; and when the experiment was finiſhed, and the ſpunk was taken out, we found it in divers places, not manifeſtly altered ſo much as in colour; and in thoſe parts, that had been moſt expoſed to the flame, it was turned to a ſubſtance very differing from aſhes, being black and brittle as tinder, and, like it, exceedingly diſpoſed to be kindled upon the touch of the fire.

EXPERIMENT II.

An unproſperous attempt to make flame kindle camphire without the help of air.

AS a farther confirmation of the difficulty of propagating flame in our vacuum, we may annex the following trials.

INTO the lately mentioned receiver we conveyed the cylindrical plate of iron, made uſe of in the former experiment; and when the air had been diligently pumped out, we did, by the help of the turning key, let down upon the hot iron a piece of ſuch brimſtone, as would, in ſpite of ſo diſadvantageous a place, be kindled with that heat. A little above this ſulphur we had tied to the ſame ſtring a piece of camphire, that being a body exceedingly apt to take fire, if not, as it were to draw it, at the flame of lighted brimſtone. But our ſulphur melting with the heat of the iron cylinder, dropt unluckily from the ſtring it was faſtened to before, and for the moſt part fell off. And as ſoon as it came to the ground, where it was diſtant from the vehement heat of the metal, the flame expired, and that part of the ſulphur, that happened to ſtick to the ſide of the iron, was inflamed by it. And I, that chanced to be then in an inconvenient poſture for ſeeing the camphire, could not, becauſe of the ſmoke of the extinguiſhed brimſtone, well diſcern what became of it. But my amanuenſis, that happened to be on the beſt ſide of the receiver, affirmed, he plainly ſaw the flame of the brimſtone reached the camphire, without being able to make it flame. Which ſeemed the leſs to be doubted of, becauſe the camphire was by help of the turning key let down low enough, and if it had afforded a flame, the difference of colours betwixt that and the blue flame of ſulphur would have made it very eaſy for me to have diſtinguiſhed them.

ANOTHER trial I would have thoroughly made to kindle one piece of ſulphur in our vacuum by the flame of another, tied a little lower in the ſame ſtring, that it might firſt touch the heated iron, and be thereby ſet on fire: but, though we could find nothing, that was viſibly amiſs in the kind of ſulphur we then uſed, yet we were not able, even by a reiterated trial, to make it take fire upon the iron, where nevertheleſs it melted and ſeemed a little to boil.

A third trial was not ſo unſucceſsful; for having in the well-exhauſted receiver let down upon the very hot iron a match, made of a piece of card dipped in brimſtone, the lower extream of it was kindled by the contact of the hot iron. But though the ſulphurated part of the match thus flamed away, yet the remaining part, which was a meer piece of card, was not thereby turned into flame, nor in moſt places ſo much as ſenſibly ſcorched or blacked; though, as I remember, the match had been purpoſely dryed before-hand to facilitate its inflammation.

EXPERIMENT III.

A ſtrange experiment upon gunpowder, ſhewing, that though it were fired itſelf, yet it would not fire the contiguous grains in vacuo Boyliano.

THE preceding trials may ſuffice to manifeſt the difficulty of communicating flame, without the help of air, from one body to another, even when the bodies to be kindled are of a very inflammable nature. But becauſe there is no propagation of flame made in any bodies, that we converſe with here below, with any thing near ſuch celerity, as in the contiguous grains of gunpowder; a great heap whereof will, almoſt in the twinkling of an eye, be turned into flame by propagation from any one ſmall kindled grain; nothing ſeemed fitter to manifeſt, how much flame is beholden to air, than if ſuch an experiment could be made, as might ſhew, that, even amongſt the contiguous grains of kindled gunpowder, flame would not be propagated without the help of air. How far a trial of this nature may be made in our engine, the following narratives will beſt declare.

WE took ſome paper, and laying it upon ſome convenient part of the plate of the engine, we made upon it a train of dry powder, as long as the glaſs would well cover; then, carefully faſtening on the receiver with good cement, we ſolicitouſly pumped out the air; which done, we took a good burning-glaſs, and about noon caſt the ſun-beams through it upon the train of ſome gunpowder: where, though the indiſpoſition to accenſion was ſo great, that the powder did not only ſmoke, but melt without going off, and the operator, though verſed in ſuch experiments would not allow, that it would ſignify any thing to continue the trial any longer; yet, upon my being obſtinate to proſecute it, he, being willing to follow the experiment, rationally conſidered, that the receiver, we had been hitherto fain to uſe, was ſo opacous, as to reſiſt the entrance of many of the beams, that ſhould have their operation upon the powder: whereupon taking a finer glaſs, that was lately come in, we laid by the former, and employed that, which, by reaſon of its tranſparency, ſo little weakned the beams of the ſun, that being according to my direction held obſtinately upon the ſame parts of the train, they were able to fire ſeveral of them one after another. But though the ſun could thus kindle the powder, yet it could not make the flame propagate, but only thoſe parts, that were melted, did at length kindle and fly away, leaving the reſt unaltered, as I curiouſly obſerved, finding ſeveral little maſſes of colliquated matter in ſeveral places

of

of the train, with the powder unchanged in all the other parts of the same train, that lay in a direct line; besides that some of the little colliquated masses were contiguous to the rest of the powder, which appeared unchanged, and kindled readily, and flashed all away, as soon as I caused the burning-glass to be applied to it in the open air.

EXPERIMENT IV.

Reciting another attempt to confirm the former.

FOR further confirmation of so odd an experiment, I shall also add a short account of another made with gunpowder in our vacuum.

To try on an occasion, that need not here be discoursed of, whether, by the help of one of those little instruments, that are now used at *London*, to examine the strength of powder, we could find any difference made by the absence and presence of the air, in the resistance of the instrument, or the effects of the powder on it; we fastened it to a competently heavy and commodiously shaped weight of lead; and when it was carefully filled and primed with powder, we placed it in a receiver of a convenient bigness, whence we pumped out the air after the usual manner, and perhaps with more than usual diligence. But though at length, after the powder had long resisted the beams of the sun, concentrated on it by a good double convex burning-glass, it did, as I expected, take fire at the touch-hole, and fill the receiver with smoke; yet this kindled powder could not propagate the flame to that, which was in the box, how contiguous soever the two parcels were to one another. And when the instrument was taken out into the air, (by which it appeared how free the touch-hole was) as soon as ever new-priming, with the same sort of powder, was put to it, the whole very readily went off: and when, for further satisfaction, we caused the instrument to be new charged, and upon its taking fire only at the touch-hole in the exhausted receiver, we ordered new priming to be added, without so much as taking the instrument out of the receiver, though afterwards the receiver was closed again, but without being exhausted of air; the powder, though closely shut up in the glass, did readily go off, as well that, which was in the box or cavity of the powder-trier, as that, which lay on the outward part of the instrument. And this trial, for the main, was repeated with the like success.

EXPERIMENT V.

Briefly mentioning two differing trials, with two differing events, to kindle gunpowder in our vacuum.

YOU will easily believe, that the event of the foregoing trials seemed strange enough to the ingenious persons, that I had desired to be present at them; and perhaps, the attentive consideration of it may well enough suggest such odd suspicions and conjectures, as I have neither the leisure, nor the boldness to discourse of in this place.

BUT here I shall not dissemble my having, by a somewhat differing way, made a couple of trials, whereof, though the first may confirm the great indisposition of gunpowder to be kindled in our vacuum, yet the second seems to look another way.

THE first is summarily set down in my notes to this purpose. [A few small corns of gunpowder being included in a very small bubble freed from its air, and secured against the return of it, or any other, and then applied warily to coals covered with ashes, did not go off, nor burn, but afforded a little yellow powder, that seemed to be sulphur, and sublimed to the upper part of the glass.]

THE latter's event I found in the same paper to have been thus registered. [But two larger bubbles, though strong, whereof one had the air but in part, and the other carefully emptied, being provided, each of them, with a greater quantity of powder (though scarce enough to promise such an effect) a while after they were put upon quick coals, each of them was blown in pieces, with a report almost like that of a musket; but though this was done in a dark place, yet we did not perceive, whether or no there were any real flame produced.]

THE event of this trial seems at first sight to contradict the inference, that probably you have drawn from the foregoing experiments. But yet it may not be unworthy of our inquiry, whether this way of trial be as proper to give satisfaction to the curious, as that, made with the sun-beams, was. And I leave it to be considered, whether or no it may not be doubted, whether the going off of the gunpowder was caused by a successive, though extremely swift, propagation of real flame, from the first kindled grains to the rest; or did not proceed from this, that the coals acting strongly at the same time on the whole area, or extent of the powder, that was next to them, and this in the absence of the air, each grain was in that case, as it were, a little granado, and the heap of them, being uniformly enough acted on by the fire, they were made to go off, as to sense, all at once, as if there had been but a contemporary explosion made of them all together by the action of the external fire, rather than any true accension made by the flaming grains of the unkindled ones. As I remember I have tried, that even in the open air one may, with a burning-glass dexterously employed, make some part of a little parcel of aurum fulminans go off, whilst the neighbouring parts of the same parcel, to which the focus does not extend with heat enough, will not be made to do so.

NEW EXPERIMENTS

About the RELATION betwixt AIR and the FLAMMA VITALIS of ANIMALS.

(Sent to the ſame Perſon, to whom the former Papers were addreſſed.)

THE twenty experiments hitherto ſet down under the three foregoing titles, by ſhewing the relation betwixt air and flame in general, may be ſerviceable to the inquirers into the nature of the vital flame in particular. But yet having had occaſion to make ſome trials, that more directly regard the requiſiteneſs of air to the flamma vitalis or vital principle of animals; I ſhall now preſent you by themſelves, as many as I could light on, without being ſolicitous, that they ſhould be quite differing from each other; becauſe in ſo new and nice a ſubject, the affinity, that may be found between ſome, either in regard of the ſubjects expoſed to trial, or in the manner of making it, may be uſeful, if not neceſſary, to confirm things by the reſemblance of events, or make us proceed cautiouſly and diſtinctly in pronouncing upon caſes, where the ſucceſs was not uniform.

EXPERIMENT I.

Wherein the durations of the life of an animal, and of the flame of ſpirit of wine, included together in a cloſe veſſel, were compared.

WE took ſome highly rectified ſpirit of wine, and put about a ſpoonful of it into a ſmall glaſs-lamp, conveniently ſhaped and purpoſely blown with a very ſmall orifice, at which we put in a little cotton-wick, which was but very ſlender.

WE alſo provided a tall glaſs-receiver, which was in length eighteen inches, and contained above twenty pints of water. This receiver, which was open at both ends, was at the upper orifice (which was not wide) covered with a braſs plate, faſtened on very cloſe with good cement, for uſes, whoſe mention belongeth not to this place; and for the lower orifice, which was far the wideſt, we had provided a braſs-plate furniſhed with a competent quantity of the cement we employed to keep the air out of the pneumatical engine; by means of which plate and cement, we could ſufficiently cloſe the lower orifice (though a wide one) of our receiver, and hinder the air from getting in at it.

THESE things being thus prepared, we took the ſmall glaſs lamp above-mentioned, and having lighted it, we placed both it, and a ſmall bird, (which was a green-finch) upon the braſs plate, and in a trice faſtned it to the lower orifice of the receiver, and then watched the event; which was, that within two minutes (as near as we could eſtimate by a good minute-watch) the flame, after having ſeveral times almoſt quite diſappeared, was utterly extinguiſhed; but the bird, though for a while he ſeemed to cloſe his eyes, as though he were ſick, appeared lively enough at the end of the third minute; at which time, being unwilling to wait any longer by reaſon of ſome avocations, I cauſed him to be taken out.

AFTER he had for a pretty while, by being kept in the free air, recovered and refreſhed himſelf, the former trial was repeated again, and at the end of the ſecond minute, the flame of the lamp went out; but the bird ſeemed not to be endangered by being kept there a while longer.

AFTER this, we put in, together with the ſame bird, two lighted lamps at once, viz. the former and another like it, whoſe flames, according to expectation, laſted not one whole minute, before they went out together. But the bird appeared not to have been harmed, after having been kept five or ſix times as long before we took off the receiver.

In the tall receiver above mentioned we included a mouſe, with a lighted lamp filled with the ſpirit of wine; but before the experiment was near finiſhed, the mouſe, being at liberty within the glaſs, made a ſhift to blow out the flame; which being revived without taking out either the lamp or the animal, the ſpirit of wine burned about a minute longer, during which time the mouſe appeared not to be grown ſick, no more than it did afterwards, when, for ſome minutes after the extinction of the flame, he had been kept in the ſame cloſe and infected air.

AFTERWARDS we placed the ſame mouſe in another receiver, which ſeemed to be by a third part leſs capacious than the former, and in it we alſo fixed a piece of ſlender wax-candle, ſuch as is wont to be made up in rolls, and employed to light tobacco. This candle continued burning in this new receiver but for one minute, during which time it emitted ſtore of ſmoke; but this not hindering the animal to appear lively enough, even after we had kept him much longer in that infected air, the ſame candle, without being taken out, was lighted again, but burned not ſo long as before; yet it ſufficed to darken the receiver, and therefore probably much to clog the included air, in which,

which neverthelefs the moufe being kept, by our guefs, eight or ten minutes longer, he appeared, neither when he was taken out, nor a while before, to have received any confiderable harm by his detention there.

EXPERIMENT II.

Of the duration of the life of a bird, compared with the lafting of a burning candle and coal in our vacuum.

WE took a green-finch and a piece of candle of twelve to the pound, and included them in a great capped receiver, capable of containing about two gallons, or fixteen pound of water, which was very carefully cemented on to the pump, that no air might get in or out. In this glafs we fuffered the candle to burn till the flame expired, (which it did, in more than one trial, within two minutes or fomewhat lefs;) at which time the bird feemed to be in no danger of fudden death; and, though kept a while longer in that clogged and fmoky air, appeared to be well enough, when the receiver was removed. Afterwards, we put the fame bird into the receiver with a piece of a fmall wax taper, whofe flame, though it lafted longer than the other, yet the bird outlived it; and it was judged he would have done fo, though the flame had been much more durable. After this, we included the fame bird with the firft-mentioned candle in the receiver, which we had caufed to be often blown into with a pair of bellows, to drive out the fmoke and infected air; and then beginning to pump out the air, we found, that the flame began more quickly to decay, and the bird to be much more difcompofed, than in the former experiments; but ftill the animal outlived the flame, though not without convulfive motions. The experiment we repeated with a piece of the fore-mentioned taper, and the fame bird; which, though caft into threatening fymptoms upon the gradual withdrawing of the air, outlived, not only the flame, but the fmoke too, that iffued from the kindled wick, which circumftance was alfo obferved in the preceding trial.

LASTLY, having freed the receiver from fmoke, and fupplied it with frefh air, we put in with the fame bird a piece of charcoal of about two inches in length, and half an inch in breadth, which had been, juft before it was put in, well blown with a pair of bellows, that it might be freed from afhes, and thoroughly kindled; and made hafte to pump out the air. This diligence was continued not only till none of the fire could be difcerned by any of the by-ftanders, but till, in our eftimation, (which the event juftified) it was irrecoverable by the admiffion of the outward air; which at its coming in found the bird very fick indeed, but yet capable of a very quick recovery. And this experiment was, with the fame animal and coal re-kindled, tried over again with the fame fuccefs.

WHETHER this furvival of animals, not only to a flame, that emits ftore of fuliginous fteams, as in this trial, but to that, which is made of fo pure a fuel as fpirit of wine, that affords not fuch fteams, as in the former experiment; whether, I fay, this furvival proceed from this, that the common flame and the vital flame are maintained by diftinct fubftances or parts of the air; or, that common flame making a great wafte of the aerial fubftance, they both need to keep them alive, cannot fo eafily as the other find matter to prey upon, and fo expires, whilft there yet remains enough to keep alive the more temperate vital flame; or, that both thefe caufes, and perhaps fome other, concur to the phænomenon, I leave to be confidered.

EXPERIMENT III.

Of what happened to the light of glow-worms in the exhaufted receiver.

FOR the fake of thofe learned men, that have thought the light of glow-worms and other fhining infects to be a kind of effulfion of the biolychnium, or vital flame, that nature has made more luminous in thefe little animals than in others; and which a very eminent phyfician of the college of *London* affirms to have felt in a warm climate more than fenfibly hot; I fhall fubjoin on this occafion fome trials made on glow-worms, which elfe fhould be referred to thofe experiments of mine about the relation betwixt air and light, that you were formerly pleafed to publifh.

WE took two glow-worms, that fhone vividly enough, efpecially one of them, whofe light appeared ftrong and tincted, as if it had been tranfmitted through a blue glafs: thefe we laid upon a little plate, which we included in a fmall receiver of finer glafs than ordinary, that we might the better fee what would happen: and having for the fame purpofe removed the candles, that no other light might obfcure that of the infects, we waited in the dark, till that was confpicuous, and then ordered the air to be begun to be pumped out; and, as we expected, upon the very firft exfuction there began to be a very manifeft diminution of the light, which grew dimmer and dimmer, as the air was more and more withdrawn, till at length it quite difappeared, though there were young eyes among the affiftants. This darknefs having been fuffered to continue a long while in the receiver, we let in the air again, whofe prefence, as we looked for, reftored at leaft as much light as its abfence had deprived us of. This experiment was repeated with one more of thofe infects; and the event was, that they all three gradually loft their light by the exhauftion of the receiver, and regained it, with fome increafe, as was judged) by the return of the air. And in this experiment we let in the air by degrees, and with an interval or two, to obferve, as we did, that, as the diminution of light was greater and greater, when the air was more and more withdrawn, fo the returning fplendor was gradually increafed, as we pleafed to let in more and more air upon the worms.

EXPE-

EXPERIMENT IV.

Containing a variation and improvement of the foregoing trial.

BUT here I foresaw, it might be suspected, that the disappearing of the light in our exhausted receiver did not so much proceed from any real, though but temporary, extinction or eclipse of it, as from this, that the glow-worms having, as I have often observed, a power of drawing the luminous part into the opacous part of their body, they might, finding themselves prejudiced by the withdrawing of the air, hide their light from our eyes, without losing it, till being again refreshed by the return of the air, they might be invited to protrude it again into the transparent part of their tails. This scruple seeming grounded upon the nature of the thing, I thought it worth while to remove it by the help of another observation, that I long since made, and have mentioned elsewhere about glow-worms. Which is this, that, if they be killed whilst they are shining, their luminous matter may continue to shine for a good while after it is taken out of their bodies; and accordingly having put some of that, we took out of the forementioned insects, upon a little paper, and included it in the receiver we employed, the candles being removed, we perceived it to shine vividly enough before the pump was set on work, and afterwards to grow dimmer and dimmer, as the air was more and more drawn out, till at length it quite vanished; and it re-appeared immediately upon the air's return. This experiment was reiterated twice more with the same success for the main. But we took notice, that the luminous matter, after the air was let in, seemed to us not only to have regained its former degree of light, but sensibly increased it, (as it once happened also in the experiment made on the living worms) which, whether it was caused by any real change made by the recess and access of the air in the matter itself, or by the greater accustomance of our eyes to the darkness of the place, I dispute not; and shall only add this phænomenon of one of our trials, that having a mind to see, whether a very little proportion of returning air would not suffice to restore some little light to the disappearing matter, it was somewhat strange to observe, that so very small a quantity of air, as was let in before the light was revived, was enough to make it become plainly visible, though but dim; in which state it continued, till we thought fit to let in more air upon it. Farther trials I could not make with these glow-worms, having received them but that night out of the country, and being the next morning to begin a journey.

EXPERIMENT V.

Wherein the former inquiry is farther prosecuted.

AFTER the lately mentioned trials we made with the glow-worms, having procured two or three other of those insects, whereof one was judged to be as large as three ordinary ones, we found, when we had brought them out of the country to *London*, that this great worm was dead, as far as we were able to judge, and finding him to retain a considerable degree of luminousness in the under part of his tail, we put him into the small receiver formerly mentioned, to try, whether, after the death of the animal, the shining matter would retain its former properties; but at the first time the air was pumped out after the usual manner, the light was not only not abolished, but continued vivid enough, and so it did, when the air being let in, and again withdrawn, the trial was made a second time. But being unwilling to abandon the experiment till we tried it yet further, I caused the receiver to be exhausted yet once or twice more, and at length I perceived, that the light began to diminish, as the air was withdrawn; and last of all, it so disappeared, that the by-standers could not see it, whereas upon the readmission of the air, the light shone vividly as before, if not more bright. This experiment was reiterated with the like success, and in both these times the like happened to the light of the dead one, and of a living one, that we included with it, to be able to compare them together; though there were this disparity betwixt them, that the luminous part of the dead worm was much larger than that of the living, and the light of the latter appeared of a very greenish blue, whereas that of the former seemed to be of a white yellow.

EXPERIMENT VI.

Made to examine, whether animals be heavier dead than alive.

IT is a received tradition, that bodies, when dead, are much heavier than the same were when alive: the matter of fact being taken for granted, some will perhaps ascribe the change to the utter inability of a dead body any way to assist those, that endeavour to remove it. But, according to the general opinion, this difference proceeds from the total extinction or recess of the spirits vital and animal, which being supposed to be not only agil but light, lessened the weight of the body they enlivened; and flame being conceived to be the lightest among bodies here below, it is not improbable, that some will ascribe the phænomenon to the levity of the flame, which by being diffused through the body of an animal, and vivifying it, deserves the name of vital. But I would not advise any to rely on this conceit, till they are duly satisfied of the truth of the matter of fact; which because I have not yet found, that any has endeavoured to try, I shall on this occasion give you the following transcript of one of my notes about statical experiments.

A mouse, weighing about three drachms and a half, being put in one of the scales of a very nice balance, was counterpoised together with a string, that was tied about his neck like a noose, and after a while, by drawing the ends of it, was there strangled. As soon as we judged him quite dead, we weighed him again, and though nothing was seen to fall from him; yet

yet, contrary to the received tradition, that bodies are much heavier dead than alive, we found the weight to have lost about $\frac{7}{8}$ of a grain; which probably proceeded from the avolation of divers subtile particles upon his violent and convulsive struglings with death. But this was no more than an experiment of this kind, made some years ago, induced me to expect and foretel.

AFTERWARDS in a larger balance, but a very good one, purposely made for nice experiments, we took a very young catlin, of between ten and eleven ounces in weight, and caused him to be strangled on the same scale, wherein he had been put. But he could not be dispatched so soon as an ordinary full grown animal; so that by that time he was quite dead, we found him not only not to be grown heavier, but lighter by four grains; which did not much surprize us, having elsewhere noted the life of so very young creatures of that kind not to be easily destroyed for want of respiration. And I remember, that, for trial's sake, another catlin of the same litter with this I have mentioned, being included in a receiver, wherein another animal of that size might probably have been dispatched in two or three minutes by the pumping out of air, was kept there somewhat above a quarter of an hour before he appeared to be quite dead.

ADVERTISEMENT.

THESE two following attempts falling into the hands of the author after the preceding experiments were printed, it was thought fit to annex them here for the affinity of the subject.

An ATTEMPT to produce LIVING CREATURES in *Vacuo Boyliano*.

IN reference to the opinion of those naturalists, that hold the seeds of living creatures to be animated, and especially to the hypothesis of those learned men, that assert the flamma vitalis lately mentioned; it may be an enquiry of moment, whether or no in the seminal principles, or rudiments of animals, the manifest operations of life may be excited without the concurrence of the air, whose interest in the production and conservation of flame may be gathered from the foregoing experiments. For, it seems likely to prove no inconsiderable discovery in reference to the lately mentioned hypothesis, if it be found, that the principle of life in seminal rudiments needs, as well as other flames, the concurrence of the air to actuate it.

I thought fit, therefore, notwithstanding the great and almost insuperable difficulties, which it was easy enough for me to foresee I should meet with, to attempt the hatching of eggs in our vacuum: but though I made some unsuccessful trials of this kind, in order to a discovery about respiration, (not here to speak of the attempts I made about the animation of putrid matter,) yet leaving the mention of them to its proper place, I shall only take notice in this, what directly concerns the present inquiry. Considering then, that pregnant females cannot be made to live and bring forth young in our exhausted receiver, and that the eggs of birds, and such greater animals, do, in this colder climate of ours, require to be hatched by the incubation of the females, or other birds; I thought the fittest subjects I could both make choice of, and procure for the designed experiments, would be the eggs of silk-worms. For, having many years since tried several things about those insects, and among others found, that their eggs would be hatched, not only by the heat of one's body, (though that be the usual way,) bur by the warmth of the sun even here in *England*, if they be kept till the spring be far enough advanced: remembering this, I say, I got a good number of silk-worms eggs; and having caused three conveniently shaped, but very small receivers, to be purposely made, that differed very little (and that accidentally) either in size or figure, we conveyed into each of them, together with a small stock of mulberry-leaves, such a number of eggs, as we thought sufficient to make one morally secure, that at least some of them were prolifick: this done, we carefully exhausted one of them, and secured it against the return of the air; the two others we left full of air: but having left in one a little hole for the air to come in and get out at, we stopped the other so close, as to hinder all intercourse between the included air and the external. All things being thus prepared, we exposed the receivers to a south-window, where they might lie quiet, and where I either came, or sent to look on them from time to time; the spring being then so far advanced, that I supposed the heat of the sun would be of itself sufficient to hatch them in no long time.

As to the success of this trial, my not being able to find any register of the particular phænomena, that occurred, keeps me from venturing to relate it very circumstantially; but this I remember in general, that both I and others took notice, that in the unexhausted receivers there were divers eggs hatched into little insects, that perforated their shells, and crept out of them; though afterwards, for want of change of food, or air, or both, few or none of them proved long-lived. But though the eggs in these receivers began to afford us little animals in a few days; yet the eggs in the exhausted receiver did not, in many more, afford us any. And though I will

will not venture to ſay, how long preciſely we kept them in the ſame window, after ſome of the above-mentioned eggs were hatched; yet (if I much miſtake not) it was, from firſt to laſt, about three or four times as long; and I remember, we kept them till it was thought to no purpoſe to wait any longer, and agreed in imputing the not hatching of the eggs by the ſo long continued action of the ſun to the abſence of the air.

WHAT other phænomena occured to us in making this experiment, and another not unproſperous one upon the eggs of flies, you may expect, when I can light on my notes about them, or have my memory refreſhed by thoſe, that aſſiſted at the making of them.

An ATTEMPT made upon GNATS in our Vacuum.

I Elſewhere mention, that it has been obſerved by a couple of our virtuoſi (whom I there name) and ſeveral times by me, that here in *England* multitudes of gnats are generated of little animals, that live, for a part of the ſummer, like fiſhes in the water; and conſidering, that by theſe a very unuſual paſſage is made from ſwimming to flying animals, I thought them very fit ſubjects, whereon to make the following experiment.

[PARTLY to try, whether at leaſt an animal already living and moving in our vacuum may be able to attain the perfection due to it, according to the courſe of nature; and partly to examine, whether, in caſe he ſhould attain it, at leaſt the lighter ſort of winged inſects may be able to fly in that place; and partly to diſcover, whether an animal, that had long lived in our vacuum, would, when turned to a fly, be able to continue alive without reſpiration, he had never been accuſtomed to, in its priſtine form or ſtate; we took divers of thoſe little ſwimming creatures, which in autumn, eſpecially towards the end of it, are wont to be turned into gnats, and having put a convenient number of them together in a fit quantity of rain-water, wherein they had been found and kept, into a ſmall receiver, the air was pumped out, and the veſſel ſecured againſt its return, and then ſet aſide in a place, where I could obſerve, that the day after ſome of theſe little animals were yet alive and ſwimming to and fro, not without minute bubbles adhering to them; but at the end of a day or two after that, I could not perceive any of them to ſurvive their dead companions, nor did any of them recover, when freſh air was let in upon them. But though this experiment were the beſt I was then able to make, yet I reſolved, if God ſhould vouchſafe me life and health, to repeat it the enſuing autumn; that, wherein it was made, proving ſo cold and unſeaſonable, that a number of theſe little creatures, put up with water into another ſmall receiver, died all within a few days, though none of the air was exhauſted; and ſeveral, that I kept in an ordinary glaſs, that was divers times unſtopped to give them freſh air, did yet periſh at no ordinary rate. And I confeſs (as unkind as this trouble of mine may ſeem to the air,) that the failing of this and ſome other experiments of producing animals in our exhauſted receivers was the more unwelcome to me, becauſe I had, and have ſtill a great deſire to ſee, if it be poſſible, what would happen to animals, which had been produced in a place free from the preſſure of the atmoſphere, as if they had been born in *Epicurus*'s imaginary intermundane ſpaces, upon their coming to be ſuddenly ſurrounded with our heavy air, and having their tenderly framed bodies expoſed to its immediate preſſure.]

NEW EXPERIMENTS ABOUT EXPLOSIONS.

(Annexed, by way of APPENDIX, to the former PAPERS.)

FORASMUCH as ſome of the learned men, that are the grand aſſertors of the flamma vitalis (whoſe opinion occaſioned my preſenting you the foregoing experiments) do alſo, with the juſtly famous Dr. *Willis*, explicate many of the motions of animals, eſpecially thoſe performed in the muſcles, by the exploſions made of certain juices or fluid ſubſtances of the body, when they come to mingle with each other: and foraſmuch alſo as I do not remember, I have heard the maintainers of this hypotheſis inſiſt on other inſtances in favour of it, than the going off of gunpowder; which being not a liquor, but a conſiſtent and brittle body, and requiring for its exploſion either actual fire, or a far intenſer heat, than can be ſuppoſed natural in men, and other animals; I was induced to ſuſpect, they were not yet provided with better examples; and therefore I preſume, it will be looked upon, as a thing neither uſeleſs, nor altogether impertinent, if, without offering to determine any thing about the truth of the opinion, I ſupply the embracers of it with two or three examples of exploſions made by the bare mingling of liquors; which I ſhall borrow from the elſewhere-mentioned notes, that I drew up ſome years ago, in order to the improvement of ſome parts of phyſick.

EXPERIMENT I.

Of an exploſion made with the ſpirits of nitre and wine.

WE took ſpirit of nitre, ſo ſtrong, that the fumes made the upper part of the glaſs it was kept in, always reddiſh; and having put but one ounce of it into a bolt-head, with a long neck, capable to contain, as we gueſſed, twelve or ſixteen times as much, we cauſed an equal weight of alcohol, or highly rectified ſpirit of wine, to be taken, and a little of it being put to the ſpirit of nitre, it preſently made ſo ſtrong and quick an expanſion or exploſion, that ſome of it flew out of the glaſs, and hit againſt the cieling of the room, (where I ſaw the mark of it) and falling upon his face, that held the glaſs, made him think (as he told me) that fire had fallen upon it, and made him run down the ſtairs like a mad-man, to quench the heat at the pump. Wherefore, bidding the laborant proceed more warily, I ordered him to put into the bolt-head but part of a ſpoonful of ſpirit of wine at a time; and yet, at each of a pretty many affuſions, that I ſtayed to ſee the effect of, there would be a great noiſe, as of an ebullition, though no ſtore of froth produced, and accompanied with ſo great a heat, that I could not hold the glaſs in my hand; and immediately there would iſſue out a copious and red ſmoke; to which when I cauſed a little candle to be held, though at near half a foot diſtance from the top of the bolt-head, it would preſently take fire, and burn at the top of the bolt-head, like a flame at the upper end of a candle, till I cauſed it to be blown out, that freſh ſpirit of wine might be poured in; which, when it was all mingled with the other liquor, the heat and conflict ceaſed.

DIVERS other phænomena relating to this experiment, (by which I intended to make out more things than one) belong not to our preſent ſubject, and are already ſet down in other papers. But yet it will be pertinent to ſhew in this place, that the noiſe and ebullition produced in this mixture is not unaccompanied with a briſkly expanſive, or an exploſive motion. To make then an experiment to this purpoſe, and yet avoid the danger, whereto the making of it unwarily might expoſe both the veſſels and us; we put an ounce of ſuch ſtrong ſpirit of nitre, as is abovementioned, into a moderately large bolt-head furniſhed with a proportionable ſtem, over the orifice of which we ſtrongly tied the neck of a thin bladder, out of which moſt part of the air had been expreſſed, and into which we had conveyed a ſmall vial, with a little highly rectified ſpirit of wine: then this vial, that before was cloſed with a cork, being unſtopped, without untying or taking off the bladder, a ſmall quantity, by gueſs not a quarter of a ſpoonful, of the alcohol of wine, was made to run down into the ſpirit of nitre, where it preſently produced a great heat and commotion, and blew up the bladder, as far as it would well ſtretch, filling alſo the ſtem and cavity of the glaſs with very red fumes, which preſently after forced their way into the open air, in which they continued, for a good while, to aſcend in the form of an orange-coloured ſmoke.

EXPERIMENT II.

Of an exploſion made with oil of vitriol and oil of turpentine.

IF I had at hand the papers, you have divers times heard me ſpeak of, about heat, I could give you the particulars of ſome trials about exploſion, that perhaps you would think more pertinent than deſpicable: but, for want of thoſe papers, I muſt content myſelf to tell you in general,

general, that I remember, that I have, more than once, taken ſtrong oil of vitriol, and common oil of turpentine, and warily mixed them in a certain proportion, by ſhaking them very well together; and that thereupon enſued (what I had reaſon to look for) ſo furious an agitation of the minute parts of the mixture, and ſo vehement or ſudden expanſion or exploſion, as did not only ſeem ſtrange to the ſpectators, but would have proved dangerous too, if I had not taken care before-hand, that the trials ſhould be made in a place, where there was room enough; and that even the operator, that ſhook the veſſel, ſhould ſtand at a convenient diſtance from the mixture.

EXPERIMENT III.

About an exploſion made by two bodies actually cold.

I Remember not, that I found the aſſertors of exploſions in animals to have taken notice of a difficulty, which to me ſeems not uneaſy to be obſerved, and yet very worthy to be cleared. For it is known, that fiſhes, and thoſe eſpecially of the vaſter ſort, can move and act in the waters with a ſtupendious force; and yet it is affirmed by thoſe, that pretend to know it, that the blood of moſt fiſhes is ſtill actually cold: and I remember, I found the blood even of thoſe I diſſected alive to be ſo. From whence moſt men would argue, that even in the vaſt ſea-monſters there can be made no exploſions, theſe being ſtill effected by or accompanied with an intenſe degree of heat.

It were incongruous to my deſign, to examine this difficulty, as it directly regards the exploſions, ſaid to be made in animals: but ſpeaking of exploſions in general, perhaps I might do the favourers of vital ones (if I may ſo term them) no unacceptable piece of ſervice, by experimentally ſhewing, that it is not impoſſible, though it ſeem very unlikely, that exploſions ſhould be made upon the mixture of bodies, which, whilſt they ſeem to put one another into a ſtate of effervescence, are really cold, nay, colder than before their being mingled. Of theſe odd kind of mixtures, I remember I have in another * paper ſet down ſome trials, that I made to other purpoſes, as well with two liquors, as with a liquor and a ſolid body; which later ſort I there mention my having made by an improvement of an experiment of the excellent Florentine virtuoſi. And among thoſe trials I find one, whoſe pertinency to the matter in hand invites me to annex as much of it, as is proper in this place.

There were put two ounces of powdered ſal armoniac into a pretty large glaſs-tube, hermetically ſealed at one end; into the ſame a ſlender glaſs-pipe, furniſhed with two ounces of oil of vitriol, was ſo put, that, when we pleaſed, we could make the liquor run out into the larger tube, which, after theſe things were done, was cloſed exactly, ſo that nothing might get in or out. My deſign was, that this inſtrument ſhould be ſo warily inverted, that the operator might get out of the way, and the oil of vitriol, falling ſlowly upon the ſal armoniac, ſhould, without producing any heat, produce an exploſion not dangerous to the by-ſtanders. But whilſt I was withdrawn to a neighbouring place to write a letter, the operator not ſtaying for particular directions, raſhly inverted the inſtrument, without taking care to get away: whence it happened, that as ſoon as ever the contained liquor, being too plentifully poured out, came to work on the ſal armoniac, wherewith it is wont to produce cold, there was ſo ſurpriſing and vehement an expanſion or exploſion made, that with a great noiſe, (which, as the laborant affirmed, much exceeded the report of a piſtol,) the glaſſes were broken into a multitude of pieces, many of which I ſaw preſently after, and a pretty deal of the mixture was thrown up with violence againſt the operator's doublet and his hat, which it ſtruck off, and his face; eſpecially about his eyes, where immediately were produced extreamly painful tumors, which might alſo have been very dangerous, had I not come timely in, and (to add that upon the by) made him forthwith diſſolve ſome ſaccharum Saturni in fair water, and with a ſoft ſpunge keep it conſtantly moiſtened by very frequently renewed applications of the liquor: by God's bleſſing upon which means, within an hour or two, the pain, that had been ſo raging, was taken away, and the fretting oil of vitriol was kept from ſo much as breaking the ſkin of the tumors, that it had made.

The firſt part of the relation of this trial might have been omitted, or at leaſt ſhortened, unleſs I had deſigned to communicate unto you a way of doing what I do not know to have been attempted by others, namely to put bodies together, when and by what degrees one pleaſes, after the glaſs, that contains them, has been hermetically ſealed up; which mechanical contrivance, eſpecially as it may be varied, may be, as I have tried, uſefully applied to more purpoſes, than it were proper here to take notice of.

But to conclude with a word or two touching the foregoing experiment; I ſhall only add, that another time we made a like trial a ſafer way, by tying a bladder ſo to the top of a bolt-head, into which we had before-hand put the ſal armoniac, that, by warily moving the bladder, whence the air had been expreſſed, we could make ſome of the ſal armoniac, we had lodged in its folds, to fall upon the liquor, with which it preſently made an exploſive mixture, that quickly blew up the bladder.

But theſe, Sir, are bare conjectures, left to be, after a farther diſcuſſion, (if you think them worthy of it) determined by you, to whom as theſe papers are addreſſed, ſo they are alſo ſubmitted by the writer of them.

I am,

SIR,

Yours, &c.

* About the production or extrication of air.

A N

AN

HYDROSTATICAL DISCOURSE,

OCCASIONED BY

The OBJECTIONS of the Learned Dr. *HENRY MORE*,

AGAINST SOME

EXPLICATIONS of NEW EXPERIMENTS made by Mr. *BOYLE*;

AND NOW

Publiſhed by way of PREFACE to the three enſuing TRACTS.

To the READER.

WHEN I determined to write this polemical diſcourſe, I did not forget, that when I firſt ventured ſome of my trifles abroad into the world, my friends obtained from me a promiſe, that after I ſhould have anſwered the two firſt, that ſhould expreſsly write againſt me (which happened to be the learned *Linus* and Mr. *Hobbes*,) to ſhew, that I was not altogether unacquainted with a way of defending truths, I would afterwards write no book in anſwer to any, that ſhould come forth againſt mine; for, not only my friends, but I, thought it enough for a perſon, that never was a gown-man, to communicate freely his thoughts and experiments to the curious, without deſpairing, that thoſe things, that ſhould be evidently true, would be able to make their own way, and ſuch as were very probable, would meet with patrons and defenders, in ſo inquiſitive an age as ours. And indeed I do not find, that either upon the account of my writings, or ingenious men's opinion of them, I have had much cauſe to repent the keeping of my promiſe, notwithſtanding the writings, that have impugned ſome of mine, but without much prejudice, that I know of, either to the propoſed truths, or the propoſer of them. And therefore I ſhould not at all have entered upon a defence of what is attacked of mine by the learned Dr. *More*, if I had not ſuppoſed, that it would not require a book, but might be diſpatched in a preface: for having by me ſome little tracts, that ſhould, though the doctor had never engaged me, have been imparted to the publick, and obſerving, that the new experiments contained in one or other of them would by an eaſy application be brought to confirm my formerly delivered explications of other phænomena, and enervate the doctor's objections againſt them; I thought I might, without long troubling the reader, or my ſelf, defend what I looked upon as truth, by anſwering ſome incidental paſſages of the doctor's diſcourſe, and referring the reader, for the main points in controverſy between us, to thoſe experiments of the following tracts, which clearly contain the grounds of deciding them. But yet this conſideration would not perhaps have engaged me to write the following preface, if the objections I was to anſwer had not been, by a perſon of ſo much fame, propoſed with ſo much confidence; and though with very great civility to me, yet with ſuch endeavours to make my opinions appear not only untrue, but irrational and abſurd, that I feared his diſcourſe, if unanſwered, might paſs for unanſwerable, eſpecially among thoſe learned men, who,

who, not being versed in hydrostaticks, would be apt to take his authority and his confidence for cogent arguments; and who (not observing how liberal some men are of titles to the arguments, that please them) would make a scruple of thinking, that what is with great solemnity delivered for a demonstration in a book of metaphysicks, can be other than a metaphysical demonstration. The care therefore, that what I judge to be true, should not be made to pass for absurd, which is a degree beyond what is merely erroneous, by being so severely handled by a person of Dr. *More*'s fame and learning, induced me to begin the following paper; which should have been shorter than now it is, but that I was persuaded to lengthen it beyond what was either necessary or designed, that I might, by the addition of some few thoughts and experiments on the occasions, that were suggested to me, endeavour to clear up and confirm some hydrostatical truths, that, I fear, are but by very few either assented to, or perhaps so much as understood, and so might make the reader amends for the trouble I was forced to give him in a dispute, which I apprehended he might otherwise think himself but little concerned in. And he will, I hope, easily discern, that I have no mind to burthen him in my preface with things not pertinent to the scope of it, if he take notice, that both for his sake and the learned doctor's, (whose civility I would not leave unanswered) I have restrained my self to the defensive part, forbearing to attack any thing in his Enchiridium Metaphysicum, save the two chapters, wherein I was particularly invaded.

But though I have declined the delivering my opinion of the doctor's book, yet I dare not forbear owning my not being satisfied with that part of his preface, which falls foul upon Monsieur *des Cartes*, and his philosophy. For though I have often wished, that learned gentleman had ascribed to the divine author of nature a more particular and immediate efficiency and guidance, in contriving the parts of the universal matter into that great engine we call the world; and though I am still of opinion, that he might have ascribed more than he has to the supreme cause, in the first origin and production of things corporeal, without the least injury to truth, and without much, if any, prejudice to his own philosophy; and though not confining myself to any sect, I do not profess myself to be of the Cartesian: yet I cannot but have too much value for so great a wit as the founder of it, and too good an opinion of his sincerity in asserting the existence of a deity, to approve so severe a censure, as the doctor is pleased to give of him. For I have long thought, that in tenets about religion, though it be very just to charge the ill consequences of men's opinions upon the opinions themselves; yet it is not just, or at least not charitable, to charge such consequences upon the persons, if we have no pregnant cause to think they discern them, though they disclaim them. And since men have usually the fondness of fathers for the offspring of their own brains, I see not, why *Cartesius* himself may not have overlooked the bad inferences, that may be drawn from his principles, (if indeed they afford any such) since divers learned, and not a few pious persons, and professed divines of differing churches, have so little perceived, that the things objected are consequent to such principles, that they not only absolve them as harmless, but extol them as friendly and advantageous to natural religion. And I see not, why so great and radiant a truth, as that of the existence of a God, that has been acknowledged by so many meer philosophers, might not as well impress itself on so capable an intellect, as that of Monsieur *des Cartes*; or that so piercing a wit may not really believe he had found out new mediums to demonstrate it by. And since the learned *Gassendus*, though an ecclesiastick, had been able, as well safely, as largely to publish the irreligious philosophy of *Epicurus* himself; it seems not likely, that so dexterous a wit as that of Monsieur *des Cartes* could not have proposed his notions about the mechanical philosophy, without taking so mean a course to shelter himself from danger, as in the most important points, that can fall under man's consideration, to labour with great skill and industry to deceive abundance of ingenious men, many of whom appeared to be lovers of truth, and divers of them lovers of him also. And I am the more averse from so harsh an opinion of a gentleman, whose way of writing, even in his private letters, tempts me very little to it, because I cannot think him an atheist, and an hypocrite, without thinking him (what Dr. *More* has too much celebrated him) to call him a weak head, and almost as bad a philosopher, as a man. For, as far as I understand his principles, some of the most important points of his philosophy (which, if it were needful, I could name) are interwoven with the truth of the existence of a God, or do at least suppose it, and are not demonstrable without it. But I must not prevent the Cartesians, who, now he cannot do it for himself, I doubt not will apologize for their master; though looking upon him as a great benefactor to, though not the first founder of the mechanical philosophy, I could not consent, by a total silence upon such an occasion, to become any way accessary to the blemishing of his memory.

AN HYDROSTATICAL DISCOURSE, &c.

SIR,

UPON the advertisement you gave me yesternight, that I was particularly concerned in the learned Dr. *More's* Enchiridium Metaphysicum, I this day turned over the leaves of one, which I have freshly received from the reverend author himself: and being assisted by the series of the titles, I quickly lighted on that part of the book, whose subject made me expect to find myself questioned there, as I presently found I was. For though that civil adversary is pleased to omit my name, and, the farther to disguise it, employs, instead of it, a great and unmerited encomium; yet by the book he cites, and the experiments, against which he argues, it is very easily discoverable, that his objections are meant against me, who see yet no cause at all to be scrupulous to own my name, and the doctrine delivered in the passages heis pleased to oppose.

I doubt not but you will presently desire to know, what I think of this much expected work; but when I have told you, that I have gained time to peruse only (and that but cursorily) the twelfth and thirteenth chapters, you will, I question not, excuse a person, that does exceedingly want health, and yet wants not almost continual avocations, if I now content myself to give you my thoughts of that part of the newly-mentioned chapters, which properly relates to me; I say, that part of the chapters, because there are others, wherein I need not interest myself. For, to omit other paragraphs, the doctor has, in the former part of the twelfth chapter, thought fit to separate from my explication of the phænomena in question betwixt us that of the learned *Henricus Regius*; and the latter part of the same chapter he employs in an ingenious dispute against those, that would have the aerial particles act with perception and design, and (as he Sect. 16,
speaks) *pro re nata*; which opinion you will 17.
easily believe I neither was of, nor am like to adopt.

It remains then, that setting aside those discourses of the twelfth chapter, wherein it is needless, that I should make myself a party; I proceed to consider those paragraphs, which will be easily guessed to be levelled at my explications, and by which I must confess, I cannot at all be yet convinced of their being false ones. But in doing this, I shall not only, in compliance with my present haste, but also to express my respect to the learned doctor, forbear to say any more, than what I shall judge requisite to answer the objections, that directly concern my own explications, without meddling, by way of retaliation, with his hypotheses or opinions, or endeavouring to set any passages of his writings at variance among themselves, or to take those little advantages, which are usually sought for by disputants.

I shall not trouble you, nor tire myself with any schemes, since the doctor has taken the pains to insert those, that are necessary for his purpose, in his book, and I have not my own at hand. Wherefore, not doubting, that you have by you those books of mine he refers to, and supposing, that you will, whilst you are reading, have also this book, with the inserted schemes before your eyes, I shall not spend time on any further preamble, but immediately enter upon the consideration of the objections I am to answer.

The FIRST SECTION.

CHAP. I.

THE first explication of mine, that the learned doctor animadverts upon in his twelfth chapter is that, which I give in the thirty-third of my physico-mechanical experiments, touching the spring and weight of the air; where I relate, that the sucker in the air-pump of our engine, having been forcibly depressed to the lower part of the brass cylinder, which yet was carefully closed at the top, so that the cavity of the cylinder was empty of air; this sucker, I say, would, in this case, appear spontaneously to remount towards the top of the cylinder, though it were clogged with a hundred pound weight to hinder its ascent. Which phænomenon I ascribed to this, that the sucker being, by the withdrawing of the air in the cylinder, freed from the wonted force of the springy air, that endeavoured to depress the internal part of it, was not enabled, by the appendant weight, to resist the pressure of of an atmospherical cylinder equal in diameter to it, which, pressing against its lower or external surface, endeavoured to impel it up.

Now the doctor having, in the two first paragraphs, made a description of my engine, (which I shall now pass over) does in the third teach us, that the corporeal cause, if there be any, of the ascent of the sucker, must be either in the sucker itself, or in the almost exhausted cavity of the cylinder, or, lastly, in the external air. Which premised, he does in the same third section, and in the fourth, endeavour to prove at large, that the cause is to be derived neither from the one, nor from the other of the two first. And therefore I, that maintain neither of the opinions he disputes against, shall leave those paragraphs of his untouched. Nor shall I meddle with the fifth, sixth and seventh, where he argues against the explications of some, that would solve the phænomenon upon some Cartesian grounds, and as well amply, as particularly against the solution, that he supposes would be given of it, congruously to his own sentiments by the learned *Regius*. These discourses, I say, of the doctor's I leave untouched; because it is at length in the eighth paragraph, that he impugns that solution of the phænomenon, which he ascribes to me, whose opinion he first delivers, though not just in the terms I would express it myself; yet I dare say very sincerely, and so near my sense, that I shall forthwith pass from the eighth section to the beginning of the ninth, where he begins to propose his objections, which he is pleased to usher in with a complement to me, that I should be very vain, if I looked upon as any thing more than a complement.

To his first objection, proposed in these
Pag. 139. words, *Primò enim, si hæc solutio verè mechanica sit, quæ tandem causa verè mechanica assignari potest gravitationis singularum particularum, totiúsque atmosphæræ in suis locis? nam quod materiam subtilem attinet*, &c. I answer, that I did not in that book intend to write a whole system, or so much as the elements of natural philosophy; but having sufficiently proved, that the air, we live in, is not devoid of weight, and is endowed with an elastical power or springiness, I endeavoured, by those two principles, to explain the phænomena exhibited in our engine, and particularly that now under debate, without recourse to a fuga vacui, or the anima mundi, or any such unphysical principle. And since such kind of explications have been of late generally called mechanical, in respect of their being grounded upon the laws of the mechanicks; I, that do not use to contend about names, suffer them quietly to be so: and to entitle my now examined explication to be mechanical, as far as I pretend, and in the usual sense of that expression, I am not obliged to treat of the cause of gravity in general; since many propositions of *Archimedes*, *Stevinus*, and those others that have written of staticks, are confessed to be mathematically or mechanically demonstrated, though those authors do not take upon them to assign the true cause of gravity, but take it for granted, as a thing universally acknowledged, that there is such a quality in the bodies they treat of. And if in each of the scales of an ordinary and just balance a pound weight, for instance, be put; he, that shall say, that the scales hang still in æquilibrium, because the equal weights counterpoise one another; and in case an ounce be put into one of the scales, and not into the opposite, he that shall say, that the loaded scale is depressed, because it is urged by a greater weight than the other; will be thought to have given a mechanical explication of the æquilibrium of the scales, and their losing it, though he cannot give a true cause, why either of those scales tends towards the center of the earth. Since then the assigning of the true cause of gravity is not required in the staticks themselves, though one of the principal and most known of the mechanical disciplines; why may not other propositions and accounts, that suppose gravity in the air, (nay prove it, though not *à priori*) be looked on as mechanical?

CHAP. II.

THE next thing the doctor opposes to my explication, is a resolute denial, that there is any such gravitation, as I pretend, of bodies, or their particles, in their proper places. But because, for the proof of his negation, he refers us to the next chapter, we shall hereafter have a fitter place than this to consider it in.

Thirdly, he tells us, we may justly doubt of the equal diffusion of the springy power, or the pressure of the air every way. In what sense, in some cases, I admit of a small inæquality between the pressure of fluids against differing parts of a surrounded body, I have * elsewhere declared, and need not here discourse of; since in the case before us, and in the like, that pressure is inconsiderable enough to be safely neglected. And whereas our author thus argues, *Semotâ vi elasticâ, particulæ tamen atmosphæræ deorsum tenderent. Est igitur depressio quædam deorsum præter vim elasticam ipsi superaddita; sursum non item, sed elastica sola, estque suppar ratio in pressionibus transversis & obliquis:* I presume, he did not sufficiently consider our hypothesis and the nature of the pressure of fluid bodies, that have weight: for water, to which no springiness is ascribed, as there is to air, but which acts by its weight and fluidity, is able, upon the score of those qualities, to buoy up great ships, that the ebbing tide often leaves upon the strand. Pag. 139.

And whereas the learned examiner proposes a fourth objection in these terms, *Quibus omnibus addas, difficile esse intellectu, si unius cylindri atmosphæræ pondus æqualis diametri cum embolo reflectione in fundum emboli derivetur, cur non quinque alii cylindri aeris, qui circumstant embolum, in ejus fundum eodem modo simul agere possunt, ita ut vis sursum impellens embolum sextuplo major sit, quàm hactenus ab hujus opinionis fautoribus existimata est. Quod si sit, tunc certè, siquo artificio fieri possit* Pag. 139.

ut

* See the Hydrostatical Paradoxes, especially Parad. 7.

ut unius ſolius cylindri actio in embolum admitteretur, reliquorum quinque excluſa, & pari tamen facilitate embolus aſcenderet, manifeſtum indicium eſſet, ne unum quidem cylindrum atmoſphæræ agere in fundum emboli, ſed totam hypotheſin ingenioſam tantummodo eſſe fictionem. I preſume, hydroſtaticians will think this might have been ſpared. For they will tell him, that there can no more of a fluid preſs directly upward againſt the cylindrical orifice of a body immerſed in that fluid, than a cylinder of that fluid of the ſame diameter with the orifice, the lateral preſſures bearing againſt the lateral parts of the cylinder. And therefore if you invert, for inſtance, a pipe open at both ends, and filled to a height with oil, with common water; the oil, that is kept up by the preſſure of the water upwards, will keep at the ſame height as to ſenſe, whether the veſſel, that contains the water, be broad or narrow, provided it be ſomewhat larger than the orifice of the pipe.

AND now, to invalidate yet further the precedent objections made by the doctor, I ſhall add, that it need not be thought incredible, that the atmoſphere by its weight, or the ſpring of the air compreſſed by that weight, ſhould be able to raiſe up four ſcore or an hundred pound, hanging at the ſucker; ſince I have * manifeſted two or three years ago, by a clear and cogent experiment, that a little air in a bladder will, by its mere ſpring, be able to heave up a weight of a hundred pound, and this without the help of any rarefaction by heat. By which experiment may be alſo confirmed, what I delivered a while ſince about the endeavour of the air, that is wont to be included in our braſs cylinder, by expanding it ſelf to thruſt away the ſucker (which, in regard of the ſtructure of the pump, it can do no otherwiſe than downwards,) with a depreſſing force, æquivalent to the preſſure upwards of the atmoſphere, againſt the external part of the ſame ſucker.

CHAP. III.

BUT I ſhall not inſiſt upon the foregoing objections, becauſe the learned doctor himſelf tells, that their attempts may ſeem to be but light ſkirmiſhes in compariſon of that, which follows. Whereunto I ſhall therefore apply my attentien.

THIS grand objection our learned adverſary takes from the already often-mentioned aſcent of the ſucker clogged with a hundred pound weight, and recommends by this intro-
Page 140. duction. *Etenim ex ipſis phænomeni viſceribus robuſtiſſimum jam contra omnem mechanicam illius ſolutionem argumentum eruo, & quod non ſolum contra vim aeris elaſticam ſuprà dicto modo explicatam militat, ſed etiam contra Carteſianum illum aeris conatum nixùmque, &c.* Which premiſed, the argument it ſelf is thus propoſed:
Page 140. *Eſt enim* (ſays he) *juxta hujus experimenti phænomenon, vis illa aeris elaſtica (nixúſque expanſorius) major multo, quàm quæ fieri poteſt a rerum natura, quámque quotidianis illis phænomenis congruit. Nam ſi nixus hic elaſticus tantam vim elaſticam haberet, ut plus centumpondo plumbum ſurſum poſſit propellere, omnes profectò rerum terreſtrium compages tantâ violentiâ comprimerentur, ut nullæ, niſi quæ admodum firmiter compactæ ſint, tantæ compreſſioni reſiſtere poſſent, quin refrigerentur, vel partium colliſione ita contererentur, ut brevi tempore perirent, &c.*

THOUGH this objection be ſpecious enough, yet it preſents me with no difficulty, that I was not well aware of; as I preſume you will eaſily perceive by what you will meet with in the following papers, eſpecially that, which conſiſts of experiments and conſiderations about the differing preſſures of ſolids, weights, and ambient fluids. The nature of which preſſure and its æquality (as far as in our controverſy it is needful to be ſuppoſed) will, I hope, ſatisfy you of the invalidity of the propoſed objections; eſpecially ſince the doctrine it impugns, namely the weight and preſſure of the atmoſphere, is not a bare hypotheſis, but a truth made out by divers experiments, by which even profeſſed oppoſers of it have publickly acknowledged themſelves to be convinced.

CHAP. IV.

IN the next paragraph (which is the eleventh) the learned doctor adds a further objection, wherein he ſuppoſes, that there is laid upon a wooden ſcale, of the ſame diameter with the above-mentioned ſucker, a lump of butter of the ſame largeneſs with the ſcale. Whence he argues, that if our hypotheſis take place, the butter muſt be preſſed againſt by two cylinders of air, the one preſſing it upwards, the other downwards, and the preſſure of them both amounting to two hundred pounds. But, ſays he, the butter is not preſſed at all, as appears by this, that no ſerous humour is ſqueezed out of it towards the edges, not ſo much as in thoſe parts, that lie parallel to the horizon, whence the concluſion ſeems eaſy to be deduced.

BUT in the twelfth paragraph, the doctor himſelf propoſes a ſolution, which he might eaſily foreſee I would employ to invalidate his argument; namely, that the air preſſing, as well againſt the ſides of the butter, as againſt the top and bottom, hinders the maſs from horizontally extending itſelf. And whereas, by way of reply to this ſubterfuge, as it is
called in the margent, he ſubjoines, *Cui re-* Page 142.
ſpondeo, quòd tamen hoc nihil prohibet, quo minùs in omnes partes horizontales exprimatur humor ſeroſus & lacteus, ſi revera eſſet ulla hujuſmodi preſſura elaſtica, qualis fingitur: the reply is eaſy, that the preſſure of the ambient air, which is a fluid more ſubtil than buttermilk, will as well hinder the ſtarting out of that liquor, as of the parts of the butter itſelf; as he will eaſily grant, that attentively conſiders the nature of the thing, and remembers how air keeps water from running out at the little holes of a gardener's watering-pot cloſed at the top. What the objector adds about

* See Continuat. of New Exper. Phyſico-Mechan. Exper. 48.

about the extrusion of what he calls a subtiler element (supposed to be harboured in the butter) by the pressure of the atmosphere, in case it had any, I think it would not be difficult to answer, if we considered, that a great and undeniable pressure, applied to water, does not sensibly condense it, or deprive it of its fluidity, because of the grossness and strength of its parts. But the argument being but transiently mentioned by the author, and grounded upon a Cartesian supposition, that I never employed, I leave it to those, that may think themselves concerned (which I am not) to make a solemn answer to.

AND whereas our learned examiner superadds, *Quod tametsi butyri massa in disci lignei speciem reducta, cujus margo centum vicibus area sit minor, interque duas laminas ligneas ejusdem formæ ac latitudinis posita, filis suspenderetur in aere tanquam in lance, ita ut pressura aeris elastica, quà ab infra, quà desuper, ducentis fere vicibus excessura sit pressionem in marginem butyri, butyrum tamen nihilo arctius comprimetur per vim aeris elasticam, nec aliter hic afficietur quàm antea:* he seems not to have sufficiently considered the laws of the hydrostaticks, according to which, supposing the pressure of the atmosphere that he rejects, the butter ought not to be deprived of its shape. For the pressure of the ambient air, being equal on all sides, if we suppose the superficies of the butter to be distinguished into a multitude of little equal portions, each of these, whether they be situated horizontally, or on the edges, can be pressed against but by an atmospherical pillar equal to its basis; and the horizontal portions, if I may so call them, cannot be thrust out of place, without there be at the same time squeezed out some of the lateral portions, which yet cannot be so displaced, because they also are, with equal force, pressed inwards by little aerial pillars, whose bases are contiguous to them, and bear against them. Which answer, though of itself sufficient, may be much confirmed by the instance, you will hereafter meet with, of a lump of butter, that kept its irregular shape, in spight of a great and manifest pressure of the water, that surrounded it.

Pag. 143.

AND this answer may suffice to disprove, what the doctor annexes in the beginning of the thirteenth paragraph, about the vast excess of pressure, which the air exercises upon the flat and horizontal surfaces of the above-mentioned lump of butter, in comparison of the pressure the marginal parts of its surface can be exposed to. What he adds, and illustrates with a scheme, about the hand's being assisted with the pressure of the air, it concerns not me to answer. But whereas among the places, where the elastical power of the air is understood not to reach, he reckons a pail full of water, with a lump of butter put in it; he supposes that, which our hydrostaticks will by no means allow, and which is disproved by several, both of our former experiments, and by those you will meet with in the following papers. By which it appears, that the pressure of the atmosphere is exercised, as indeed I do not see what should hinder it from being, even upon bodies, that are quite immersed under water; and by which, added to what has been hitherto discoursed in answer to the learned doctor's objections, you will easily judge, how deservedly he shuts up the arguments, we have been examining, with this conclusion. *Adeo ut extra omnem controversiam positum videatur, quòd nulla est ejusmodi vis elastica in aere, qualem è doctis nonnulli supponunt, multoque minus tam fortis, ut centum librarum pondus superet. Quod erat demonstrandum.* Pag. 143.

CHAP. V.

BUT this is not all the doctor urges against me in this chapter; for in the fourteenth paragraph he seconds his former argument by another, drawn from this experiment of mine, that having taken two round marbles, whose surfaces, that were to be contiguous, were as well ground very flat, as carefully polished; and having placed them one directly upon the other, they did in a horizontal posture so firmly cohere, without the help of any glue, or viscous body *, that the upper marble being pulled up, would take up the lower, though clogged with a weight of fourscore and odd pounds.

THIS experiment, when I many years ago first published it, I referred to the action of the atmosphere, which pressing equally and strongly against the surfaces of both the marbles, except where they were contiguous, the higher could not be drawn directly upwards from the lower (and consequently must be followed by it) by a less force, than that, which was equivalent to the weight of as great a cylinder of the atmosphere as leaned upon the upper marble.

THIS experiment thus explained, though it hath been judged a very favourable one to the hypothesis, on whose behalf I alledged it, does yet to the justly famous doctor seem a very considerable argument against it, though for this judgment of his he urges only this reason, that if the force, with which the air presses the lower marble against the upper, be able to sustain that marble, though clogged with the great weight above-mentioned, the same pressure of air would much more easily support a plate of wood brought to a true plain, and not loaded with any weight, if the wooden plate were substituted to the lower marble, and, instead of it, applied to the upper.

BUT since the experiment, as I proposed it, did upon trial succeed very well, it had not been amiss, if the learned examiner had considered it as it was really and successfully made, and shewed, why the pressure of the ambient air was not able to hinder the separation of the marbles: and his needless substitution of a wooden plate, instead of the lower marble, easily suggests a suspicion, that there may lie some fallacy, though not intended by him, in

* See the History of Fluidity and Firmness, p. 222. of the second edition.

the variation he proposes of the experiment. And he seems to have himself had thoughts of this kind, by taking notice, that it may be answered on our behalf, that a wooden plate cannot be so exactly applied to the upper marble, but that there will be a little air intercepted between it and the bottom of that stone. And though, having granted, that it may be so, he employs two pages to shew, that this intermediate air could not keep the pressure of the atmosphere from supporting the unclogged plate of wood, if it had been that pressure, which, when there was no such intermediate air, had sustained the lower marble with all the appendant weight; yet I confess, his proofs seem not to me to be answerable to the assurance he uses in speaking of them. His examples taken from gunpowder and wind you will easily judge not to be very proper, where we are not considering a force, that acts by a sudden and vanishing impetus, but a constant and equal pressure. And as to his other instance, which is taken from five men, that thrust against the sixth (standing with his back to a wall) who is but as strong as any one of them; I answer, that neither is this example near enough of-kin to our case. For each of these five men is supposed to have an equal power of thrusting, proper to himself, and independent from all, or any of the other four. And the sixth man is likewise supposed to resist but by his own single force, without having his power of re-acting increased by the force wherewith the others thrust against him. But in our case the thing is quite otherwise; for supposing, that some aerial particles be so placed, that a solid body hinders them to recoil or expand themselves, we are to consider, that as the contiguous corpuscles of air press against them, not by their own single weight or pressure, but as they transmit the action of all the other particles of the air, which by their weight or pressure thrust them on; so the aerial particles, contiguous to the solid body, resist not barely by that force, which they would have if they were not compressed, but by vertue of the springiness they acquire upon the score of the forcible inflection they sustain from the action of the corpuscles, that either mediately, or immediately, thrust against them; and consequently, in proportion to that external force, the elasticity of these compressed particles will be increased, as we see, that a bow, or other springy body, the more it is bent by an external force, the greater power it has to resist further compression. Upon which grounds it need to be no wonder, that a small portion of air, being almost included in a solid body, and having for some (though but very little) time been exposed to the outward air, should be capable of resisting the pressure of as much of the whole atmosphere, as can come to press against it. For, this pressure of the atmosphere being continual, if the springiness of the aerial particles were not now great enough to resist that pressure, they must necessarily have been beforehand inflected or compressed by it, till the endeavours of the one, and the other were reduced to an equipollency. Of which I shall give you an instance in so obvious a body as a bubble at the top of water. For, though there be but a little air included in a very thin and transparent film of water, yet this little air is so well able to resist the weight of all the atmosphere, that can come to bear against it, that all the pressure of it is not able to make the film shrink, or become wrinkled; which it would do, if the corpuscles of the internal air were not reduced to a springiness, which makes its power of resisting equal to the endeavour of the external atmosphere to compress it. And to let you see, that we may well conceive such a springiness of the air included in the bubbles, I have elsewhere related, how, by barely withdrawing the pressure of the ambient air from glass-bubbles, hermetically sealed with air in them, not compressed beyond its usual state, the spring of the internal air would make the bubbles fly in pieces: and this will happen to stronger glasses than bubbles, as you will find in one of the former experiments *. And if we would illustrate what we are debating of by an example, it should not be by considering, as the doctor does, the endeavour of five men against the sixth, that hath his back to the wall; but that of five bladders full of air, piled up, and resting upon a sixth. For in this case, whatever force or power of pressing we suppose in the incumbent bladders, they all bear jointly upon the lower, which continuing at a stand, must thereby be so compressed, as to be able to resist their joint endeavours; as it is manifest, because otherwise it would not continue in that state, but be farther compressed; which is against the supposition.

This notion about pressure and resistance I have the more particularly deduced, because I found many modern naturalists, and even hydrostaticians themselves, to be great strangers to it. For which reason, I shall add, that I have evinced it by purposely devised experiments in the continuation of the physico-mechanical experiments † about the air. Were it not for this, I should perhaps have spared myself the labour of setting down these thoughts, as not necessary to the solution of the doctor's objections. For he admits a layer, or (as he aptly speaks) an area of aerial particles to be interposed between the upper marble and the wooden plate; and therefore the flatness and stiffness of those two bodies must keep them from an immediate contact, as well at the edges, as by the help of the same area they do elsewhere; and consequently, that interposed air may communicate with the ambient air. From whence the laws of the hydrostaticks (which I have elsewhere shewn) will allow me to conclude, that the weight of the atmosphere endeavours to depress the upper surface of the wooden plate; and so what the examiner urges of the inconsiderable resistance, that the few aerial particles, interposed between the flat bodies, can make to the great pressure of the column of air, that

* See the Tract about the Pressure of the Air's Spring on Bodies under water.

† Exper. xxv. and elsewhere.

that thrusts the wooden against the marble plate, would not conclude, though our former answer could not have been made; since the resistance, made by the interposed aerial particles to the pressure upwards of the atmosphere, is not, in our present supposition, made by those particles alone, but by the weight of the lateral and superior part of the atmosphere, exercised by the intervention of these particles. Which being so, what the learned doctor adds, that the weight of the wooden plate itself is here of no consideration, must needs be a mistake. For the two equal atmospherical pressures, the one against the upper surface of the wooden plate, and the other against the lower, countervailing, and consequently frustrating the endeavour of each other, the gravity of the wood itself will suffice to make it fall, as well as if it were pressed against by neither of them. And from this discourse you will easily judge, whether the doctor had reason to say as he does, *Quam ab omni ratione igitur absonum est, ut superficies illa sive area aerearum particularum, quæ insinuant se laminam ligneam inter & marmor, solidam columnam hujusmodi particularum, vi elastica sursum enitentium, contra laminam ligneam obnitendo vincat, ipsamque laminam in terram deturbet.*

Pag. 146.

CHAP. VI.

WHAT he adds in the sixteenth number against those, that fancy the aerial particles to be endowed with perception, and to act with design *pro re nata*, does not all concern me; and what he adds in the next paragraph, wherewith he concludes his twelfth chapter, I shall altogether pass by, as far as it concerns the extravagant conceit he opposes. But because at the close of the paragraph, he makes an inference, which comprizes our opinion also; since he concludes, that the experiment by him alledged, *Certissimum est indicium, particulas aerias nec cum consilio nec sine consilio inferius marmor sustinere nec suffulcire:* it will not be amiss to shew, that our opinion is undeservedly included in the inference; which I shall do by briefly solving the phænomenon the doctor lays so much weight on. For if we conceive with him, that the two flat marbles formerly mentioned be suspended, and that to the lower of them, a flat wooden plate of the same shape and extent be applied; I see no cause to wonder, why the two marbles should stick together, and not the lower of them to the wooden plate. For, as I lately noted, there being an area, or bed of aerial particles interposed betwixt the marble and the wood, the weight of the atmosphere, exercised by the intervention of those aerial corpuscles, ought to be æquipollent to the pressure of the atmospherical cylinder, that bears against the lower surface of the plate; which consequently by its own weight must drop down: whereas there being no such layer of aerial particles interposed betwixt the two marbles, the pressure of the ambient atmosphere, which touches them every where, save where their polished surfaces are contiguous, must keep them strongly coherent.

Pag. 150.

I presume I need not mind you, that hitherto I have discoursed upon supposition, that the doctor experimentally knows, what he delivers concerning the non-adhesion of an exactly smooth wooden plate to a marble one; and upon his concession, that, because of the want of sufficient congruity between the surfaces of two bodies, there is a bed of aerial corpuscles interposed between them. But now, I think, it will not be unfit to take notice to you, that though to illustrate, on this occasion, a subject, that is generally so little understood, as the exercise of pression among fluid bodies, I have answered my learned adversary's objections, as if I had nothing more to say for my explication of the suspension of coherent marbles, than what I many years since delivered in the little tract by him cited; yet I have since abundantly confirmed that explication by the 50th of the experiments published in my continuation; which if the doctor had been pleased to read, perhaps he would have received the same satisfaction, that other learned men have done; since there I experimentally shew, that the undermost marble, without the accustomed clog, would, upon the bare withdrawing of the sustaining air, drop off from the upper. And whereas the two marbles in our vacuum would not cohere, as soon as the formerly excluded air was let in upon them, it did by its supervening pressure make them stick together very strongly.

The SECOND SECTION.

CHAP. I.

I PROCEED now to the second of those two chapters, that I am interested to consider, in which the learned examiner is pleased to attack three or four of my hydrostatical opinions and explications; in the defence whereof, I hope, I shall be the less put to exercise your patience, because the learned doctor himself is pleased to grant me almost as much as I need desire concerning the truth of the hypothesis, whereon my paradoxes and explications are founded. For whereas the main thing I supposed in my hydrostatical papers is, that in water, though stagnant, the superior parts do actually, though not always prevalently, gravitate upon the inferior, or (if you will) press upon them, even when they do not sensibly depress

press them; the doctor in divers places, allows this hypothesis to be consonant to the principles of the mechanical philosophy; and accordingly having shewed, that in a suspended tub of water the whole liquor gravitates upon the bottom of the tub; he subjoins, *Jam verò cum totà hæc aqua constet ex particulis aqueis non compactis vel concretis, sed solutis à se invicem, impossibile est, ut omnes fundum situlæ premant, nisi infima quæque ab omnibus superioribus prematur, quemadmodum clarè demonstravimus in secunda sectione hujus capitis; nempe, si nullæ causæ nisi purè mechanicæ (quales sunt motus localis, magnitudo, figura,* &c.) *in edendo hoc phænomeno se intermiscent.*

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AND elsewhere in the same chapter he speaks thus of the gravitation of liquors (towards the close of the second paragraph.) *Necesse utique est, ut partes singulæ gravitent, cum totius sit gravitatio, si non sit aliquid immateriale principium in rerum natura,* &c. And adds, at the beginning of the next number; *atque sanè huic externi motûs hypothesi, & gravitationis elementorum in propriis locis inde necessariò emergentis, apprimè consonum est primum illud experimentum, quod scriptor profert in paradoxis suis hydrostaticis.*

Page 152.

AND now, Sir, I presume you do not much wonder, if I think these concessions reach the main thing I pretend to. For though I do as freely and heartily, as the doctor himself, who, I dare say, does it very sincerely, admit, or rather assert an incorporeal being, that made and governs the world; yet all that I have endeavoured to do in the explication of what happens among inanimate bodies, is to shew, that, supposing the world to have been at first made, and to be continually preserved by God's divine power and wisdom; and supposing his general concourse to the maintenance of the laws he has established in it, the phænomena, I strive to explicate, may be solved mechanically, that is, by the mechanical affections of matter, without recourse to nature's abhorrence of a vacuum, to substantial forms, or to other incorporeal creatures. And therefore, if I have shewn, that the phænomena, I have endeavoured to account for, are explicable by the motion, bigness, gravity, shape, and other mechanical affections of the small parts of liquors, I have done what I pretended; which was not to prove, that no angel, or other immaterial creature, could interpose in these cases; for concerning such agents, all that I need say, is, that in the cases proposed we have no need to recur to them. And this being agreeable to the generally owned rule about hypotheses, that *entia non sunt multiplicanda absque necessitate,* has been by almost all the modern philosophers of different sects thought a sufficient reason to reject the agency of intelligences, after *Aristotle,* and so many learned men, both mathematicians and others, had for many ages believed them the movers of the celestial orbs.

CHAP. II.

BUT you will tell me, that the doctor's concessions will not avail me, since he urges against the gravitation of the elements in their proper places, which gravitation he would have to be suspended by his incorporeal principle, an experiment, which he says is most manifestly repugnant to our hypothesis. He conceives then, that in a tub or pail full of water, with a perfectly cylindrical cavity, whose diameter is of sixty two parts, there is violently kept at the bottom, by the help of a stick, a round plate of wood, whose diameter amounts but to sixty one of those parts; and that as soon as ever the stick is removed, the wooden plate will emerge to the top and float. *Quod,* says he, *prorsus impossibile esset, si omnes partes aquæ ab (F G) ad (H J) non solùm junctim fundum vasis, sed singulæ singulas in eadem serie subjectas actu premerent.* To which assertion he immediately subjoins this argument to prove it by; *Cum diameter laminæ ligneæ (H M) partes 61, habeat æquales, diameter vasis (H I) habeat 62, manifestum est, quod superficies fundi vasis ad superficiem laminæ se habet ut* 3844, *ad* 3721; *quorum differentia est* 123. *Itaque rotundum intervallum inter latera vasis & marginem laminæ ligneæ habet se ad aream laminæ, ut* 123 *ad* 3721, *hoc est, area laminæ ligneæ excedit aream dicti intervalli plusquam triginta vicibus. Ac proinde aqua incumbens ligneæ laminæ excedit magnitudine aquam incumbentem dicto intervallo inter marginem laminæ & latera vasis plus quàm triginta vicibus, pondúsque sive pressio hujus alterius pondus pressionémque vincit plusquam triginta vicibus. Adeò ut impossibile sit, ut aqua incumbens prædicto intervallo ita premat aquam ipsi subjectam, ut hujus vi sublevetur lamina, quam vis tricies major deprimit. Quod* (says he, by way of inference) *æque absonum atque absurdum phænomenon esset,* &c.

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How little this ratiocination agrees with the experiments I have formerly told you of, about the cases, wherein light bodies will be detained under water, or emerge to the top of it, you will easily perceive, if you compare the one with the other, which you may quickly do, if you please to compare the doctor's discourse with the following narratives of those trials *, to which alone I might therefore refer you. But yet in the mean time, you may, if you think fit, consider a little, whether the argument, whereon the doctor lays so much stress, be any more than a paralogism.

FIRST then, since according to his computation the area of the interval, between the sides of the vessel and the edges of the round boards, is 123 of such parts, whereof the area of the board amounts to 3721; it is evident, that there must be room enough for the water to pass between the sides of the vessel and the edges of the board, which is supposed on all hands to be of some wood lighter in specie than

* See the Tract of the Positive or Relative Levity of Bodies under water. Exp. I. &c.

than water, since else it would not emerge upon the withdrawing of the stick.

NEXT, this board or wooden plate is not here intimated, or supposed to be (and indeed in practice can scarce be) made exactly congruous to the bottom of the vessel, and consequently the water may get in between them; for which cause it is necessary to keep the wooden plate forcibly down with a stick, which else were needless. And consequently this interposed water will communicate with the laterally superior water in the vessel, which superior water may, according to the laws hydrostatical, by the intervention of the interposed, exercise its pressure upwards against the lower surface of the wooden plate.

THIRDLY, the doctor's scheme allows and assists us to conceive, (which we may do however,) an imaginary plane of water to be parallel to the bottom of the vessel, and to pass along the bottom of the board; so that, of the water, that lies between this plane and the bottom of the vessel, one part is covered by the wooden plate; and the other, between the edges of that and the sides of the tub, is covered with the incumbent water only.

CHAP. III.

THESE things being premised, I thus argue: it is manifested by hydrostaticians after *Archimedes*, that in water, those parts, that are most pressed, will thrust out of place those, that are less pressed: which both agrees with the common apprehensions of men, and might, if it were needful, be confirmed by experiments. It is also evident, that that part of the above-mentioned imaginary plane, that is covered by the wooden plate, must be pressed by a less weight than the other part of the same plane; because the wood being bulk for bulk lighter than water, the aggregate of the wood and water incumbent on the covered part of the same plane must be lighter in specie, than the water alone, that is incumbent on the uncovered part of the same plane; and consequently this uncovered part being more pressed, than the other part of the plane, the heavier must displace the lighter, which it cannot do but by thrusting up the board, as it does, when the external force that kept it down is removed. And, to add this upon the by, this greater pressure against the bottom than against the top of bodies immersed in water specifically heavier than they is a true reason of their emersion, as I have elsewhere shewn. So that there happens no more in this case, than what usually happens in the ascension of bodies in liquors specifically heavier than themselves, on the account of the newly mentioned difference of pressure. And it is with an express, or supposed, exception of such a difference, which in many other cases may be safely neglected, that, which I desire you to take notice of, in most places of this discourse I speak of the pressure of ambient fluids on immersed solids, as uniform or every way equal.

IT is true, that according to the doctor's supputation, if the solid cylinder, consisting of the wooden plate, and all the water directly incumbent on it, were put into an ordinary balance, it would there many times out-weigh the hollow cylinder of water alone, that leans upon the uncovered part of the imaginary plane. And that is it, that seems to have deceived the learned doctor. But there are divers hydrostatical cases, wherein the phænomenon depends not so much upon the absolute weight of the compared bodies, as upon their respective and their specifick gravity; on whose account it is, that a small pebble, for instance, that weighs not a quarter of an ounce, will readily sink to the bottom of the river, on whose surface a log of wood of a hundred pound in weight will float. It is a rule in hydrostaticks, that when two portions of water, or any other homogeneous liquor press against each other, the prevalency will go, not according to the absolute weight, but the perpendicular height of those portions. And accordingly we find, that if a slender pipe of glass, being filled with water, have its lower orifice unstoped at the bottom of a vessel of water, which contains much more of that liquor than the pipe; yet if this last named water were, for instance, two foot high, and that in the vessel but one, the water in the pipe will readily subside, till it come almost to a level with the external water, though it cannot do so without raising the whole mass of water that stagnated in the vessel.

AND now I shall subjoin an experiment, which, though at first it may seem slight, and was made in lesser glasses and quantities, than I would have employed, if I could have procured better accommodations, has the advantage of requiring no curious instruments, and yet I hope, will serve for an ocular proof of the fallaciousness of that reasoning the doctor is so strangely confident of.

WE took an open mouthed glass, such as some call jars, and ladies often use to keep sweet-meats in, which was three inches and a half, or better in diameter, and somewhat less in depth, and had the figure of its cavity cylindrical enough. Into this having put some water to cover the protuberance, wont to be at the bottom of such glasses, we took a convenient quantity of bees-wax, and having just melted it, we poured it cautiously into the glass, warmed before-hand to prevent its cracking, till it reached to a convenient height. This vessel, and the contained liquors we set aside to cool, in expectation, that when the heat, that had dilated the wax, was gone, it would shrink from the glass, and consequently leave a little interval every where between the concave superficies of the vessel, and convex of the hardened wax; which accordingly came to pass, and saved me the labour of getting the wax shaped for my purpose with tools; which might have been done, but not without trouble and less exactness. And now it was easy for me to try the experiment I designed; for pouring in warily some water between the glass and the wax, so that it filled all the interval, left between those two bodies, both at the bottom and the sides, the wax was

was made presently to float, being visibly lifted up from the bottom, and its upper part appearing a little above the level of the water, which was no more than I did, and had reason to expect, according to the true principles of hydrostaticks. For water being somewhat, though but little, heavier in specie, than wax, and that, which was poured into the bottom and stagnated there, being pressed by the collateral water, every way interposed between the concave part of the glass, and the convex of the wax (so that this collateral liquor answered what I lately called a hollow cylinder of water in the doctor's experiment) that part of the stagnant water, that was leaned upon by the wax, being less pressed than the other part of the same stagnant water was by the water incumbent on it; this latter must displace the former, which it could not do, but by raising up the wax, that leaned upon it. And yet this collateral water was so far from being heavier, than the wax its pressure impelled up, that both the collateral, and the stagnant water all together, being weighed in good scales, amounted to little above a quarter of the weight of the wax, which happened by reason of the narrowness of the vessel, which, if it had been wide enough, I doubt not but the experiment would have succeeded, though the wax had outweighed the collateral water ten times more, than in our experiment it did. But that the solid body exceeded almost four times the weight, not only of the collateral, but the stagnant liquor too, does sufficiently overthrow the doctor's ratiocination. Whose fallaciousness will yet further appear by two other improvements, among others, which I made of one experiment.

FOR, I. though we poured in more and more water, as long as the vessel would contain any, the cylinder of wax was but lifted higher and higher from the bottom of the glass, but did not appear raised more, than at the first, above the upper surface of the water; which argues, that it was not at all the quantity of the inferior water, which was continually increased, but the pressure of the collateral water, which continued still at the same height in reference to that wax, that caused the elevation of the body.

AND II. to manifest yet more clearly the doctor's mistake, I devised the following trial. We took a round plate of lead about the thickness of a shilling, and having made it stick fast to the bottom of the cylinder of wax, to make this body sink the more directly, we placed one after another, upon the upper part of the wax, divers grain weights (first wetted to keep them from floating) till we had put on enough to make the wax subside to the bottom: for the facilitating whereof we had pared off its edges; by this means, the glass having been at first almost filled with water, there swam about an inch or better of that liquor above the upper surface of the wax. And lastly, we took off by degrees the grain weights, that we had put on, till we saw the wax, notwithstanding the adhering lead, rise, by degrees, to the top of the water, above which some part of it was visibly extant.

FROM this experiment I thus argue: it is manifest, that, according to the doctor's supposition, here was incumbent upon the wax a cylinder of an inch in height, and of the same diameter or breadth with the round surface of the wax; whereas upon the removing part of the water, that lay at the bottom when the wax began to rise, there was incumbent no greater weight than that of the collateral water, and as much of the superior and stagnant, as was directly incumbent upon that collateral water (and would have deserved the same name, if we had supposed the convex surface of the wax to have been continued upwards as high as the glass reached.) But now, whereas, according to the doctor's ratiocination, this cylinder of water incumbent on the wax, being an inch deep, and a good deal above three inches broad, must press the wax with a greater weight by several times, than that, which the lateral and hollow cylinder of this stagnant water could have upon the rest of the collateral water; yet the height of this aggregate of collateral waters being the same with that of the wax and the water swimming upon it, the difference of the pressure was so small, that barely taking off a weight of four or five grains, the wax would, notwithstanding the pressure of the water incumbent on it, be impelled up and made to float; and by the like weight, put again upon it, it would be made to sink, and by another removal of such a weight, (for I purposely reiterated the trial more than once,) it would, though slowly, re-ascend. And these phænomena do so much depend upon a mechanical æquipollence of pressure, that even four grains would not have been necessary to make the wax rise or sink, if it had not been for some little accidental impediments, that are easily met with in such narrow glasses; for otherwise in a larger vessel we have made the same lump of wax readily enough sink or float, by the putting in or taking off a single grain or perhaps less.

BY this you may see, that for the regulation of hydrostatical things, nature has her balance too, as well as art; and that in the balance of nature the statical laws are nicely enough observed.

YOU may also take notice, upon the by, how little the weight of the cylinder of water upon a body immersed in stagnant water is considerable, whilst there is a pressure of collateral water to counterbalance it; since in this last trial, though the cylinder of incumbent water did continually increase or decrease in length, whilst the lump of wax was sinking or emerging; yet the same despicable weight of a grain or less, that was just able to depress it beneath the upper surface of the water, did by its pressure or removal procure its sinking to the very bottom, or rising again to the top, and on both occasions with an equal slowness, bating that little acceleration of motion, that ought to happen upon another account, and which therefore is to be observed in the wax, during its rising as well as during its sinking.

CHAP.

CHAP. IV.

SOME other phænomena I produced, by varying the hitherto mentioned experiment, which are very favourable to our notions about hydrostaticks. But, since they do not directly concern the present controversy, I shall in this place only annex a couple, the former whereof affords an easy confirmation of that paradox, which we lay as the ground of divers others, and the contrary whereof is maintained not only by doctor *More*, but by many other famous and learned men; namely, that in stagnant water the upper parts do actually press the lower.

WE took then a very slender pipe of glass, whose cavity was narrower than that of an ordinary goose-quill, that heterogeneous liquors may not be able to get by one another in it. This pipe, near one end, was bent upwards like a syphon, that it might have a short leg, as parallel as the artificer could make it to the longer. Into this crooked pipe we put a little oil, and then held it perpendicularly in a somewhat deep and wide-mouthed glass, filled partly with water and partly with a lump of wax, of the bigness and shape of that already mentioned; that so the pressure of the incumbent water, upon the open orifice of the shorter leg, might impel the oil into the longer leg, somewhat above the surface of the water in the vessel; which it was convenient should be done, that we might the better see the motions of the oil, and which we knew must be done by the course we took; both because oil is lighter in specie than water, and consequently required not an equal height of water to counterbalance it; and because, in very slender pipes, water is wont to ascend a little above the level of the external water, whereinto they are immersed. The pipe being, as was said, held upright, it was easy to take notice by a mark, fixed on the outside, to what height the oil reached in it.

Now if we conceive a horizontal plane, parallel to the bottom of the vessel, to pass by the basis of the floating wax, it is evident by what has been formerly shewn, that, of this imaginary plane, that part, on which the wax is incumbent, is as strongly pressed by the weight of the wax, as the lateral part of the same plane is by the weight of the water incumbent on it; (otherwise these pressures would not be æquipollent, but the wax would be raised:) and consequently, that part of this plane, that is placed directly over the orifice of the shorter leg of the pipe, is no more pressed, than any equal portion of that part of the same plane, that is covered by the wax. This body being taken out of the water, the liquor subsided a great way in the vessel, and so did proportionably the oil in the longer leg of the pipe. And lastly, having weighed out in a good pair of scales as much water, as we found the wax to amount to, this liquor was, instead of the wax, poured into that which remained in the glass; whereupon the oil, in the longer leg of the pipe, was again impelled up very near to the former mark, to which it had been raised by the wax. Whence we may gather, that the water newly put in, though in the air it weighed no more than the wax, yet it did as much press the water, that lay beneath the forementioned imaginary plane, and consequently, that which was directly over the shorter leg of the pipe, as the wax, that had been taken out, had done. And since we have already proved, that the wax did considerably press that plane, it ought not to be denied, that the water also (which instead of it was able to impel upon the oil in the pipe) did in like manner press that plane; and consequently, that water may be gravitated in water, as well as a solid body, such as wax is, can. And this is the first additional use, I told you, I would make of our experiment.

BUT, to come now to the second, there is another phænomenon of it, viz. the abovementioned tenderness of nature's balance, whose use seems to be of no less general concernment to the true doctrine of the hydrostaticks. For, by duly considering that phænomenon, and reasoning a while upon it, we may be helped to rectify that plausible mistake, which has long deluded both philosophers and mathematicians, and does yet impose on most of them; namely, that a body does not actually gravitate when it does not descend. For we have seen already, and shall further shew by and by, that the sunken wax, and the brass grains, that lie on it, do actually press or gravitate upon the subjacent water and bottom of the vessel on which it is incumbent; and consequently its pressure being not surmounted by that of the collateral water, which is unable to raise it, must be as great, as that of this collateral water. Therefore, when upon the removal of a single grain, the wax, with its incumbent weight, is made to ascend, and that but very slowly, it is evident, that it was so far from not gravitating before, because it did not actually descend, that it retained its gravity even whilst it ascends: as may appear not only by the slowness of its motion upwards, proceeding from its being in nature's balance very little less heavy, than it need be, to countervail the pressure of the collateral water; but by this also, that if but a single grain be laid on it, when it begins to rise, its ascension will be checked and hindered, which could not be done by the addition of so inconsiderable a weight, if the wax and the adhering metal did not, even during their ascent, retain their former gravity, though that were frustrated as to the act of descending, or so much as keeping their station by the prevailing pressure of the collateral water: so that, since, as we found, the wax and adhering metal amounted to a good deal above four-thousand grains, it did in the balance of nature weigh, whilst it was ascending, not so much as a four-thousandth part less than it did, whilst it was actually descending.

CHAP. V.

I Should beg your pardon, Sir, for having detained you so long with my reply to a single objection of the doctor's, how pompously

pously soever proposed; but that I thought it not amiss to do some service to the true theory of hydrostaticks, by taking this occasion to present you some things, that I thought not unlikely to illustrate some parts of that theory, though above what was necessary to answer he doctor's argument; to which, I confess, I was troubled to see so learned a man subjoin the following conclusion: *Hæc tam luculenta demonstratio contra gravitationem particularum aquæ inter se quamvis junctæ situlæ fundum urgeant, si non sit vera atque solida, equidem nec mei ipsius nec ullius unquam mortalis in posterum ratiociniis credem.* But I hope he will not be as bad as his word, but will be pleased to consider, as well as I do for him, that a man may be very happy in other parts of learning, and of greater moment, that has had the misfortune to mistake in hydrostaticks, a discipline, which very few scholars have been at all versed in, and about which divers of those few have had the misfortune to err, not only in the conclusions they have drawn, but in the very principles they have embraced.

To the foregoing argument the doctor, though he declares he thinks it needless, adds in the fifth paragraph another, taken from the last experiment of my hydrostatical paradoxes, by which he ingenuously acknowledges, that I seem at first sight to have demonstrated what I pretend to, about the gravitation of the upper parts of stagnant water upon the lower. And I am sorry that I cannot in return acknowledge, that his objection, at first sight, seemed to me a cogent one, for neither at the second nor third perusal can I clearly discern where his ratiocination lies, supposing it to be meant for an answer to my experiment. And though I consulted with some learned members of the Royal Society, whereof two are mathematicians, and one his particular friend; yet they all confessed he had not sufficiently explained himself on this occasion, nor could they shew me to what argumentation I might properly direct my reply. Only one of the doctor's correspondents, having seriously perused his discourse, and the annexed scheme, told me, that what seemed the most probable to him, was, that though the doctor was too civil to give me, *in terminis*, the lye; yet he did indeed deny the matter of fact to be true. Which I cannot easily think, the experiment having been tried both before our whole Society, and very critically, by its royal founder, his majesty himself. But since you have your self seen, and made it more than once, I need not spend words to convince you, that the matter of fact is true.

But after I had in vain sought the doctor's meaning where I expected it, chancing lately to cast my eyes on another place, where I saw my scheme repeated, I find this passage in the explication he endeavours to give of the phænomenon by his hylarchical principle: *Cum verò tam profundè immergitur tubus, ut obturaculum tangat superficiem V W, vis retractionis aeris ita augetur, ut etiam ponderis appensi superadditam depressionem superet. Videtur igitur quasi quædam sursum-suctio aeris in tubo contenti, & conformis ac contemporanea aquæ compulsio in obturaculum, quo tam firmiter in os valvulæ comprimitur, ibique cum appenso pondere sustentatur.* What considerable interest the supposed, but unproved, retraction of the valve, or the air itself, can have in this phænomenon, I confess I do not discern; not being able to see, but that the experiment would succeed, when tried *in vacuo*, although all the atmospherical air were annihilated. But if I mistake the doctor's meaning, I am to be excused, since I do it not willingly, and his own obscurity has been accessary to it. Nor am I very apprehensive of being unable to defend my account of an experiment, which (as you know) has had the good fortune to recomend the doctrine, for the proof whereof I devised it to many learned and curious persons, several of which were sufficiently indisposed to admit it.

And to avoid all mistakes and disputes, that may arise (which I think they must do needlesly) upon the score of the valve employed in our experiment, I shall remind you of another, that I remember I have sometimes shewn you, and divers other virtuosi, though I remember not whether I have mentioned it in any of my published writings. The sum of this trial is, that an arbitrary quantity of quicksilver being, by suction, raised into a very slender glass-pipe, whose upper orifice is stopped with the experimenter's finger, to keep the mercury from falling before its time, the open end of the pipe with the mercury in it is thrust into a competently deep glass of water till the little cylinder of mercury have, beneath the surface of the water, attained to a depth, that is at least fourteen times at great, as the mercurial cylinder has of height. For then, the finger being removed from the upper orifice, the glass-pipe will be open at both ends, and there will be nothing to hinder the quick-silver's falling down to the bottom, but the resistance of the cylinder of water, that is under it, which cylinder can resist but by virtue of the weight or pressure of the stagnant water, that is superior to it, though but collaterally placed above it: and yet this water being by the pipe, whose upper part is higher than its surface, and accessible only to the air, kept from pressing against the mercury any where but at the bottom of the pipe, and being about a fourteenth part of the weight of an equal bulk of mercury, it is able at that depth to make the subjacent water press upwards against the mercury, which is but a fourteenth part as high as the water is deep, with a force equivalent to that of the gravity wherewith the mercury tends downwards. And to manifest, that this phænomenon depends meerly upon the æquilibrium of the two liquors; if you gently raise the lower end of the pipe towards the surface of the water, this liquor, being not then able to exercise such a pressure, as it could at a further and greater depth, the mercury preponderating, will, in part, more or less, as the pipe is more or less raised, fall out to the bottom of the glass. But if, when the quicksilver is at the first depth, instead of raising the pipe, you thrust it down farther under the water, the pressure of that liquor against the mercury increasing with its depth will not only

only sustain the mercury, but impell it up in the pipe to a considerable distance from the lower orifice of it, and keep it near about the same distance from the surface of the laterally superior water. And this experiment may not only serve for the purpose, for which I here alledge it; but also, if duly considered and applied, may very much both illustrate and confirm the explication formerly given of the seemingly spontaneous ascent of the clogged sucker in our exhausted air-pump.

THE last argument the doctor urges against the gravitation of water in what they call its proper place, is deduced from what happens to the divers, who in the middle of the sea, though the salt water of that be much heavier, than that of fresh water rivers, do not find themselves oppressed, or so much as feel themselves harmed or compressed by the vast load of the incumbent water.

BUT that the equality of the pressures of an ambient fluid will go a great way towards the solving of this difficulty, you will find by the experiments and considerations you will meet with in the following * papers, to which, for that reason, I refer you. And though the doctor in this same paragraph objects, *tametsi hæc pressio æqualis sit, nihil tamen impedit, quò minùs subtiliores partes corporis magisque fluidas exprimat & elidat:* I remember I answered that exception before, by saying, that those liquors, that he supposes should be squeezed out, cannot be so, because there is as great a pressure against those parts, at which they should issue, as against any of the rest, if the parts that should be squeezed out, be not too spirituous and subtile, which if they be, I should gladly learn how the doctor knows, that no such minute and spirituous particles are really expelled: especially if that be observed, which we shall soon have occasion to relate, that a small animal, being vehemently compressed in water, seemed a little, though but a little, to shrink.

BUT that we may the more distinctly consider this grand argument, taken from the experience of the divers, that is wont to be employed by the schools, and others, for the vulgar opinion, and is now urged by the learned doctor to prove his; it will be convenient to observe, that it does, at once, both propose a question, and contain an objection, grounded upon the surmised insolubleness of that question.

AND to begin with the problem, "whence "it is, that divers are so far from being killed "or oppressed by the weight of the incum-"bent water, that they are not so much as "hurt by it, nay, that they scarce feel it at "all?" We may take notice, that there is in it somewhat supposed, as well as somewhat demanded. For, in the question, it is taken for granted, that divers, though at never so great a depth, feel no pressure exercised against them by the water; which is an affirmation in point of fact, of whose truth I make some question, for the reasons I shall ere long have occasion to mention.

But it will clear the way for what is to follow, if I here divide the noble and difficult problem, we are to consider, into two questions; the first, why a diver should not be oppressed and crushed to death by the pressure of the incumbent and ambient water. And the second, why at least he should not be made sensibly to feel it, by suffering some considerable inconvenience from it.

IN answer to the first of these questions, you will easily perceive, that divers things may be pertinently applied, that you will meet with in the following paper, to shew the difference betwixt the pressure of fluid, and that of solid bodies. And that *de facto* the pressure of water may be exceeding great, without destroying an animal quite surrounded with that liquor, I have long since shewn in another † treatise, by the experiment of a little tadpole, which being, together with the water it swam in, included in a bent glass sealed at one end, the animal was not killed or sensibly hurt, but only (according to what was lately noted by anticipation) seemed to shrink into somewhat, and but little, lesser dimensions.

IF it be here alledged, that this experiment makes rather against me than for me, the learned doctor having made use of it with a scheme to explain it in his sixteenth paragraph; it will be fit for me to consider his objection. Having then recited the matter of fact newly delivered, he adds, *Quod certè fieri non posset nisi juxta legem quartam contrusio particularum aquæ contra se invicem principio hylarchico inhiberetur & eluderetur. Atque hinc fit, ut quamvis aqua in tubo (A B C) vi trudis (G F) aliquantò facta sit condensatior, partes tamen sic compressæ, ut propiùs ad se invicem accedant, nihilo inde inter se fiunt comprimentiores.* And then subjoining the following passage; *Neque enim sequitur ex earum contactu, quod premant se invicem, quandoquidem particulæ, uti fit in duris corporibus, in unum coalescere possunt, & tamen non mutuò se premere*; (wherein are some things that might be questioned, if it were necessary;) he thus pursues his discourse: *Cùm verò hîc particulæ aquæ, si omninò premerent se invicem, pressura in gyrinum, columnæ aqueæ, ducentos vel trecentos pedes, æneæ verò, plus viginti vel triginta pedes altæ, pressionem adæquaret, luculentum est indicium, quod reverà particulæ se invicem non premant. Nam planè est incredibile, columnam æneam pro corpore quidem gyrini latam, sed altam viginti vel triginta pedes & amplius, gyrincque ad perpendiculum incumbentem, omnia viscera tam tenellæ gelatinæ non esse elisuram.* Notwithstanding which allegation I am apt to think, you will judge the argument, from this experiment, to be more probable on my side than on the doctor's. For there being in our case an animal, exceedingly much more tender than a man, exposed to a pressure, which he affirms is so great, that if it were exercised on the tadpole, it ought to squeeze out all his guts, I think, I may pretend to have given a pertinent instance, that a diver may be at a considerable depth under water preserved from being crushed to death by the

* The Author means the new experiments of the differing pressure of heavy solids and fluids.

† The Author points at the Appendix to the Hydrostatical Paradoxes.

weight of it. And whereas the doctor tells us, that the cause of the incolumity of the tadpole is, that the pressure or contrusion of the particles of the water against one another is hindered or frustrated by the *principium hylarchicum*, I reply; that what I affirm is matter of fact, and evident, (namely, that there was a great external force duly, and yet ineffectually applied to press to death, by means of the water, the animal swimming in it;) but that this mechanical force was suspended, or made ineffectual, by some invisible and immaterial agent, is but the doctor's hypothesis, and a thing, which, whether it be true or no, is at least not manifest.

HAVING said thus much about the first question; I now proceed to the second, "Why "divers, though at never so great a depth, "complain not of the pressure of the water, nor "suffer any harm nor inconvenience by it?"

AND here, Sir, the question highly meriting a particular curiosity, I shall not scruple in the more full enquiry I am now entering upon, as well sometimes to employ and inlarge particulars already mentioned in the last of the following papers, as oftentimes to strengthen them with new ones. And I shall also, for a while, suspend my difference with the doctor, and addressing myself to you, who, I am sure, will allow me, that water weighs in water, propose, according to my custom, not as a dogmatist, but as an enquirer, some particulars, that may tend to the solution of a problem, which I take to be as difficult as noble. Not that I doubt, but it must and will be explicated upon the mechanical principles; but partly, because the application of them to the solution, will not offer itself to every seeker; and partly, because we are not yet well furnished, either with experiments made on bodies under water, or so much as with so competent an account of the matter of fact, as I think may keep wary men from hesitations about it. For what is commonly reported concerning the divers, is (as has above been intimated) grounded but upon their own relations and answers, perhaps amplified or procured by leading questions from persons, who are generally either slaves or ignorant men, taken from the less sober part of the illiterate vulgar, and prepossessed with the common opinion of the non-gravitation of water in its own place; and consequently are not like to make over-accurate observations, but prone to refer the inconvenient alterations, they feel, to any other cause, than the pressure of the water, which they are taught to be none at all. If observations about diving were made by philosophers and mathematicians, or, at least, intelligent men, who would mind more the bringing up out of the sea instructive observations, than shipwracked goods, we should perhaps have an account of what happens to men under water, differing enough from the common reports.

YOU will in one of the following papers find mention of a learned physician of my acquaintance, that, upon his diving leisurely, perceived a constriction to be made of his thorax by the action of the surrounding sea-water.

A Spanish prelate, that lived long in *America*, speaking of the deplorable condition of those wretched Indians, that were employed by their inhuman masters about the fishing for pearls, gives us this account of them: "It Purch.
"is impossible, that men should be able to live Tom. IV.
"any long season under the water, without Lib. 8. p.
"taking breath, the continual cold piercing 1587.
"them; and so they die commonly parbreaking of blood at the mouth, and of the "bloody-flux caused by the stomach. Their "hair, which are by nature cole-black, alter "and become afterwards a branded russet, like "to the hairs of sea-wolves, &c."

AND a general of the English in the *East-Indies*, being by them employed on an embassy to the emperor of *Japan*, has this passage concerning some female divers, that he met with in his voyage: "All along this Purch.
"coast, and so up to *Ozaca*, we found women Tom. I.
"divers, that lived with their houshold and Lib. 4. C.
"family in boats upon the water, as in *Hol-* 1.
land they do the like. These women would "catch fish by diving, which by net and line "they missed, and that in eight fathom depth. "Their eyes, by continually diving, grew as "red as blood, whereby you may know a "diving woman from all other women." I know it may be said, that these diseases may proceed from the coldness and moisture, or other qualities of the sea; nor would I confidently reject such a surmise: but it may also be possible, that the compression, they suffered under water, might have, at least, a share in the production of these ill effects. For how are we yet certain, that the pressure of the water against their bodies, though it does not manifestly dislocate any solid or firm part, but only somewhat press inwards, as in the above-mentioned tadpole the outward skin and the fibres, (both which will easily yield a little way, without being painfully stretched,) may not, by straitening the vessels, and otherwise inconveniently alter the circulation of the blood and the motion of the humours, spirits, and other fluid parts of the body? And I am not sure, that much of the cold, that divers are wont to complain of, when under water, may not be a disaffection produced in the nervous and membranous parts, occasioned by the compression of the ambient water, there being divers things, and pressure among others, besides actual cold, that will make men complain of being cold; and in our case, this sensation may be excited, or assisted, by the hindering of the usual perspiration at the constipated pores of the skin. And it seems not impossible, that one, not so ignorant and heedless as divers are wont to be, may refer a new sensation, that really proceeds from pressure, to other causes; since learned and intelligent men, when prepossessed (as these common divers usually are) with the vulgar opinion about the non-graviation of water and air in their natural places, do almost always refer * an experiment of

* The reason of which experiment may be gathered from the fourth Chapter of the Author's long since published Defence against *Linus*.

of my engine to suction, which is indeed the effect of the pressure of the ambient, (as I have * elsewhere clearly shewn,) and affirm, that the pulp of the finger, or hand, is drawn up into a hollow pipe, into which it is indeed thrust by the weight of the ambient air. But all these things I have mentioned, not as if I laid any great weight upon each of them, but to let you see, that it was not altogether without cause, that I complained of the incompetency of the history of what divers feel under water; especially at great depths, where this want of information may be more considerable: for, as far as I have yet learned by perusing voyages, and enquiring of travellers of my acquaintance, the places, where they are wont to dive for pearl, are but moderately deep, and indeed shallow, in comparison of the great depths of the sea; so that if we were furnished with as many relations of these profound places, as we have of the others, possibly the accounts would be different enough to render doubtful, or correct the received opinions about the conditions of divers at the bottom of the sea. For, I remember, that a credible eye-witness, who, if I mistake not, was the intelligent *Oviedo*, speaking of the pearl-fishing on the American island of *Cubagua*, has, among many other notable observations, such a passage as this: "But whereas the "place is very deep, a man cannot naturally "rest at the bottom, by reason of the abundance of aery substance, which is in him, "as I have oftentimes proved. For although "he may by violence and force descend to the "bottom, yet are his feet lifted up again, so "that he can continue no time there. And "therefore where sea the is very deep, these Indian fishers use to tie two great stones about "them with a cord, on each side one, by the "weight whereof they descend to the bottom, "and remain there, until them listeth to rise "again, at which time they unloose the stones, "and rise up at their pleasure."

And now to come closer to the explication of our difficult problem; there yet occurs to me nothing more likely in order to it, than what I have already mentioned in the paper you will meet with about the differing pressures, &c. And therefore it shall here suffice me to enlarge, and by further considerations and experiments confirm, what is there more summarily discoursed; namely, that the phænomenon may depend, chiefly, upon these two things, the uniform pressure of the fluid ambient, and the robust texture of a human body exposed to this pressure.

In one of the following † papers, you will find examples of the great pressure, that may be sustained unharmed by such frail bodies as eggs and thin glasses, that one would expect should be broken in pieces thereby, provided the pressure be exercised by the intervention of an ambient liquor; as water. And by the account, elsewhere referred to, of the tadpole, it seems highly probable, that even that tender animal, when it seemed by some small diminution of the bulk to be every way a little compressed inwards, was put to no considerable, or perhaps to any sensible, pain or inconvenience, since it seemed to swim without any irregular motions, which would in likelihood have ensued, if it had been much harmed or incommodated. Which example, with those formerly pointed at, may teach us, that there may be a vast difference betwixt the resistance, that a body can make, when compressed immediately by solid bodies, and when in the compression every way ambient fluids intervene. Which you will the less admire, if you consider, that by reason of the grossness, hardness, or rigidness of visible solid bodies, the pressure can never be made every where so equally, as by the parts of liquors, whose smallness, which renders them singly invisible, fits them to accommodate themselves far more closely and conveniently to all the superficial parts of the body immersed in them, and to have the force of the compressing body more uniformly distributed to them. But because the instances referred to are taken from bodies surrounded with water, I will take two or three about the resistance of bodies to violently compressed air; partly, because those made in our engine are wont to be performed with air, not condensed, but rarified or expanded beyond its usual consistence; and partly, because it will not be denied, that the corpuscles of air may be really compressed or thrust against one another, since it is clear, that they may be crouded into far less room, than they possessed before, and bear so strongly against the glasses, that imprison them, as not seldom, if too much compressed, to burst them in pieces.

Consider then, that among bodies not fluid, the swims of smaller fishes are likely to be judged none of the most able to resist compression, since they consist of bladders so thin and delicate, that a piece of fine Venice paper is very thick in comparison, and that they contain nothing in them but soft air, not compressed by any outward force. I caused one of these bladders, of above an inch in length, and proportionably great, to be taken out of a roach, and anointed it with oil to keep it supple, and preserve it from being pierced or softened by the water; and having by a weight of lead, fastened to the neck of it, let it down to the bottom of a hollow cylindrical tube, sealed at one end, and made purposely large, and about 56 inches long, for some hydrostatical experiments; we could not perceive, that by the weight of all the incumbent water it was manifestly compressed, or that it did discover the least wrinkle or other depression of the very thin membrane, though stuffed but with air. And this trial was made more than once with the same succes; and yet, that this proceeded rather from the robustness of the bladder, that was able to resist the weight of a taller pillar of water, than from the non-gravitation of water in the upper part of the tube on that in the lower, we shewed, by presently letting down such a mercurial-gage, as is described, and often mentioned in the Continuation of our New Experiments.

For

* In a paradox about suction.

† New experiments about the differing pressure of heavy solids and fluids.

For letting down this by a string to the bottom of a tube, the weight of the incumbent water forced up some of the mercury out of the open leg of the syphon into the sealed one, and consequently compressed the air included there, which though it were not very much, yet it was very manifest. For the uncompressed air being three inches and ⅝ in length, we judged it at the bottom of the tube about ⅝ by the intrusion of the mercury, that was impelled up; and to satisfy myself, and others, that if the incumbent water had been heavy enough, it would have visibly depressed the bladder in spite of any *principium hylarchicum*, since I could not have a tube long enough, the bladder was sunk into a chrystal glass, that had a long and cylindrical neck, and was so well stuffed with a stopple, that was cylindrical too, that it was very difficult for any thing to get out betwixt it and the orifice of the glass; then a competent quantity of air being left above the water, the stopple was warily, and by degrees thrust down, and so lessening the capacity of the glass, compressed the air, that was next it, and by the intervention of that, the water, that was under it. And though there did not, upon a slight compression of the outward air, appear any sensible operation upon the bladder, that was at the bottom of the water; yet, upon a farther intrusion of the stopple, the pressure being increased, the immersed bladder discovered not only one, but two considerably deep wrinkles, which presently disappeared upon the drawing up of the stopple. Upon whose being thrust in again, depressions were again to be seen on the swim. And we having been careful to convey into the same glass such a mercurial gage as has been lately spoken of, we estimated, by the condensation of the air in the sealed leg of that gage, that the bladder had been exposed to a pressure, that might be equivalent to that of a pillar of about forty foot of water.

THIS I hope will lessen the wonder, that bodies of so firm a texture as those of lusty men, should support the pressure of the water at such depths, as divers are wont to stay at; since we see, what resistance can be made by so exceeding thin and delicate a membrane stuffed only with air, in comparison of the strong membranes and fibres of a man, stuffed besides air, with more firm parts. I will not here urge, that great weights may be sustained in the air by such tendons, or cords of fibres, and by other fibres, as it were, interwoven into membranes, in comparison of what an ordinary man would expect: but I shall invite you to consider with me, that not only upon the account of the stable parts of the human body, but of the spirits too, it may resist very violent pressures (and such as perhaps have not yet been considered) of a fluid body, not only without any manifest contusion or dislocation of parts, but without any sense of pain; which I suppose you will grant me, if, considering what great effects gusts of wind have upon doors, trees, nay masts of ships, blowing them down, nay breaking them; and that yet a man, without being extraordinary strong, will stand against the impetuosity of such a strong wind, and walk directly against it, by vertue of the vigour of his muscles and spirits, without being thrown down, or bruised by so violent a current of air, as beats upon him, but without so much as complaining, that he feels any pain; and this, though the wind that beats against him, however it be a fluid body, yet because it acts as a stream, does not uniformly compress him, but invade only the fore-part of his body. Likewise, in the lifting up heavy weights by porters, carriers, and other lusty men, we may see the slender tendons of the hands loaded with a hundred, or a hundred and fifty, or perhaps a far greater number of pounds, without having their fibres so far compressed, or stretched, as to make the lifters complain of pain, though sometimes they may of difficulty. So that, (as I could, if it were needful, confirm by other instances) a human body is an engine of a much firmer structure than scholars are wont to take notice of. And here let me add, that I doubt whether, if the structure of a man were not considerably (though not perhaps equally) firm, he would, especially in a deep sea, be able to bear the pressure of the water, though not immediately applied, without pain. For (to give you one reason more of my not acquiescing in vulgar reports about diving) having several times conversed with a man, apt enough both to enquire and observe, who got his living by taking up ship-wrecked goods, he answered me, when I asked him, whether he felt any peculiar pressure against the drums of his ears, which are membranes not so well backed as those of other parts; that when he staid at a considerable depth, as ten or twelve fathoms, under the surface of the sea, he felt a great pain in both his ears, which often put him to shifts to lessen it; which, by his manner of describing it, I concluded was from the incompetent resistance of the air, which he acknowledged to me, he found by manifest tokens to be notably compressed by the superior water. Which relation from such a person does not only confirm our explication, but likewise warrant us to doubt, whether the common reports, that are made concerning divers, be fit to be relied on, without farther examen and observation.

IN the mean time, I shall add two or three experiments more, to confirm the resistance, that animals may make to a great pressure, when exercised by the mediation of a fluid body. And I the rather gave you an account of this way of making trials, because it may be also helpful to discover the resistances of inanimate bodies, whose shape and consistence we may choose and vary, almost at pleasure, to the pressure of (totally, or in great part) ambient fluids. And if I had been furnished with a tube wide enough, and a quantity of mercury great enough, I might, by the way, have shewn you, that whatever the learned Dr. *More* is pleased to suppose, that to butter itself, even as considerable a pressure may be so applied, as not to be able to make it yield thereunto. For on this occasion I shall add, that I well remember, that, among other trials to the same purpose,

purpose, I caused a piece of fresh butter, about the bigness of a small hen-egg, to be brought to an irregular shape, that if the compression were such, as many would expect, the long corners, or solid angles, being at least flatted, the butter might be reduced into a more capacious figure, and less remote from roundness. But, though having put this lump of butter into a bladder, almost full of fair water, we proceeded, both in the same brass cylinder, and much after the same manner, that I employed about the egg mentioned in the fourth experiment of the Tract of the differing pressure of heavy solids and fluids; yet I found, that after the plug had been loaded with a weight of lead of above fifty pound, neither I, nor the operator, perceived the irregular figure of the butter to be altered. Nor was this the only trial of this kind I made with the like success upon butter, though I dare not charge my memory with the circumstances; and therefore I shall, without delay, proceed to what I was about to recite concerning the resistance of animals.

We took then a common flesh-fly, neither of the biggest sort of all, nor of the least, but of a middle size; and having put it into the shorter leg of a bent glass, which we caused to be hermetically sealed at the end, there was put in as much mercury as filled that leg, and a part of the other, leaving little more than an inch of air between the quick-silver and the sealed end, that there might be room both for the fly, and the condensation of the air, and then with a little rammer, fitted for the purpose, we caused the mercury in the open leg to be thrust against that in the sealed leg, which thereupon did necessarily croud the air near the fly into less room; so that, by our guess, it was condensed into about a third part of the space, which it possessed before, and which it regained when the rammer was withdrawn: and though this were done more than once, yet not only the fly was thereby not killed, but not so much, that appeared, as sensibly hurt; and I perceived her, whilst she was pent up, to move her legs, and to rub them one against the other, as it is usual with that sort of insects to do of their own accord in the free air. Nor did I question, but that, if the glass had not been inconveniently shaped to admit the rammer farther into it, the fly would have supported a far greater pressure.

Another experiment, to the same purpose, we tried with water instead of mercury: but, whereas this last named liquor could neither wet nor drown our fly, (for which reason I chiefly made choice of it) the other did first wet its wings, and soon after, by a mischance, drown it. But first we had an opportunity to compress the air into a third, if not into a fourth part of its former dimensions; and yet the fly continued to move divers of her parts, and especially her legs, very vigorously, as if nothing troubled her, but her being, as it were, glued to the inside of the glass by part of her wetted wings. And this, I hope, will keep the resistance of divers to the ambient water from seeming incredible; since such flies were able to resist, and, for aught appeared, without harm or pain, the pressure of the crouded particles of the air; though we guessed this to have been as much compressed by the force of the rammer, as it would have been by a cylinder of water of fifty, or between fifty and sixty foot high. By which also we may be helped to conceive, how great a difference there is, whether the same pressure be exercised by a solid, or by a fluid body. For, according to our estimate, the pressure against the body of the fly was as great, as if a slender pillar of marble, having the fly for its base, and eighteen or twenty foot in height, had leaned upon the little animal; which, I presume, you will easily think was more than enough to crush her to death.

But because, though the foregoing trials are not like to be rejected by the skilful, yet they require a somewhat dextrous and nimble experimenter, and leave something to his estimate, I will subjoin an experiment more easy to be made, and wherein the weight may be determined by measure, rather than conjecture, being made to be perpendicularly incumbent on the fly, or other animal. For the experiment may be as well made on other insects, as worms, though some, that I had provided, chanced to miscarry before they came to be used.

We took then some ordinary black flies (such as use to haunt butchers stalls in warm seasons,) of a middle size, (the length of the body and head of one animal, which for trial's sake we measured, being about three eighths of an inch,) and having placed one of them with the head upwards, that there was some distance left betwixt her and the sealed end of the glass-tube nine or ten inches long; we poured in quick-silver very slowly and cautiously, lest the force of so heavy a body, acquired by the acceleration of its descent, should, more than the meer weight itself of the liquor, oppress the fly. To this effect stooping the glass very much towards the horizon, and letting the mercury pass into the tube thorough a funnel, whose lower part was very slender, that it might come down but by little and little, we at length got in as much mercury as the tube would receive, and then holding it upright, we watched, whether the fly would make any motions; and finding, that she did manifestly stir notwithstanding the incumbent mercury, we measured the height of the mercurial pillar, reaching from the middle of her body to the top of the liquor, and found it to be about eight inches; and the quicksilver being poured out, the fly appeared to be so lively and vigorous, that I doubted not, but if we had a longer glass, the experiment had been much more considerable. But, when afterwards I was able to procure a better tube, the season of flies being almost quite past, I could scarce get any, and those not brisk, as they are wont to be in summer. But however, we repeated the experiment with one of the best we could take of the above-mentioned size, and ordering the matter so, that the mercury incumbent on her, (for there was some beneath her,) appeared to be of a greater height than the formerly employed tube was of, we saw her move one or other of her little legs divers times, though the tube were held upright; and therefore measuring the height of the mercury above her, we found it to amount to sixteen inches and better, and then freeing her from this pressure, we observed, that she immediately found her legs again, and moved up and down briskly enough; but when she was loaden with twenty-three or twenty-four inches of the same quick-silver (though the liquor were soon after poured out) she gave no signs of life; which I suspected might happen, not so much from her having been oppressed by the greatness of her weight, as from the great care of the operator to let down the mercury very obliquely and warily upon her. And this I was the rather confirmed in, because, having got another fly of about the same bigness, though when she was at the bottom of the quicksilver, she seemed so compressed, as not to have any motion, we could take notice of, yet, upon her being taken out of the glass, she presently appeared to be alive by walking about, and beginning to display her wings, though the pillar of mercury, that had leaned upon her, amounted to above twenty-seven inches. And I presume, the success would have been much more considerable, if the experiment had been tried in the summer, when these creatures are brisk and lively, and not, as it was, in the winter; besides that probably these little animals were hurt or weakened by the violence, that would scarce fail to be used in catching them, and putting them into such a place and posture in the glass, as was required; the actual coldness of the quicksilver perhaps also making them somewhat torpid, whilst it touched them so many ways. And it must not be here omitted, that a fly, that seemed but about half so big as one of those hitherto mentioned, being well placed, with some mercury under it, in a glass-pipe held upright, sustained a mercurial pillar of somewhat above twenty-five inches; and though she was not observed to move under so great a weight, yet when once it was taken off, she did not appear hurt, much less crushed to death by it, and probably would have escaped under a much greater weight, if the tube, which was too large, had not already employed all the stock of mercury we then had at hand. But I do presume, that what we did try, will be available to our purpose, since we see clearly, that so small an animal as a fly may survive so great a pressure, and that she could not only live, but was able to move such long and slender bodies as her legs, when she was pressed against by above sixteen inches of mercury, and consequently by a weight, equivalent to a pillar of water of above eighteen foot and a half, which being above five-hundred and ninety times her own length, and, according to the estimate our measure suggested, many times more her own height; so that a diver, six foot tall, (which is somewhat more than an ordinary man's stature,) to have as many times his height of water above him, as our fly might have had, and yet have moved under it, must dive, at least in fresh water, to near a hundred fathom, which is a far greater depth, perhaps by five or six times, than, for aught I could learn by enquiry, the divers either for coral or pearl are wont to descend.

AND now, Sir, having tendered you the likeliest conjectures, that occurred to me about the solution of this difficult problem; I shall return to doctor *More*, and consider the objection he frames from the supposed insolubleness of it. And on this occasion, I shall have two or three things to represent to you.

THE first is, that there would be much more weight in what he objects, if our assertion of the gravitation of water in water were, like the *principium hylarchicum*, a meer hypothesis advanced, without any clear positive proof; whereas our doctrine is not only elsewhere directly proved by particular experiments, but by the very controverted one of the tadpole; to elude whose force, so ingenious a person is fain to fly to a principle, that, to say here no more, is not physical. And from this first of the things, I lately mentioned, I shall hasten to the second, because it will require to be longer insisted on.

I shall then further represent, that whatever power he is pleased to suppose at the bottom of the sea, to suspend the impression of the incumbent water, I think, that supposition ought to give place, if not to our former ratiocinations, yet to experience itself, which shews, there really is a great pressure exercised by the water at the bottom of the sea. I remember, that a friend of the learned doctor's and mine, Sir *R. M.* who is so eminent a virtuoso, as to have been often president of the royal society, related a while since to me, that a mathematical friend of his, whom he named, having had an opportunity to try an experiment, I have in vain endeavoured to get tried for me, had the curiosity to let down in a deep sea a pewter-bottle, with weight enough to sink it, that he might try, whether any sweet water would strain in at the orifice or any other part; but when he had pulled it up again, he was much surprized to find the sides of his pewter-bottle very much compressed, and, as it were, squeezed inward by the water. I also, not long since, enquired of an observing acquaintance of mine, that has a considerable estate in *America*, whether he had not tried to cool his drink, when he sailed through the torrid zone, by letting down the bottles to a great depth into the sea, and if he did, in what condition he found them when they were drawn up again. To which he answered, that he had several times employed that expedient for the refrigeration of his drinks, but was at first amazed to find the corks, with which the strong stone-bottles had been well stopped before, so forcibly and so far thrust in, that they could scarce have been so violently beaten in with a hammer, and it was scarce possible to get them out. And another ingenious person, that practises physick in the *Indies*, having the like question put to him, answered me, that he had some while since had the curiosity to try, in a very deep

deep part of the ſea, whether any freſh water would ſtrain into ſtone-bottles through a thick cork ſtrongly ſtopped in, and having let it down with a convenient weight to one hundred fathom, was much diſappointed, when he drew it up, by finding, that the preſſure of the water at ſo vaſt a depth had quite thruſt down the cork into the cavity of the bottle (which elſe perhaps would have been cruſhed to pieces;) an effect, which he would ſcarce have expected from the ſtrokes of a mallet. And if to all this it be objected, that it was not the preſſure, but the coldneſs of the water, that did the recited feats, by condenſing the included air, and obliging nature to do the reſt for fear of a vacuum; I will not launch into the controverſy, whether nature do any thing *ob fugam vacui*, but only anſwer, that I cannot find, by the relations of the divers, or otherwiſe, that it is ever ſo cold at the bottom of the ſea, as it is frequently above ground in winter, when great fiſhes are commonly ſaid to return to the deep parts of the ſea for warmth; and yet, in the ſharpeſt winters, I never obſerved corks to be driven in by the cold of the ambient; nay, I purpoſely tried with a frigorifick mixture, that very intenſe degrees of cold, ſuch as would quickly freeze many liquors, would not occaſion the breaking of thin bubbles of glaſs, purpoſely blown at the flame of a lamp and hermetically ſealed.

AND to ſhew *ad oculum* (as they ſpeak) that water may preſs more and more, as it grows deeper, againſt the ſtopple of a bottle, though the veſſel be inverted, I will ſubjoin this experiment. Becauſe we have no water hereabouts, that is near deep enough to force in a cork, as the ſea-water did in the above recited trials, I thought of a way of ſo cloſing the glaſs-veſſel, as that the ſtopple ſhould keep aſunder the air in the veſſel and the outward water, and hinder all immediate intercourſe between them, and alſo make ſome reſiſtance againſt the preſſure of the external water, and yet be capable of freely moving up and down, and ſo be a good ſuccedaneum to a ſolid ſtopple. Taking then a glaſs-vial, furniſhed with a ſomewhat long cylindrical neck, whoſe cavity was large in proportion to the reſt of the veſſel, we put into it as much quickſilver as would in the neck make a ſhort mercurial pillar of between half an inch and an inch; then, a piece of very fine bladder, dipped in oil, was ſo tied over the orifice of the glaſs, that no mercury could fall down, or get out, nor water get in at the orifice, and yet the bladder, by reaſon of its great limberneſs, might be eaſily thruſt up towards the cavity of the vial, or depreſſed by the weight of the mercury. This little inſtrument, firſt furniſhed with a weight of lead to ſink it, being inverted, the mercury deſcended into the neck, and cloſed the orifice as exactly as a ſtopple, and yet, with its lower part, depreſſed the bladder beneath the horizontal plane, that might be conceived to paſs by the orifice; then the glaſs being a while kept in the water, that the included air might be brought to the temperature of the ſurrounding liquor, and by a ſtring let further down into the ſame glaſs veſſel filled to about two foot in height, the preſſure of the liquor againſt the orifice of the vial did by degrees drive up the bladder and the mercurial ſtopple into the cavity of the neck, as was manifeſt by the aſcenſion of the quickſilver; and when the inſtrument was leiſurely drawn up again, the weight of this mercury made it ſubſide and plump up the bladder again as before. An experiment a-kin to this, and therefore fit to confirm it, I have delivered in another diſcourſe*.

AND here I ſhall ſubjoin what very opportunely occurred to me ſince the writing of the laſt page. Meeting caſually with an ingenious mechanician, (whom you will find I have elſewhere † mentioned) that deviſed a ſuit of cloaths and other accommodations, wherein I once ſaw him let down into the water, by whoſe help, and that of a boat, he could, and did continue there a great while, at a conſiderable depth under water, and there work; I aſked him afreſh (to obtain fuller informations than formerly) whether he felt not the preſſure of the water againſt his breaſt and belly; to which he anſwered me (more circumſtantially than he had before) that when he was about four or five yards under water, though but in the river *Thames*, his breaſt and abdomen was ſo compreſſed, that there being hardly room enough left for the free motion of his lungs, he could ſcarce fetch his breath, and was neceſſitated to make them draw him quickly up, and that (among his later trials to improve his engine) having for remedy hereof, cauſed a kind of armour for the cheſt and back to be made of copper, though the ſtiffneſs of the metal defended him from receiving any miſchief in thoſe parts, yet in the others, where only the leather, though ſtrong, was interpoſed, when he came to the depth of about ſix fathom, though in freſh water, he found a great preſſure againſt his legs and arms and all the other parts againſt which the water was able to thruſt the leathern ſuit inwards. And this preſſure being found by him, as he told me, pretty equal, againſt all the expoſed parts, (for from the other, which were more yielding and obnoxious, the armour kept it off,) he received no miſchief from it, nor yet much incommodity (and ſome he might expect from the ſtiffneſs and unequal yeilding of the leather:) ſo that he could ſtay under water, though not ſtill at ſo great a depth, about two hours or longer. And upon the whole matter he anſwered me, that he was well ſatisfied by his trials, that the ambient water endeavoured to preſs him and his diving ſuit every way inwards. Whether the coldneſs of the water had any intereſt in this phænomenon, I particularly enquired of the engineer; but he replied, that by reaſon of the tightneſs of his diving ſuit or inſtrument, the warm ſteams of his body, that were pent in, and other concurring circumſtances kept him from feeling any cold, and made him ſometimes feel a greater heat than he wiſhed. He has promiſed me, before it be very long, to make for me a trial or two, that

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* See the paradox about ſuction.

† In the tract of the differing preſſure of heavy ſolids and fluids.

I propounded to him, from whose succefs, if he can but reduce them to experiment, I hope to be able to prefent you a farther confirmation of our hypothefis. In the mean time, the things already recited, together with the preceding experiments, may well fuffice for our prefent purpofe. For, by what hath been faid, it appears, that water does actually prefs againft bodies, whether fpecifically lighter or heavier than itfelf, placed under water, and that this preffure increafes with the height of the water above the emerfed bodies. And this being fo, it is not more neceffary for me, than for men of other opinions, to give a clear reafon, why divers can refift fo great a preffure of the incumbent water. And the preffure of the water in our recited experiment having manifeft effects upon inanimate bodies, which are not capable of prepoffeffions, or giving us partial informations, will have much more weight with unprejudiced perfons, than the fufpicious, and fometimes difagreeing accounts of ignorant divers, whom prejudicate opinions may much fway, and whofe very fenfations, as thofe of other vulgar men, may be influenced by predifpofitions, and fo many other circumftances, that they may eafily give occafion to miftakes. I know, that learned men, that never were converfant in hydroftaticks, are wont to think it very difficult, if not impoffible, to conceive, how fo weak a thing, as they fancy an animal to be, fhould avoid the being oppreffed, or fo much as harmed by fo great a weight of water. But they, that fhall attentively confider what has been offered towards the removal of this difficulty, and remember, how little they would have believed, that there is fo great a difference, as we have by the tadpole, the fly, and other inftances, fhewn there really is between the preffure of folid and of fluid bodies, will, I prefume, be apt to think it fit, that if, for want of a fufficient hiftory of matters of fact, any fcruple remain about the folution we have offered from the nature of the uniform preffure of fluids, and the firm ftructure of the human body, we fhould, to remove thofe remaining fcruples alfo, rather range about for other phyfical helps to folve more compleatly the problem, about fuch a thing as compreffion, which is an action purely corporeal and mechanical, than for want of a ready and compleat folution to fly to the immediate interpofition of an immaterial and intelligent, yet created agent, to explain clearly whofe manner of working would be a much more difficult tafk, than the folution of the phænomenon without it.

AND now, Sir, having prefented to you the reflections I thought requifite to write upon the learned doctor's difcourfes againft my hypothefis and explications, relating to the gravitation and preffure of fluids, I have little more to trouble you with in this paper. For, though in the latter part of the thirteenth chapter, the doctor is pleafed to fpend divers pages in the explication of divers of my hydroftatical phænomena by the agency of that incorporeal director, that he calls *principium hylarchicum*; yet fince thefe explications of his are rather attempts to accommodate the phænomena to the hypothefis, than objections directly levelled againft my folutions, I fhall altogether forbear to examine them; the main thing, that I intended in this paper, according to what I told you at the beginning, being to fhew, that the arguments urged againft the mechanical folutions of the experiments by me recited do not evince any of them to be erroneous. And I have neither the defign nor the leifure folicitoufly to examine the doctor's hylarchical principle. Of which I fhall only fay, that though he tells us, it is *paratum ad movendum quoquo verfum materiam pro data occafione*; Page 175. yet fince he alfo tells us, *Quòd particulæ molis corporeæ, five ftabilis five fluidæ, à principio hylarchico in unam aliquam partem omnes junctim urgeri poffunt & premi, quamvis fingulæ fingulas in nullam partem premant, quodque pro magnitudine molis major minorve totius fit preffio*; Page 167. and that the force, by which it endeavours to keep the elements in their true and natural confiftence, though it be very great, is not invincible: Ibid. I fee no need we have to fly to it, fince fuch mechanical affections of matter, as the fpring and weight of the air, the gravity and fluidity of the water and other liquors, may fuffice to produce and account for the phænomena, without recourfe to an incorporeal creature, which it is like the Peripateticks, and divers other philofophers, may think lefs qualified for the province affigned it, than their fuga vacui, whereto they afcribe an unlimited power to execute its functions. I leave it therefore to you, Sir, to judge, which of the two ways of explicating an hydroftatical phænomenon, the learned doctor's, or, that which I have made ufe of, relifhes moft of the naturalift. And I fhall only tell you, that if I had been with thofe Jefuits, that are faid to have prefented the firft watch to the king of *China*, who took it to be a living creature, I fhould have thought I had fairly accounted for it, if, by the fhape, fize, motion, &c. of the fpring-wheels, balance and other parts of the watch I had fhewn, that an engine of fuch a ftructure would neceffarily mark the hours, though I could not have brought an argument to convince the Chinefe monarch, that it was not endowed with life. From which comparifon you will eafily gather, that what I have thought my felf concerned to do in this place, was not to demonftrate in general, that there can be no fuch thing, as the learned doctor's *principium hylarchicum*, but only to intimate, that, whether there be or not, our hydroftaticks do not need it. Nor do I think it neceffary to the doctor's grand and laudable defign, wherein I heartily wifh him much fuccefs, of proving the exiftence of an incorporeal fubftance. For as I think, truth ought to be pleaded for only by truth; fo I take that, which the doctor contends for, to be evincible in the righteft way of proceeding by a perfon of far lefs learning than he, without introducing any precarious principle; efpecially experience having fhewn, that the generality of heathen philofophers were convinced of the being of a divine architect

tect of the world, by the contemplation of so vast and admirably contrived a fabrick, wherein yet taking no notice of an immaterial *principium hylarchicum*, they believed things to be managed in a mere physical way, according to the general laws, settled among things corporeal, acting upon one another. And after this I have nothing more to say, but that I would not have any thing, that I have said, misconstrued to the learned doctor's prejudice. For it is not necessary, that a great scholar should be a good hydrostatician. And a few hallucinations about a subject, to which the greatest clerks have been generally such strangers, may warrant us to dissent from his opinion, without obliging us to be enemies to his reputation. And therefore, if you have found any thing in this paper inconsistent with a just tenderness of that, you have not only my consent, but my desire to alter it, as an expression, that doth not well comply with my intentions of not appearing any farther his adversary in our debate, than the desire of shewing my self a friend to the truth I was to defend, should exact of,

SIR,

Yours, &c

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HYDROSTATICAL LETTER,

Written *February* 13, $167\frac{2}{3}$.

CONTAINING,

A Dilucidation of an Experiment of the Honourable Author of these TRACTS, about a Way of weighing Water in Water, upon the occasion of some Exceptions, made to it by Mr. *George Sinclair* *.

* In his Hydrostaticks, printed at *Edinburgh*, 1672. p. 146. ff.

To the READER.

WHEN this discourse was just finishing in the press, there came to the publisher's hands a dilucidation of an experiment of the honourable author of these tracts, about a contrivance of his for estimating the weight of water in water, formerly published in Number L. of the *Philosophical Transactions*, and by the following discourse cleared from the exceptions to be met with in Mr. *George Sinclair*'s book, entitled *The Hydrostaticks*, &c. printed at *Edinburgh*, 1672. Which dilucidation, because of the affinity of the subject, was thought fit to be here annexed.

AN
HYDROSTATICAL LETTER, &c.

SIR,

CALLING this night in *Paul's Church-yard* for the ingenious Mr. *Ray's* travels, that you yesterday commended to me, I was also shewn a new treatise, that I never saw before, of a learned gentleman, and hastily running over the index, found an experiment of mine declared insufficient; and though, being hindered to make hast home, it be so late, that, far from having time to peruse the book itself, (which I tell you, that you may not now expect any character of it from me) I have been scarce able to read over, more than once, what directly concerns me in it; yet I shall adventure to say something about it this night, for fear I should not, in so busy a time as this, be allowed to do it to-morrow.

WHEREAS then the learned objector, having recited my experiment about weighing water in water, as you were pleased to publish it in a book enriched with so many better things, the *Philosophical Transactions*, begins his animadversions with saying, that "herein is a great mistake:" I shall not in that much oppose him; for possibly the dispute between us is not much more than verbal. And because my experiment coming abroad by itself, and supposing things, that I had formerly proved, and published, but which were not expresly referred to in it, I wonder not, that my meaning should not by all readers be fully understood. And therefore, to explain myself on this occasion, give me leave both to repeat my opinion, and to shew you, on what occasion, and how far, I designed to confirm it by this experiment. My opinion then was, and still is, that as water is a heavy fluid, so it does retain its gravitation and power of pressing; by which I mean a tendency downwards (whatever the cause of that gravity be) whether it have under it a body either specifically heavier or lighter than itself, or equiponderant to it. For I see not what should destroy or abolish this gravity, though many things may hinder some effects of it. And therefore I suppose, that water retains its gravity not only in air, but in water too, and in heavier liquors, and consequently, by vertue of this, the liquor presses upon them; but if a surrounded fluid have, upon the score of its specifick gravity, an equal, or a stronger tendency downwards, than water, it will, by vertue of that, be able to impel up this liquor, or to keep it from actually descending: so that a portion of water, supposed to be included in a vessel of the same specifick weight with water, this portion, I say, placed in a greater quantity of the same water, will neither rise nor fall, as I have elsewhere shewn; but yet it retains its gravity there; only this gravity is kept from making it actually descend by the contrary action of the other water, whose specifick gravity is supposed equal: as when a just balance is loaded with a pound weight in each of its scales, though neither of the weights actually descend, being hindered by its counterpoise, yet each retains its whole weight, and with it presses the scale it leans upon; so that our lately mentioned included portion of water does really press the subjacent water, though it does not actually depress it, or (as perhaps a school-man would phrase it) does gravitate on it, but not pregravitate. Nor do I think, that the only way of judging, whether a body gravitates, is to observe, whether it actually descends, since in many cases its gravity may be proved by the resistance it makes to heavy bodies, which, if it were not one, would raise it: as may be declared by what I just now noted about equal weights in a balance. And for want of this distinction I have known even learned men, treating of hydrostatical things, mistake both me and the question.

Numb. L.

THE next thing I had to tell you, is, that the adversaries I had to deal with, both in print, and in discourse, denied, that in standing water, the upper parts did press or gravitate upon the lower; and though they could not but grant, that the whole weight of the water did gravitate upon the bottom of the vessel; yet they would have the parts of it to do so *actione communi* (as they speak) and fancied I know not what power of nature to keep the homogeneous portions of water, as well as other elements, from pressing one another, when it is in its proper place. Against this opinion, (which I presume my learned adversary and I agree in opposing,) it was alledged, besides other things, which I found many, otherwise good scholars, were not fitted to understand, that if a glass-vial or bottle, well stopped, were deeply immersed under water, it would strongly tend upwards; but if it were dextrously unstopped, when it was thus immersed, so as the water could get in, abstracting from or allowing for the weight of the glass itself, it would by the water, that crouds in and thrusts out the air, be made strongly to tend downwards, and continue sunk. But this not satisfying, because it was pretended, that the reason of the empty bottle's emerging, when stopped, was the positive levity of the air it was filled with, and the sinking of it, when unstopped, was from the recess of the same air, that by the intruding water was driven with large bubbles out of the bottle; I thought this evasion might be obviated by contriving an experiment, wherein the water should be plentifully and suddenly admitted into the glass, and yet no air expelled out of it, (which circumstance I therefore took notice of, where

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I say, "no bubble of air appeared to emerge "or escape through the water,") so that if then the glass, that was kept up before, should fall to the bottom, with a gravitation amounting to a considerable weight in respect of its capacity, the sinking of it could not by them be ascribed, as they suppose, with positive levity, but to the weight of the admitted water, which, when thus weighed, would be invironed with water of the same kind: and to shew, that this admitted water might have a considerable weight, notwithstanding the place it was in, I employed a pair of scales after the manner, that is recited in the experiment.

BY what I have been discoursing, you may conceive, that however my expressions disagree with those of my adversary, the distance of our opinions is not so wide, as at first sight it seems. For he allows, as well as I, that the superior parts of water do by their gravity, (for I know not on what other score they can do it) press the inferior. But this he would not have amount to this expression, "that water weighs or gravitates in water;" whereas I scruple not to cloath my sense in that expression, because I think water does always exercise its gravity, though it does not always pregravitate, or actually descend, being often, as I noted above, either impelled up by an opposite and prepollent weight, or hindered from descending by the resistance of other water, that counterpoises it: so that, if he thinks, that in my experiment I meant to propose a method of making water descend in water, and weigh it in that liquor with a pair of scales, just as if I would weigh in the same water a piece of lead, or a portion of mercury, which are bodies much heavier in specie than water, either he mistakes my intention, or I did not sufficiently declare it. But that, which I designed to shew, and, for aught I can yet see, have shewn, was, that by the help of an ordinary balance it may be made appear, that water admitted into the glass bubble, I employed, did make the glass bubble weigh so much heavier than it did before that liquor entered into it; and that this new weight, that was manifested by the balance, was not due, as my adversary supposed, to such a recess of the air, as I mentioned a while ago.

AND now, Sir, it will be proper to take notice of some passages in the objector's discourse, in order to dilucidate the subject of it. Whereas he says, (page 149, and 150.) "Take a piece of wood, that is lighter in "specie than water, and add weight to it by "degrees, till it become of the same weight "with water; knit it with a string to a balance, and weigh it in water; and you will "find the whole weight supported by the "water:" I answer, that this does not at all overthrow my opinion, but agrees very well with it. For suppose the weight you add to the light wood be lead, it cannot be said, that the metal loses its native ponderosity, whilst it rests in the water; and the reason, why it descends not, is, that it, and the wood it is joined to, are hindered by the counterpoise of the collateral water, which, by its pressure, would raise the surface of the water, whereon the floating or swimming body leans, if it were not hindered by the weight of these incumbent solids: and this resistance of theirs to the endeavour upwards of the water, being exercised only upon the account of their gravity, shews, that they do in my sense gravitate, though not pregravitate.

AGAIN, if you please to consider the case, put by the objector, page 151, and cast your eyes upon his scheme, which, supposing you to have his book, I shall, for brevity sake, make use of at present; you will find him thus argue. "Now, I say, it is six ounces of the weight (*B*) that makes this alteration, and "turns the scales: for if twelve ounces sink "the glass below the water, when it is full "of air, and no water in it, then surely six "are sufficient to sink it, when it is half full. "And the reason is, because there is a less "potentia, or force, in six inches of air, by the "one half, to counterpoise a weight of twelve "ounces, than in twelve inches of air. Therefore this air being reduced from twelve inches to six, it must take only six ounces to "sink it." Pag. 151.

To which I answer, that I know not yet what, on this occasion, he means by a potentia, or force, in six inches of air, to counterpoise a weight of twelve ounces. For by the term counterpoise, where the question is about weighing, one would think he speaks of weight; and yet air, according to the vulgar opinion, is positively light; according to us, though it have a gravity, yet in our case that must amount to so little, that what air the bubble needed to fill it, could not weigh at most above four or five grains, which therefore might safely be neglected. But, according to my opinion, the reason of the phænomenon is clear enough, without meddling with the potentia of the air. For if we conceive a horizontal plane to divide the water mentally, and pass by the bottom of the suspended bubble; before the little stem be taken off, there is a far greater pressure upon the other parts of that plane, than upon that, which lies under the bubble, in regard they are pressed by the weight of the collateral water (*A, L, G, D, M, C,*) whereas the other is pressed only by the weight of a body very much lighter than its equal bulk of water: so that, to keep the bubble from being forcibly buoyed up, there was requisite eighteen ounces of lead, that make up the plummet (*B,*) to detain it under water, and keep the beam of the balance horizontal; that when access is given (at *C*) to the neighbouring water, it is by the weight of the collaterally superior water impelled into the cavity of the bubble, where the air, being much rarified before, could not resist its ingress, and thereupon, six ounces of water getting in, that part of the imaginary plane, on which the bubble was incumbent, is pressed by a greater weight than formerly by six ounces, and consequently, there needs the like weight in the opposite scale of the balance, to reduce the scale to an æquilibrium. And if

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we suppose, with our author, the glass to be compleatly full of water, and the counterpoise in the scale (O) to need six ounces more to make a new æquipondium, the account of the phænomenon will be the same, as, if you attentively consider it, you will clearly perceive. And the reason, why the additional weight of six ounces is required, will be, that the upper half of the bubble, that before contained less than three or four grains weight of air, being now filled with water, amounted to six ounces more of water than formerly, and so the counterpoise, in the opposite scale (O,) will need the weight of six ounces to make a new æquipondium.

CONGRUOUSLY to this explication, when
Pag. 152. the examiner says, "Now I enquire, whether "these eighteen ounces are the æquipondium "of the water within the glass, or of the weight "of the lead (*B*)? It is impossible they can "counterpoise both, seeing the water is now "twelve, and *B* eighteen. It must then either "be the counterbalance of the water, or the "counterbalance of the lead. It cannot be the "first, because twelve cannot be in æquipon-"dio with eighteen; it must then be in the "second: or if these eighteen ounces in the "scale (O) be the counterpoise of the water "within the glass, I enquire what sustains the "weight of the lead (*B*)? The weight of it "cannot be sustained by the water, because it "is a body naturally heavier than water; it "must therefore be sustained by the balance." I answer, that this specious objection seems, (for it is somewhat obscurely worded) to be founded upon a mistake of my meaning in the question. However, as to the phænomenon itself; according to my sense, the eighteen ounces in the scale (O,) are the counterpoise of the eighteen ounces, that hang from the opposite and æquidistant scale, and make up the leaden plummet (*B*,) which answer, I see not how our author prevents.) But then you will ask, what counterpoises the water in the bubble, which alone weighs twelve ounces? I answer, that it is the gravitation of the collateral water, which presses the other parts of the lately-mentioned imaginary plane, as much as the water in the bubble, the weight of the glass being here not reckoned by either of us; and the water incumbent on the bubble does press that part of the plane on which they lean; so that there being in all thirty ounces to be sustained, the eighteen of the plummet, and the twelve contained in the glass, the lead, that hangs in the water, is counterpoised by eighteen ounces in the scale, and the water in the bubble by the pressure of the collateral water.

BUT you will say, that it appears not, that the included water presses at all, since it does not at all descend. To which I answer, that as long as the water was getting into the cavity of the bubble, so long it did manifestly gravitate upon the subjacent plane, and actually descend, raising the counterpoise in the scale: but when, by adding more weight to that counterpoise, things are brought to a new æquilibrium, there is no reason, why the gravitation of the water should again change the now regained æquipondium. Suppose, in the two scales of a balance there were placed two equally capacious and equiponderant vials, whereof one is quite full, and the other almost full; it is evident, that the full vessel will keep the scale it leaned upon depressed, and if you gently pour in as much water into the unfilled, as the filled has more than it, the scale, that was formerly kept raised, will be now depressed, till the beam be brought to be horizontal; to which posture when it is once brought, the æquilibrium will continue: and yet it will not be said, that though the added water, whilst it was filling the glass, depressed the scale it belonged to, yet it lost its weight, or, which in my sense is all one, did not gravitate upon the scale, when the balance was come to an æquilibrium, because then this water did no longer depress it. And how much the water in our bubble does, notwithstanding its immersion, gravitate, would be visible, if, by supposition, it were all annihilated, and no other suffered to supply its room. For then the subjacent part of the imaginary plane being much less pressed, than immediately before, the weight of the collaterally superior water would strongly impel up the bubble, if it were not kept in its place by a proportionable addition of weight to the plummet. Nor should it seem a strange thing, that I should say, that the thirty ounces, lately mentioned, should be counterbalanced partly by the weight in the opposite scale, and partly by the water, that fills the immersed bubble; since this notion may be warranted even by the common practice of weighing heavy solids hydrostatically. For if you would, for instance, weigh a lump of copper of nine pound in common water, the metal, hanging by a horse-hair under water, will need, according to my elsewhere mentioned experiments, either just or near about eight pound in the opposite scale, to keep the balance horizontal, so that the whole nine pound, that the lump weighed in the air, is counterpoised partly by the eight pound newly mentioned in the opposite scale, and partly by the weight, or resistance following from weight, of as much of the water as the copper fills the room of; which, as experience shews, is one pound: and if we should conceive water in a vessel adiaphorous, as to gravity and levity, to be substituted in the place of the metalline lump, it would weigh as much as the ninth part of the copper lump weighed in the air, and the same counterpoise of eight pound would maintain the æquilibrium.

WHAT the learned objector has, at the close of his discourse about the natural and artificial balance, could not without prolixity, and is not here necessary to be dwelt upon; especially since you will see, in what I suppose you have now received from the press, in answer to the ingenious Dr. *More*, what is to be said on that subject, according to my hypothesis. Wherefore, though my learned adversary does in the 152d page conclude, "That "water cannot weigh in water," and asserts "that the pressure of water is one thing, and "water to weigh in water is another;" yet, as

I said at first, I conceive much of our difference may be verbal; and, in my sense, when water presses subjacent water, because it does so upon the score of its gravity, it gravitates in water, though it does not pregravitate, that is, actually descend. And since it is in the sense of this last expression, that our author, if I mistake him not, speaks of weighing in water, his conclusion, that water cannot weigh in water, does not contradict me, who affirm not, that water does so weigh in water. Whether we shall agree in all other points of Hydrostaticks, you will easily believe, that I cannot yet tell; though by the expression he is pleased to use in the 146th page, to usher in his objection with, it is probable we may. And as to the now-dispatched debate, if I have employed some words in another sense than he, I presume he is so equitable as to consider, that I did not write of these things after having seen this book of his, but some years before; and have since found those expressions justified by the use, that eminent writers have thought fit to make of them. And however I am glad, that he has given me this opportunity of clearing my experiment, and declaring by examples, as well as words, the opinion it relates to; especially, if it seems to others, that I omitted to express myself so fully; my design being, as I formerly told you, to convince such adversaries, as I then had met with, by shewing, that the above-recited phænomena of the emersion and sinking of a glass vial depended upon the gravity of the water, and not upon the positive levity of the air.

NEW EXPERIMENTS

Of the POSITIVE or RELATIVE

LEVITY of BODIES under WATER.

IT is obvious, even to the vulgar, as well as to philosophers, that if wood, wax, or another body, that is lighter in specie than water, and naturally floats upon it, be detained under water, it will, upon removal of that force, emerge to the top. And this it does so readily, and, as it seems, spontaneously, that not only the Peripatetick schools, but the generality of philosophers, both ancient and modern, do, as well as the vulgar, ascribe this ascension of lighter bodies in water to an internal principle, which they therefore call positive levity.

BUT this principle was not always so universally received among philosophers, as in later ages it proved to be; *Democritus*, and several of the ancients, both atomists and others, admitting no absolute, but only a relative or respective levity; which opinion some of the moderns have ingeniously attempted to revive.

BUT, because whatever wit they may have employed in arguing, yet the schools seem to have the advantage in point of experience, the obvious instances, given by the Peripateticks, having neither been solved by real and practical variations of the same instances, nor counterbalanced by new experiments of a contrary tendency; the importance and difficulty of the subject invited me to attempt, when I was upon hydrostatical trials, whether I could experimentally shew, that whatever becomes of the general question about positive levity, we need not admit it for the true and adequate cause of the emersion of wood, and such lighter bodies, let go under water.

EXPERIMENT I.

THE instance, that is wont to be urged to prove the positive levity of wood in water, seems to me to have been too perfunctorily made, to be safely acquiesced in. For even as it is proposed with advantage by a learned foreign mathematician, I cannot think it accurate enough to determine the present controversy: for I will readily allow him to suppose, that in case a flat board, as for instance, a trencher, have its broad surface kept by a man's hand or other competent force upon the horizontal bottom of a tub full of water, if the hand or other body, that detained it be removed, it will ordinarily happen, that the trencher will hastily ascend to the surface of the water. But I do not perceive, that a decisive experiment of this kind is easy (not to say, possible) to be made with such materials. For the wood, whereof both the trencher and the bottom of the barrel consist, are supposed to be lighter in specie than water; and to be so, they must be of a porous and not very close texture. To which agrees very well, that the solider woods, as lignum vitæ, Brasil, &c. whose texture is more close and compact, will not float on water but sink in it: and therefore, if there be not much more care used, than I have yet heard, that any experimenter has employed, to bring the surfaces of the trencher, and the bottom of the barrel, to a true flatness, and as much smoothness, as they can be brought to, I shall not think the trial so accurately made, as it might be; not to say, which I suspect, that though it be mentally, yet it is scarce

ſcarce practically poſſible to bring ſuch porous bodies, as thoſe of the lighter woods, to be fit for ſuch a contact as might be neceſſary to make the trial accurately. And in caſe that were actually done, I ſhould be kept from expecting, with my adverſaries, the emerſion of the trencher, by the experiment by and by to be recited, and by the true reaſon of it.

I think then, that the cauſe, why in ordinary inſtances, wood, wax, and other bodies ſpecifically lighter than water, being let go at the bottom of a veſſel full of that liquor, emerge to the top, is chiefly, that there is no ſuch exquiſite congruity and contact between the lowermoſt ſuperficies of the wood, and the upper ſurface of the bottom of the veſſel, but that the lateral parts of the water, being impelled by the weight of the parts of the ſame liquor incumbent on them, are made to inſinuate and get between the lower parts of the wood and the bottom of the veſſel, and ſo lift or thruſt upwards the wood, which bulk for bulk is leſs heavy than the water that extrudes it.

THAT this is the reaſon of the emerſion or aſcenſion of bodies, lighter in ſpecie than the fluids they ſwim in, is moſt conſonant to the laws of * Hydroſtaticks, as I have elſewhere ſhown. But whereas the whole force of the argument of thoſe I diſpute with, conſiſts in a ſuppoſition, that, becauſe the trencher (formerly ſpoken of) is placed upon the bottom of the barrel, no water can come between to buoy it up, whence they conclude, it muſt aſcend by an internal and poſitive principle of levity, I thought fit to make the experiment after another, and, if I miſtake not, a better manner.

WE took then two round plates of black marble ſhaped like cheeſes, which had thoſe ſuperficies, that were to be clapped together, ground very flat, and poliſhed very carefully, that the ſtones being laid one upon the other, might touch in as many of the ſuperficial parts, as the workman could bring them to do; that, whilſt they were in that poſition, the uppermoſt being taken up, the other would ſtick to it, and aſcend with it. And to keep out the water the better, the internal ſurfaces were, before they were put together, lightly, and but very lightly, oiled; which did not hinder them from moſt eaſily ſliding along one another, either forward or backwards, or to the right, or to the left, as long as the contiguous ſurfaces were kept horizontal.

THESE things being done, a blown bladder, of a moderate ſize, was faſtned to the upper marble, and both of them were let down to the bottom of a tub of water, where, by the help of an eaſy contrivance, the lower marble was kept level to the horizon. And now the patrons of poſitive levity would have concluded, that the bladder, being a body, granted to be by vaſt odds lighter than wood, and being in an unnatural place beneath the ſurface of the water, ſhould, of its own accord, and with impetuoſity, emerge; but I expected a contrary event, becauſe the bladder being tied to the upper marble, ſo that both of them might in our caſe be conſidered as one body, the water could not impel them up, in regard that the cloſe contact of the ſurfaces of the two marbles kept the water from being able to inſinuate itſelf between them, and conſequently from getting underneath the upper marble, and preſſing againſt the lower ſuperficies of it. And to ſhew, that this was the reaſon of the bladder's not emerging, I cauſed one of the by-ſtanders to thruſt his arm down to the bottom of the tub, and with his hand to make part of the oiled ſurface of the upper marble ſlide off, on any ſide, from that of the lower, which, by reaſon of the ſmoothneſs and ſlipperineſs of the ſurfaces, he found moſt eaſy to do. But the contact ſtill continuing according to a greater part of the ſurfaces than was requiſite, I bid him yet ſlide, but by ſlow degrees, more and more of the upper marbles from the lower, till at length, when, according to his gueſs, the marbles touched but in one half of their ſurfaces, the endeavour of the water to extrude the bladder full of air being ſtronger than the reſiſtance, which the contact, but of part of the ſurfaces of the ſtones, was able to make, they were ſuddenly disjoined, and the bladder was by the extruding water impetuouſly, as it were, ſhot up, not only to the top of the water, but a good way beyond it.

WITH theſe marbles we made ſeveral other experiments of this kind, moſt commonly letting down the marbles both together; but once or twice at leaſt placing the upper marble under water upon the lowermoſt already fixed to the bottom of the barrel.

THAT it was not the weight of the upper marble, nor want of lightneſs, whether poſitive or relative, of the air included in the bladder, that kept it from aſcending, was plain, not only upon the newly-mentioned impetuous emerſion of it, upon the disjoining of the marbles, but by this, that the bladder would lift up from the lower parts of the water, not only the upper ſtone, when it touched not the other, but a weight of ſeven or eight pound hanging at it.

AND that a *fuga vacui* was not an adequate cauſe of the coheſion of the marbles in our experiment, may be argued from this, that whether or no nature do any thing, at any time, out of abhorrence of a vacuum (which may be much diſputed;) yet, in our caſe, this abhorrency could not be well pleaded by its aſſertors, ſince many of them hold it to be unlimited, and the more modeſt, to be at leaſt capable of lifting up prodigious weights; whereas, in our experiment, the levity of a bladder, that could not raiſe ten pound weight, was ſufficient to disjoin the marbles, when they yet touched one another according to half their ſurfaces.

EXPERIMENT II.

TO ſhew, now whether it is not rather the gravity and preſſure of the water, or other ambient fluid, than the poſitive levity of

* See the Hydroſtatical Paradoxes.

of a body lighter in ſpecie than it, that makes the immerſed body aſcend to the ſurface of the liquor, I deviſed this experiment:

WE took a bladder, out of which a great part of the included air had been expreſſed, and tying the neck of it very cloſe, that none of the remaining air might get out, we faſtned to it a conſiderable weight of ſome very ponderous body, as lead or iron. By the help of this, we ſunk the bladder to the bottom of a wide mouthed glaſs, full of water, that the ſurface of the liquor might be a good deal higher than the upper part of the bladder: this wide mouthed glaſs we included in a great receiver (whoſe orifice muſt be very large to be able to admit ſuch a veſſel) which I cauſed to be carefully cemented on to the engine. The main ſcope of this experiment was to ſhew, that though the air, included in the bladder, was very far from being able, by its abſolute levity, to lift up ſo great a weight, as the bladder was clogged with, yet the ſame air, continually included in the bladder, would, by its meer expanſion, without any new external heat, acquire a power of aſcending in ſpite of that weight; which aſcenſion therefore muſt be attributed to the water, which, according to the laws hydroſtatical, ought, *cæteris paribus*, to reſiſt, or buoy up more potently thoſe immerſed bodies, that being lighter in ſpecie, than it, poſſeſs the greateſt place in it, and hinder the more water from acquiring its due ſituation: as we ſee, that among hollow ſpheres of glaſs and metal, equally thick and well ſtopped, there is a much heavier weight requiſite to ſink a large one than a ſmall one. For the proſecution of this trial, we began to pump the air out of the great receiver; and its preſſure upon the ſurface of the water being thereby more and more leſſened, (according to what we elſewhere more fully declare) the ſpring of the included air began by degrees to diſtend the ſides of the bladder, till at length that veſſel of air ſwelling every way, took up ſo much more room in the water than it did before, that the water was able to lift the bladder and the annexed weight to the top, and detain it there, till we thought fit to let in again ſome of the excluded air, which forcing that in the bladder to ſhrink in its dimenſions, the weight was preſently able to ſink it to the bottom.

AND here it may be noted, that if, inſtead of hanging ſo great a weight at the neck of the bladder, we faſtened but a moderately heavy piece of lead, ſuch as would only ſerve to ſink the bladder, and keep it at the bottom of the water, ſo that the aggregate of the bladder, air, and metal, was but a little heavier than a bulk of water equal to them: then, upon the firſt ſuck or operation of the pump, which could withdraw but a ſmall part of the air in the receiver, the air in the bladder ſuddenly expanding itſelf, would forthwith be impetuouſly extruded by the water, though after ſome reciprocations it would float in its due poſition, till upon the return of a little outward air, ſometimes as little as we could conveniently let in, it would immediately ſubſide.

BUT this is not ſo neceſſary to be inſiſted on, as it is to take notice, that I foreſaw it may be objected, that the aſcenſion of the weight was not effected by the preſſure of the water, but by this, that rarity and levity being qualities exceedingly of kin, the great rarefaction of the air might proportionably increaſe the levity of it, and conſequently enable it to perform much greater things than it could do before.

I will not here diſpute, whether, generally ſpeaking, a body rarified without heat, would, in vacuo, or in a fluid not heavier in ſpecie than the body when rarified, meerly, by ſuch a greater diſtance of its parts as may ſuffice to entitle it to rarefaction, become really heavier or lighter than before. I will not, I ſay, diſcuſs this queſtion here, where it may ſerve my turn to ſatisfy the recited objection by the following experiment.

EXPERIMENT III.

ABOUT the neck of a conveniently ſhaped vial capable to hold ſome few ounces of water, I cauſed to be carefully tied the neck of a ſmall bladder, whence the air had been diligently expreſſed, ſo that the bladder, being very limber of itſelf, and probably made more ſo, as well as more impervious to air and water, by the fine oil we had cauſed it to be rubbed with, lay upon the orifice of the vial like a ſkin clapped together with many folds and wrinkles.

THIS done, we let down the vial into a conveniently ſhaped veſſel full of water, and the vial, being poiſed before-hand for that purpoſe, ſunk perpendicularly in the liquor, till the neck of the glaſs was partly above and partly beneath the ſurface of the water: then covering the external glaſs with a large receiver, we cauſed the air to be pumped out, and as the preſſure of that was gradually withdrawn, the air in the floating vial did little by little expand itſelf into the bladder, and unfolded the wrinkles of it, till at length it became full blown, without altering the erected poſture of the glaſs it leaned upon. But this great expanſion, being made above the water, and conſequently in a medium not heavier than the included air, gave that highly rarified air no ſuch increaſe of levity, as enable us to perceive, that it made ſo much as the neck of the glaſs ariſe higher in the water, than it did before. Nor did we take notice, that the return of the air into the receiver, by reducing the air in the bladder to its former unrarified eſtate, made the glaſs ſink deeper than before. But when the experiment was tried with the ſame glaſs and bladder, at the bottom of the water, then, upon the pumping out the air, the bladder being dilated under water, was after a while carried up to the top, and took up with it about eight or ten ounces, that had been, to clog it, faſtened to the bottom of the vial.

NEW EXPERIMENTS

About the PRESSURE of the

AIR'S SPRING

ON BODIES UNDER WATER.

I Do not think it were difficult for an intelligent peruſer of our phyſico-mechanical experiments, to find there divers phænomena, whence it may be deduced, that bodies under water, though kept by that liquor from the immediate contact of the air, may yet be expoſed to its preſſure, whether the air act as having a weight, or as a ſpring. But becauſe not only the vulgar, but philoſophers, have been ſo long and generally poſſeſſed with an opinion, that a fluid ſo little heavy as the air, cannot by its weight act upon a liquor, that is, like water, bulk for bulk, a thouſand times heavier than it: and becauſe alſo it ſeems yet more ſtrange, that a little air, perhaps not amounting to a ſcruple or drachm in weight, ſhould in its ordinary ſtate of laxity act conſiderably upon bodies, which, being covered with water, ſeem, by the interpoſition of that liquor, to be fenced from the incumbent air; it may be worth while to add three or four hydroſtatical experiments, to confirm a truth, that very few are yet acquainted with; and add to the proofs, already given of the power of the ſpring of the air, ſome of the operations we have diſcovered it to have upon bodies placed under water.

There are two ſorts of trials, that I ſhall employ to ſhew, that a ſmall quantity of encloſed air may, by its preſſure, (which in our caſes muſt depend upon its ſpring) have a conſiderable operation upon bodies under water, notwithſtanding the interpoſition of that liquor.

For, this preſſure we ſpeak of, may be manifeſted, in the firſt place, by what it directly and poſitively operates upon bodies covered with water: and, in the next place, by the things, that regularly enſue upon the removal of the encloſed air, or the weakning of its ſpring.

EXPERIMENT I.

To begin with the former way of ſhewing the preſſure of the air, I thought it ſufficient, in regard of the trials to be referred to the ſecond way, to make the following experiment.

We took a ſquare glaſs-vial, gueſſed to be capable of holding between half a pint and a pint of water; the neck of this we luted on carefully and ſtrongly (for elſe it would have been buoyed up) over the orifice of the ſmall pipe, at which the air paſſes in our engine out of the receiver into the pump: then whelming over this glaſs a great receiver, we luted it ſtrongly to the engine (that it might as well keep in the water as keep out the air) and at the top poured in as much water as ſufficed to inviron the internal receiver (if I may ſo call it) and cover it to a pretty height. This done, we exactly cloſed with a turning-key, the hole in the great receiver, at which the water had been poured in, that no air might get in or out that way. And laſtly, we began to pump out the air contained in the internal receiver; to the end that that air, which by the above-mentioned pipe had communication with the external air, might no longer by its preſſure aſſiſt the glaſs to reſiſt the preſſure, which the incumbent and encloſed air, by vertue of its ſpring, conſtantly exerciſes upon the ſubjacent water, and by its intervention upon the ſides and bottom of the internal receiver.

And as we expected not, that this glaſs by its own ſingle force, ſhould reſiſt the preſſure of the air encloſed in the upper part of the great receiver notwithſtanding the interpoſition of the water; ſo the event fully juſtified our conjecture: for at the firſt exſuction, which could not be ſuppoſed to have well emptied the internal glaſs, this veſſel was, by the preſſure of the ſuperior air upon the circumſtant water, broken into I know not how many pieces. And the ſame experiment, though with a little ſlower ſucceſs, was repeated with a ſtronger internal glaſs.

EXPERIMENT II.

I Proceed now to the ſecond way of manifeſting the preſſure of encloſed air upon bodies under water, which is by ſhewing the phænomena, exhibited by thoſe bodies upon the removal or leſſening of that preſſure.

Having ſqueezed out of a moderately ſized bladder the greateſt part of its air, we tied the neck of it very cloſe, and then faſtening to it a competent weight, we placed it at the bottom of the talleſt and largeſt glaſs we could cover with our great receiver, that ſo, though the incumbent air were pumped out, none of the water might be pumped out with it, but ſtill retain the ſame height above the bladder. Having then poured upon the bladder as much water, as would ſwim a great way above the upper part of it, we covered this glaſs

glaſs of water with a great receiver, which being carefully cemented on to the engine, the pump was ſet a work, and as the air, which by its ſpring preſſed upon the ſurface of the included water, was by degrees pumped out, ſo the air, that was impriſoned in the bladder, did gradually expand it ſelf at the bottom of the water, as if no ſuch liquor had interpoſed between them otherwiſe than by its weight, upon whoſe account it muſt be allowed to give ſome little impediment to the expanſion of the bladder, in proportion to the height it had above it.

THE event of our experiment was ſuch as was expected, namely, that the immerſed bladder was at length full blown, by the dilatation of the air incloſed in it; and by its intumeſcence made a conſiderable part of the water run over by the ſides of the glaſs, that before contained it all. And when acceſs was given again to the external air, the internal being compreſſed, the bladder was preſently reduced to its wrinkled ſtate.

EXPERIMENT III.

WE took a ſmall but fine bladder, whoſe neck was ſtrongly tied up, when it was, by gueſs, about half full of air: this we put into a ſhort braſs cylinder, the lower of whoſe baſes was cloſed with a braſs-plate, and the other left open; this open orifice we afterwards ſtopped, but not exactly, with a cylindrical plugg, that was ſomewhat leſs wide than it, and was by a rim at the top hindered from reaching too deep into the cavity of the cylinder, that it might not do miſchief to the bladder, that lay there beneath it; upon this plugg we placed an almoſt conically ſhaped weight of lead, and this pile of ſeveral things being ſo placed upon our engine, that we could cover it with a great receiver, we carefully cemented on this veſſel, and at the top of it poured in ſo much water, as would ſerve to fill the vacant part of the braſs cylinder, and the cavity of the engine to ſuch a height, that it covered all the leaden weight, which was ſeveral inches high, except a rim, which was faſtened to the top of it for the convenienter removing of it.

ALL this being done, the pump was ſet to work, and long before we had exhauſted the air of the receiver, that, which was incloſed in the lank bladder, had by degrees, diſplayed ſo vigorous a ſpring, that it had heaved up the weight, that lay upon it, to a notable height, and kept it there, till the air was let in from without, to aſſiſt its being depreſſed by the leaden weight, which amounted to no leſs than about 28 pound.

EXPERIMENT IV.

THERE remained yet one trial to be made, which, in caſe it ſhould ſucceed, ſeemed likely to appear as great an evidence of the force of the air's ſpring upon bodies under water, as could be reaſonably deſired of us; it having been looked upon by many virtuoſi, as the conſiderableſt inſtance of the force of the air's ſpring, even when no water intervened in the trial.

TO ſatisfy therefore our curioſity, we took a copper veſſel of a cylindrical ſhape, and a conſiderable height; into this, being firſt almoſt filled with water, we put a ſquare glaſs-vial, capable, by gueſs, to hold nine or ten ounces of water, and exactly ſtopped with a cork and a cloſe cement: this vial, by a competent weight, was detained at the bottom of the water, from whoſe upper ſurface it was conſiderably diſtant: then the copper veſſel being placed upon the engine, and included in a great receiver well cemented on, the air was by degrees pumped out; but before it was quite exhauſted, the glaſs at the bottom of the water was, by the ſpring of the air included in it, burſt into many pieces, not without great noiſe, and a kind of ſmoak or miſt, that appeared above the ſurface of the water.

ANOTHER glaſs of the ſame ſort had been broken after the ſame manner in another veſſel; but having afforded us no particular phænomenon, I barely mention it, to ſhew, that we made more than one trial of this kind.

THE conſequence, that will naturally reſult from the three laſt experiments, is this, that ſince barely upon the withdrawing of the preſſure of the included air (which was perhaps but very little in quantity,) the air reſiding in the immerſed bodies, did, by virtue of its ſpring, expand itſelf ſo forcibly as we have recited, and perform notable things, the air above the water muſt have exerciſed a very powerful preſſure upon the ſurface of it, ſince (ſetting aſide the weight of the water, of ſmall moment in our trials,) it muſt have been, at leaſt, equivalent to (and probably much exceeded) that force of the immerſed air, whoſe exerciſe it was able totally to hinder.

AND from hence it may be eaſily deduced, that the weight of the atmoſphere acts upon bodies under water, notwithſtanding that the interpoſed liquor is, by vaſt odds, heavier in ſpecie than air; for we have juſt now proved the preſſure of incloſed air, (which conſiſts in its ſpring,) upon bodies under water; and it is manifeſt, that the ſtrength of the ſpring of this inferior air, we make our trials with, is cauſed by the weight of the ſuperior air, which bends and compreſſes thoſe little aereal ſpringy particles, whereof our air conſiſts; ſo that the weight of the atmoſphere being equivalent to the ſpring of the inferior air, (for elſe it could not compreſs it as much as it does,) muſt lean upon the ſurface of the ſubjacent water, with a force equivalent to the ſpring of that part of it, that is contiguous to the water.

THIS experiment brings into my mind another, that I once made, which, though not properly hydroſtatical, yet relating to poſitive levity, may perhaps be not uſeleſly added on this occaſion: wherefore I ſhall here ſubjoin a tranſcript of the phænomenon, that belongs to our preſent purpoſe, as it is regiſtered ſoon after the experiment was made.

[To examine, by a visible experiment, the common doctrine, that a portion of air, by being much dilated, rarified or expanded, does acquire a new and proportionable degree of positive levity, I devised to put in practice the following way:

WE took a bladder of a moderate size, that was very fine and limber, that it might be the lighter and more easily distended. The most part of the air being squeezed out of the bladder, the neck of it was tied up very close, that no air might get out of it, nor any external get into it. This limber bladder was hung at one of the scales of a balance, whose beam had been purposely made more than ordinarily short, that the instrument, (which yet was ticklish enough) might be suspended, and capable of playing in the cavity of a great receiver, into which we conveyed it, having first carefully counterpoised the bladder with a metalline weight put into the opposite scale.

THIS done, the air was pumped out, and, as that was withdrawn, the bladder was more and more expanded by the spring of the internal air, till at length, when the receiver was well exhausted, it appeared to be quite full. Notwithstanding which great dilatation of the included air, it did not appear by the depression of the opposite scale, to be grown manifestly lighter, than it was at first. And the bladder seemed also to retain the same weight, after it had, by the air, that was let into the receiver, been compressed into its former wrinkled state.]

NEW EXPERIMENTS ABOUT THE DIFFERING PRESSURE OF HEAVY SOLIDS AND FLUIDS.

SINCE not only in vulgar spectators of physico-mechanical experiments, but even among some learned men, it has proved a great impediment to men's freely acquiescing in the doctrine founded on those phænomena, that if the atmosphere could really exercise so great a pressure, as we ascribe to it, it would unavoidably oppress and crush all the bodies exposed to it, and consequently neither other animals, nor men, would be able to move under so great a load, or subsist in spite of so forcible a compression.

THIS I readily grant to be a plausible objection; but I suppose the force of it will be taken away by the following considerations put together.

AND first, the power of pressing, that we ascribe to the air, is not a thing deduced, as too many other consequences in physick are, from doubtful suppositions or bare hypotheses, but from real and sensible experiments. And therefore since we have clear and positive proofs of the pressure of the air, though we could not explain, how men and other animals are not destroyed by it; yet we ought rather to acknowledge our ignorance in a doubtful problem, than deny what experience manifests to be a truth: as is generally practised in treating of the attractive and other powers of the loadstone, which are freely acknowledged, even by those, that confess themselves unable to explicate them; though, if experience did not satisfy us of them, they were liable to divers more considerable objections, than any, that is urged against the pressure of the air.

SECONDLY, but though it be not absolutely necessary, that we should answer the above-recited objection otherwise, than by thus declaring, that the spring of the air is not to be rejected for it; yet we will endeavour very much to lessen it, if not quite remove the difficulty, before we put an end to the discourse.

I consider then thirdly, that they, that urge the lately mentioned objection against the great pressure of the air, seem not to be aware, that we were conceived and born in places exposed to the pressure of the atmosphere, and therefore how great soever that pressure appeared to be, it ought not to crush us now, since when we were but embrios, or new-born babes, and consequently very much more weak and tender than we now are, we were able to resist it, and not only live, but grow in all dimensions in spite of it.

IF there were any place about the moon, or some other of the cœlestial globes, that some learned men fancy to be inhabited, that has no atmosphere, or equivalent fluid about it, and where yet men could be generated a-new, if one of those men should be supposed to be transported thence, and set down upon our earth, there might be made an experiment fitted for our controversy. In the mean time, I doubt, that since nature is not observed to make

make things superfluously strong, such a human body being not made to resist any weight or pressure of air, would be of so tender and compressible a make, that it would easily be crushed inwards by our atmospherical pressure. And though we cannot give an instance of this kind, yet we make trials somewhat analogous to it in our pneumatical engine. For when we place water in our receiver, and pump out the air, that was above it, there will be generated a multitude of bubbles, some of which, when the air is carefully withdrawn, will be of a strange and scarce credible bigness; these bubbles being generated where the air cannot press upon them, these dimensions are so natural to them, that if the receiver be supposed not to leak, nor other unfriendly accidents to intervene, they would (for aught we know) last a good while; since I have elsewhere shewn, that the spring of highly dilated air did continue for many months, and a bladder would for no less time continue blown and filled in our vacuum by a little air, that was left in it, when the ambient air began to be withdrawn from it. And yet the large bubbles abovementioned, when once the outward air is suffered to come in upon them, are thereby so violently compressed, that in a trice they shrink into dimensions, too small to keep them so much as visible; and if I could have succeeded in my attempt of producing such living bodies as I endeavoured (but did not expect) in our vacuum, I suppose the success would have confirmed what I have been saying.

FOURTHLY, but you will tell me, that so great a weight and pressure, as I assign the atmosphere, must needs make a man feel pain, and, if not otherwise dislocate some of the parts, must, at least, press the whole body inward.

BUT first, being accustomed to the pressure from our very birth, and even before it, so early and long an accustomance hinders us from taking notice of it; those pressures only being sensible to us, that are made so by some additional cause, which by making a new impression excites us to take notice of it. So we are not sensible of the weight of the clothes we are accustomed to wear; and so a healthy man is not sensible of the heat in his heart, because it is constant there, and the sentient parts of the heart have been still used to it, whereas that heat oftentimes has been very considerable; and when in living dissections a man puts his finger into the heart of an animal, which probably has a fainter, or at least no stronger degree of heat than a human heart, he will feel in his fingers, accustomed to the air, a manifest degree of heat, if they be but in their usual temper. 2. I have elsewhere proved by experiments, that a cubick inch of air, for instance, has as strong a spring, as suffices to enable it to resist the weight of the whole atmosphere, as far as it is exposed thereunto; for else it would be more compressed than *de facto* it is. And 3. I have also shewn, that a very little portion of air, though it will much sooner loose its spring by expansion than a greater, yet it will resist further compression as much as a greater. And 4. I have also shewn, that in the pores of the parts of animals, whether fluid or consistent, as in their blood, galls, urines, hearts, livers, &c. there are included a multitude of aereal corpuscles, as may appear by the numerous bubbles afforded by such liquors, and the swelling or expansion of the consistent parts in our exhausted receiver. 5. To this we may add, that, besides the bones, whose solidity is not questioned, a much greater part of the human body, than is wont to be imagined, does really consist of membranes and fibres, and the coalitions and contextures of these; and that these substances are, by the providence of the most wise author of things, made of a much closer and stronger texture, than those, that have not tried, will be apt to think; as I could make probable by the great force that bladders will endure, and the very great weight, that tendons of no great thickness will lift up or sustain, and by other things, that I shall not now insist on. Lastly, there is a far greater difference, than men are wont to suspect, between the effects of the pressures made upon bodies by incumbent, or otherwise applied solid weights, and those, that they suffer from heavy, but every way ambient fluids; as will appear by the experiments to be mentioned by and by.

FROM the particulars contained in these considerations, we may be assisted to shew, why it is not necessary, that the pressure of the atmosphere, though as great as we suppose it, should oppress and crush the bodies of men, that live under it: for, the solidity of the bones, and the strong texture of the membranes and fibres, and the spring of the aereal particles, that abound in the softer, as well as in the fluid parts of bodies, is equivalent to the pressure of as much of the atmosphere, as can exercise its pressure against them, and makes the frame of a human body so firm, that it may well resist the pressure of the outward air, without having any part violently dislocated, whilst the external pressure is exercised but by the air, which being but an invironing fluid, presses it equally (as to sense) on every side. And because our bodies have been produced in the atmosphere, and from our very birth exposed, without intermission, to the pressure of it; our continual accustomance to this pressure, and the firmness of their structure, keep us from being sensible of the weight or pressure. And that it was not impertinent for me to mention the firmness of the frame of our bodies on this occasion, I shall manifest by an instance, that will upon another account also be proper for this place.

WE know, that multitudes of men have had occasion to pass over high mountains; and besides, that I have been myself upon the *Alps* and *Appennines*, I have enquired of travellers, that have visited the Asian and American mountains, and some, that have been upon the top of the pick of *Tenerif* itself: but though divers of them took notice of a great difference in the air at the top and bottom, as to some other quality, as coldness and thinness;

ness; yet I never met with, nor heard of any, that took notice of a difference, as to the weight of air he sustained, or that complained, that when he was come down to the foot of the mountain, he felt any greater compression from the air, than at the top. And yet the experiments made, as well by others as by ourselves, sufficiently witness, that on more elevated parts of the earth, which have a less height of the atmosphere incumbent on them, the weight and pressure of the air is not so great as below. And on very high mountains, it is not unlikely, that this difference may be very considerable, since, when the Torricellian experiment was made near *Clermont* in *France*, upon the *Puy de Domme*, (which is none of the highest mountains in the world, being found, by the ingenious makers of that observation, to be but about 500 fathoms,) they found the difference of the mercury, at the top and bottom, to amount to about three inches: and consequently, if the trial had been made with water instead of quicksilver, the difference would have been about three foot and a half in the perpendicular height of the water. And it is very probable, that in much higher mountains, the difference of the mercurial cylinders height, at the top and bottom, may be much greater; and at the bottom of some very deep well or mineral groove, which may, without improbability, be supposed to be placed at, or near the foot of one of these mountains, if we conceive the baroscope to be let down, the variation of the height of the mercurial cylinder may be yet much more considerable; and yet we find not, that the diggers in the deepest mines, in mountainous countries, are sensible of being leaned on or compressed by any unusual weight. But not here to build on any thing but matter of fact, it appears by the newly-named observation, that, when a man was at the bottom of the hill, he had as much greater weight of air leaning upon his head, than he had at the top, as was equal to the height of an imaginary vessel full of water, which having his head for basis, were three foot and a half high: which is so considerable a weight, as could not but have been, not only sensible, but very troublesome and uneasy to support. And what has been said of the gravity of a pail of water, that leaned on his head, may be proportionably applied to his shoulders, arms, &c.

WHENCE I think I may infer, that the reason, why such a weight was not felt by the man it compressed, was not, that the air, that pressed him, was not considerable, but that the pressure was exercised after the uniform manner of fluid bodies.

AND this may suffice to shew, that there is no necessity, that the compression of the atmosphere should make it impossible to live in it. But because 'tis observed, that those, that dive to great depths under water, are not oppressed by the great weight of the incumbent water, and the cause of this strange phænomenon is not so easy to be assigned, and therefore has been made one of the two grand arguments, whereon the non-gravitation of water in water, and air in air, has been, and still remains founded: I shall here offer something *ex abundanti* towards the solution of that noble and difficult problem.

AND first, that what is observed by the divers, does not evince, that water does not weigh in water, I have elsewhere * proved by such reasons and experiments, as had the good fortune to convince eminently learned men, that were sufficiently prepossessed with the vulgar opinion: and in the same treatise I have given a clear account, why a bucket full of water is not felt considerably heavy, whilst it is under water, in comparison of what it is, whilst it is drawn up into the air; which is the other phænomenon, that I freshly intimated the common opinion to be founded on.

NEXT, I do not think it strange, that that follows not, which it is objected should follow from our hypothesis; namely, that a diver should be violently depressed to the bottom of the water, by the weight of so great a pillar of the sea as is placed perpendicularly over his body. For if we imagine a plane so to cut the sea-water, as to pass by the diver's body; then as that part of the plane, on which his body leans, will be pressed by it, together with the water, that is perpendicularly incumbent on it; so all the other parts of the same plane will be pressed by equally tall, pillars of water perpendicularly incumbent on them; and consequently, if the man's body were just equiponderant to an equal bulk of water, it and the water, that leans on it; would be sustained by the pressure of the collateral water incumbent on the other parts of the same plane (as may be easily understood by what I have elsewhere † said.) And therefore there is no reason, why the divers bodies should be more forcibly depressed than its depression is resisted. It is true, that this body will sink, but that is because it is not only, as we lately supposed it, æquiponderant to an equal bulk of water, but heavier than that. But then, since the water, by its gravity and resistance, takes off as much of the weight of the diver's body, whilst that is immersed, as a quantity of water equal to it, would weigh in the air, the subsiding of the human body by its own weight ought to be but slow, because that being not in specie much heavier than water, it can sink but by virtue of the surplusage of weight, that it has above water. And, in effect, I have been informed by swimmers, that in the sea, whose water, by reason of the saltness, is specifically heavier than the common water, they could hardly dive when they had a mind; the salt-water did so much support them. And having, because I had no conveniencies to make trials upon the parts of human bodies, examined the weight of parts of other animals in air and water, I found the overplus of the weight of the animal substance above an equal bulk of water to be but very small. And this may suffice to take off the wonder, why, though water may be admitted

* See the Hydrostatical Paradoxes.

† See appendixes to the Hydrostatical Paradoxes.

mitted to gravitate in water, yet divers are not depressed by that, which leans upon them; the endeavour, they use to keep themselves from sinking, by striking the resisting water with their arms and legs, easily compensating their weak tendency downwards, which the small surplusage of gravity above-mentioned gives them.

BUT it seems to me far more difficult to render a reason, why those, that are a hundred foot beneath the surface of the sea, are not crushed inwards, especially in their chests and abdomens, or at least so compressed as to endure a very great pain.

To clear up or lessen this difficulty, I have two things to offer.

1. I confess, that I am not entirely satisfied about the matter of fact; for I do not yet know, whether it fares alike with the divers in all depths under water: for, according to the answers I obtained from persons, that had been, one of them at the coral-fishing in the *Streights*, and the other at the pearl-fishing near *Manar*, I do not find, that the divers are wont to descend to the greatest depths of the sea, which if they did, perhaps they would find a notable difference.

AND in small, or but moderate depths, those, that dive without engines, usually make such haste, or are so confounded, or have their minds so intent upon their work, that they take not notice of such lesser alterations, as else they might observe, especially they being persons void of curiosity and skill to make such observations. Which I the rather mention, because having met with a learned physician, that living by the sea-side in a hot climate, delighted himself much in diving; and enquiring of him, whether he felt no compression, when he passed out of the air into the water, he answered me, that when he dived nimbly as others use to do, he took not notice of it, but when he let himself sink leisurely into the water, he was sensible of an unusual pressure against his thorax, which he several times observed.

A man, that gets his living by fetching up goods out of wrecked ships, complained to me, that if with his diving-bell he went very deep into the sea, and made some stay there, he found himself much incommodated; which, though he imputed to the coldness of the water, yet by the symptoms he related, I was inclined to suspect, that the pressure of it upon the genus nervosum might have an interest in the troublesome effect. And I have been assured by an eminent virtuoso of my acquaintance, that he was lately informed by a person, whose profession it is to fetch up things from the bottom of the sea by the help of a diving-bell, that several times, when he descended to a great depth under the surface of the water, he was so compressed by it, that the blood was squeezed out at his nose and eyes; which relation seems to favour our conjecture, and would much more confirm it, if I were sure, that the effect was no way caused by some fermentation, or other commotion in he blood itself, occasioned by the great density, or other alterations of the air he breathed in and out, or by some other operation of the ambient medium distinguishable from the compression of the water, though perhaps conjoined with it.

AND on this occasion I remember, that questioning an engineer, who had made use of an engine to go under water, quite differing from the diving-bell; he answered me, that when he came to a considerable depth, he found the pressure so great against the leathern case, wherein he descended, and by that means against his belly and thorax, that he feared it would have spoiled him, which forced him to make haste up again. But this observation, to have much built upon it, should be further enquired into.

THESE things, and not these only, make me wish, that what is felt by those, that dive to great depths, and stay at them, might be more heedfully observed by intelligent men, that being fully informed, what is true in point of fact, we may the better and more chearfully indagate the reasons.

IN the mean while, taking things as they are thought to appear, I shall propose two things towards the solution of our difficulty; namely, the firmness of the structure of a human body, and the uniformity of the pressure made by fluids.

OF the first of these I shall add but little to what has been already said, where I spoke of the resistance made by our bodies to the compression of the atmosphere; only shall here take notice, that whereas the membranes are very thin parts, and therefore seem unfit to make any great resistance; we have tried, that if a piece of fine bladder were fastened to the orifice of a brass-pipe, of about an inch in diameter, we could not, by drawing the air from beneath it, make the weight of the atmosphere break the bladder, though the weight were perhaps equivalent to an erected cylinder of water, of the wideness of the orifice, and about thirty foot high, and were indeed such, that divers men, that laid their hands on the orifice, when the air was pumped out from beneath, complained, that they were not able to lift off their hands again, till some of the air was re-admitted.

BUT the main thing I shall propose, towards the solving of the difficulty we are considering, is the uniformity, wherewith fluid bodies press upon the solid ones, that are placed in them. And because I remember not to have met with experiments purposely made to shew, how this sort of pressure is more easy to be resisted, than that of solids against solids, I shall subjoin the following trials.

EXPERIMENT I.

IN the short cylinder of brass above-mentioned, we put a fine bladder tied so close at the neck, that none of the air (whereof it was about half full) could probably get out. Which we did, to the end, that the hen-egg, we were to bed in it, might lie soft, and have its sides almost covered with the limber and flaccid bladder and contained air: this done, we covered the remaining part of the egg with another bladder, that nothing, that was hard, might come to bear immediately upon the shell: then we put the wooden plug into the cylinder, and a weight upon the plug, which is to be done very slowly and warily, lest the quick descent of the weight should make the plug break the egg it leans on. Lastly, the cylinder thus fitted, being covered with a large receiver, and the air being drawn out, that air, which was tied up in the bladders, by degrees expanded itself so strongly, as to lift up the plug and the incumbent weight to a pretty height, and keep it there, till the external air was re-admitted.

Now since it will be readily granted, and appears by divers experiments, elsewhere related, that the air in such cases expands itself vigorously every way, it appears by the recited trial, that it pressed against the egg with the same force, that it pressed proportionably against the bottom of the plug, and that force was more than sufficient to lift up the weight, which (together with the plug) amounted to about thirty pound, and yet the egg being taken out, appeared perfectly whole and no way harmed; whereas, upon the same egg, if I mistake not, or at least another of the same kind, laying warily a while after small weights, one upon another, the egg was crushed to pieces by about four pound weight. This experiment, though it seemed considerable to those that saw it, and may prevent an objection, for which reason I here mention it; yet will appear in no way strange to them, that consider, that the weight of the atmosphere, which the egg supported, before it was put into the cylinder, was more than æquivalent to such a pressure of the air, as may suffice to lift up the plug: wherefore I thought fit to make further trials of a differing nature.

EXPERIMENT II.

WE took a glass bubble of about an inch and half in diameter, which we caused to be blown at the flame of a lamp, that it might be far more thin and easy to break, than the thinnest vials, that are wont to be blown in the glasser's furnaces. This bubble we included between bladders, as we did the egg in the former experiment; and then having warily put the plug into the cylinder, so as it might press upon the bladder that invironed the glass, we leisurely put the weights upon the plug, till they, together with the plug, amounted to thirty pound, or more, which being removed, the plug was taken out, and the glass-bubble, though it were extraordinarily thin, perhaps no thinner than fine white paper, was taken out whole.

EXPERIMENT III.

BUT lest the great resistance of so thin a glass, which yet was not hermetically sealed, should be ascribed to the sphericalness of its figure, we employed, instead of it, the shell of an egg, whence by a hole, made at one end of it, the yolk and white had been taken out. This empty and imperfectly closed shell we handled, as we did the glass-bubble in the former experiment; and, notwithstanding the great leaden weight, that leaned by the intervention of the plug upon the soft body, that invironed it, it was taken out, not only uncrushed together, but, for aught we could perceive, without the least crack.

EXPERIMENT IV.

AND to shew, that what we observed about the nature of the compression of fluid bodies will hold as well in water as air, though it seemed difficult to make the trial with the accommodations we then had, we thought upon the following expedient.

INTO a limber bladder, almost full of water, we put a hen egg, and tying the neck very strait, that nothing might get in or out, we so placed the bladder in the brass cylinder, that the egg might not be immediately touched by any thing, that was hard: then putting the plug into the cylinder, we warily and leisurely heaped upon it flat-bottomed weights of lead conveniently shaped, till they amounted (if both I and another misremember not) to about seventy-five pound; notwithstanding all which, the egg was taken out sound and uncracked; and probably might have supported a much greater pressure, if we had been furnished with more weights of a commodious figure to heap upon it.

IF we compare with this what was noted at the close of the first experiment, about the breaking of an egg with four pound weight, when no fluid body was interposed, it will be obvious to conclude, how great a difference there is between the resistance, that a body may make to the pressure of solid bodies, that bear hard against some parts, and not against others; and its resistance to others, that compress it uniformly, or in all places alike. For though it be denied, and that, I think, upon very insufficient grounds, that bodies under water are pressed by the incumbent water, because, as it is pretended, the elements gravitate not in their proper place; yet this objection cannot be pretended to take place in our last experiment, where the main thing, that leaned upon the water, which surrounded the egg, being not a pillar of homogeneous water, but a great and solid weight of lead, the included egg must, by the intervention of the water, have been compressed. Nor were eggs the only bodies we endeavoured to crush after this manner, the trial having been also made upon a substance more soft, and of a very irregular shape.

To apply this now to divers, when they are at a moderate depth under water; it seems not improbable, that the structure of their bodies should be robust enough, not to be violated by the pressure of the incumbent, and otherwise ambient water. For we have seen by the former experiment, and especially by the last recited

cited, that a body, easy to be broken inwards by an incumbent solid weight, will remain entire, and unaltered in point of figure, under a very much greater weight, that compresses it after the manner of an ambient fluid. And though it would seem to many, that even in our supposition, the thorax being, as they think it, a kind of empty space in the body, the ribs and muscles ought, by the weight of the water, to be crushed into the great cavity intercepted between them; yet it is to be considered on the other side, that the air contained in the chest, especially when its spring is increased by those accidental causes, that may take place, when men are deep under water, particularly the preternatural heat, which the want of the usual respiration is apt to produce, will very much help the chest to resist the pressure, as they will easily grant, that have tried the resistance, that air makes, to be considerably compressed under water, the difficulty of farther compressing it still encreasing, as in springs it ought to do, the more it is compressed. And I further observe, that the structure of the thorax is much more firm, than men are wont to suppose; as appears by the very great solid weights, that some men do, for gain, or to shew their strength, suffer to be laid on their breasts, without receiving any mischief thereby. And if I should admit, that at great depths the water had some little compressive operation upon the chest; yet that can be no other than the pressing the parts a little inwards, and that the structure of the thorax itself, fitted by nature for constriction and dilatation (as may appear in vehement takings in and blowings out of the air), may admit with small inconvenience. To which purpose I recall to mind, what I lately mentioned concerning the physician, that found his thorax somewhat compressed when he leisurely dived; as also what I have * elsewhere delivered concerning a tad-pole, which swimming in water, that was strongly compressed by an external force, seemed thorough the glass, that contained the water, to be somewhat lessened in bulk, and yet not killed, nor sensibly crushed, notwithstanding its great tenderness. And if there were parts of a human body, that were of a texture too weak, and too disproportionate to the rest, I think it possible, that this compression inwards might be great enough to be very sensible to the divers. For having purposely enquired of a certain man, whose trade it was to fetch up goods out of ships cast away, by the help of a diving instrument, he told me, that when he was at a considerable depth under water, as about ten or twelve fathoms, he found, suitably to my conjecture, so great a pressure against the drums or thin membranes of his ears, which were not sufficiently counterpressed from within, as put him to a great deal of pain, till he had found some contrivances to lessen the inconvenience. Nor was this man the only diver, that has complained of this troublesome pressure, which seems to argue, that, at least at great depths under water, the firmness of the structure of a man's body does concur with the uniformity of the fluid's pressure, to keep him from being hurt by the incumbent, and otherwise ambient air.

BUT I shall now say no more of the problem about divers, since (besides that the matter of fact is not yet, in my opinion, accurately enough stated and determined) the true solution of it is not necessary to give a reason, why the weight of the air, a fluid so much lighter than water, should not oppress nor crush the bodies of animals; though what has been already said, about the resistance of bodies under water, may serve very much to confirm the reasons I proposed, why we, that live in the atmosphere, are not sensibly compressed, much less oppressed by its weight.

* In the Appendix to Hydrost. Paradox.

SOME

SOME OBSERVATIONS ABOUT SHINING FLESH, Both of VEAL and of PULLET, AND THAT Without any ſenſible PUTREFACTION in thoſe Bodies.

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YESTERDAY, when I was about to go to bed, an amanuenſis of mine, accuſtomed to make obſervations, informed me, that one of the ſervants of the houſe, going upon ſome occaſion into the larder, was frighted by ſomething of luminous, that ſhe ſaw (notwithſtanding the darkneſs of the place,) where the meat had been hung up before. Whereupon, ſuſpending for a-while my going to reſt, I preſently ſent for the meat into my chamber, and cauſed it to be placed in a corner of the room capable of being made conſiderably dark, and then I plainly ſaw, both with wonder and delight, that the joint of meat did, in divers places, ſhine like rotten wood or ſtinking fiſh; which was ſo uncommon a ſight, that I had preſently thoughts of inviting you to be a ſharer in the pleaſure of it. But the late hour of the night did not only make me fear to give you too unſeaſonable a trouble, but being joined with a great cold, I had got that day by making trial of a new teleſcope, you ſaw, in a windy place, I durſt not ſit up long enough to make all the trials, that I thought of, and judged the occaſion worthy of. But yet, becauſe I effectually reſolved to employ the little time I had to ſpare, in making ſuch obſervations and trials, as the accommodations I could procure at ſo inconvenient an hour would enable me, I ſhall here give you a brief account of the chief circumſtances and phænomena, that I had opportunity to take notice of.

1. THEN I muſt tell you, that the ſubject, we diſcourſe of, was a neck of veal, which, as I learned by enquiry, had been bought of a country-butcher on the tueſday preceding.

2. IN this one piece of meat I reckoned diſtinctly above twenty ſeveral places, that did all of them ſhine, though not all of them alike, ſome them of doing it but very faintly.

3. THE bigneſs of theſe lucid parts was differing enough, ſome of them being as big as the nail of a man's middle finger, ſome few bigger, and moſt of them leſs. Nor were their figures at all more uniform, ſome being inclined to a round, others almoſt oval, but the greateſt part of them very irregalarly ſhaped.

4. THE parts, that ſhone moſt, which it was not ſo eaſy to determine in the dark, were ſome griſly or ſoft parts of the bones, where the butcher's cleaver had paſſed; but theſe were not the only parts, that were luminous; for by drawing to and fro the medulla ſpinalis, we found, that a part of that alſo did not ſhine ill: and I perceived one place in a tendon to afford ſome light; and laſtly, three or four ſpots in the fleſhy parts, at a good diſtance from the bones, were plainly diſcovered by their own light, though that were fainter than in the parts above mentioned.

5. WHEN all theſe lucid parts were ſurveyed together, they made a very ſplendid ſhew; but it was not ſo eaſy, becauſe of the moiſtneſs and groſſneſs of the lump of matter, to examine the degree of their luminouſneſs, as it is to eſtimate that of gloworms, which being ſmall and dry bodies, may be conveniently laid in a book, and made to move from one letter or word to another. But by good fortune having by me the curious tranſactions of this month, I was able ſo to apply that flexible paper to ſome of the more reſplendent ſpots, that I could plainly read divers conſecutive letters of the title.

6. THE colour, that accompanied the light, was not in all the ſame, but in thoſe, which ſhone livelieſt, it ſeemed to have ſuch a fine greeniſh blue, as I have divers times obſerved in the tails of gloworms.

7. BUT notwithſtanding the vividneſs of this light, I could not, by the touch, diſcern the leaſt degree of heat in the parts, whence it proceeded; and having put ſome marks on one or two of the more ſhining places, that I might

might know them again, when brought to the light, I applied a sealed weather-glass, furnished with tincted spirit of wine, for a pretty while, and could not satisfy myself, that the shining parts did at all sensibly warm the liquor: but the thermoscope, though good in its kind, being not fitted for such nice experiments, I did not build much upon that trial.

8. NOTWITHSTANDING the great number of lucid parts in this neck of veal, yet neither I, nor any of those, that were about me, could perceive, by the smell, the least degree of stink, whence to infer any putrefaction; the meat being judged very fresh, and well-conditioned, and fit to be dressed.

9. THE floor of the larder, where this meat was kept, is almost a story lower than the level of the street, and it is divided from the kitchen but by a partition of boards, and is furnished but with one window, which is not great, and looks towards the street, which lies northward from it.

10. THE wind, as far as we could observe it, was then at south-west, and blustering enough. The air, by the sealed thermoscope, appeared hot for the season. The moon was passed its last quarter. The mercury in the barometer stood at $29\frac{3}{16}$ inches.

11. WE cut off, with a knife one of the luminous parts, which proved to be a tender bone, and being of about the thickness of a half crown piece, appeared to shine on both sides, though not equally; and that part of the bone, whence this had been cut off, continued joined to the rest of the neck of veal, and was seen to shine, but nothing near so vividly as the part we had taken off, did before.

12. To try, whether I could obtain any juice, or moist substance from this, as I have several times done from the tails of glow-worms; I rubbed some of the softer and more lucid parts, (which I caused to be purposely cut off) as dexterously as I could, upon my hand, but I did not at all perceive any luminous moisture was thereby imparted; though the flesh seemed, by that operation, to have lost some of its light.

13. I caused also a piece of shining flesh to be compressed betwixt two pieces of glass, to try, how well the contexture of it would resist that external force; but I did not find the light to be thereby extinguished, during the short time I could allot to the experiment.

14. BUT supposing, that high rectified spirit of wine might so alter the contexture of the body it permeated, as to destroy its faculty of shining, I put a luminous piece of veal into a chrystalline vial, and pouring on it a little pure spirit of wine, that would have burned all away, after I had shaken them together, I laid by the glass, and in about a quarter of an hour, or less, I found, that the light was vanished.

15. BUT water would not so easily quench our seeming fires; for having put one of them into a China cup, and almost filled it with cold water, the light did not only appear, perhaps undiminished, through that liquor, but above an hour after was vigorous enough not to be eclipsed by being looked upon at no great distance from a burning candle, that was none of the smallest; and probably the light would have been seen much longer, if we could have afforded to watch out its duration.

16. WHILST these things were doing, I caused the pneumatical engine to be prepared in a room without fire, (that the experiment might be tried in a greater degree of darkness;) and having conveyed one of the largest luminous pieces into a small receiver, we caused the candles to be put out, and the pump to be plied in the dark; but the diminution of light, after the pump seemed to have been employed for a competent while, appeared so inconsiderable, (whether because our eyes had leisure to be fitted to that dark place, or for what other cause soever,) that I began to suspect, that the instrument, having been managed in the dark, had leaked all the while. Wherefore causing the lights to be brought in, and a mercurial gage to be put into the receiver, when we were sure, that this glass was well cemented on to the engine, the candles being removed, the pump was set a work again; and then opening my eyes, which I had kept closed against the light of the candles, I could perceive, upon the gradual withdrawing of the air, a discernible and gradual lessening of the light; which yet was never brought quite to disappear (as I long since told you, the light of rotten wood and glowworms had done) or to be so near vanishing as one would have expected; though, upon the bringing in of the candles again, it appeared by the gage, that the pump had been diligently applied. But the room being once again darkned; by the hasty increase of light, that had disclosed itself in the veal, upon this letting in of the air to the exhausted receiver, it appeared more manifestly than before, that the decrement, though but slowly made, had been considerable. This trial we once more repeated with a not unlike success; which, though it convinced us, that the luminous matter of our included body was more vigorous, or tenacious, than that of most other shining bodies, yet it left us some doubts, that the light would have been much more impaired, if not quite made to vanish, if the subject of it could have been kept long enough in our exhausted receiver: but the unseasonable time of the night reducing me at length to go to bed, I could not stay to prosecute this, or any other trial.

17. ONLY, whilst I was undressing, this further observation occurred, that supposing there might be, in the same larder, more joints of the same veal than one, ennobled with this shining faculty, it was found, that a leg of veal, which was caused to be brought into my chamber, had some shining places in it; though they were but very few, and faint, in comparison of those, that were conspicuous in the above mentioned neck.

18. WHAT further phænomena this morning might have afforded me, I cannot tell, having been hastily called up, before day, for a neice, that I am very justly, and exceedingly concerned for; who was thought to be upon the

the point of death, and whose almost gasping condition had too much affected and employed me, to leave me any time for philosophical entertainments, that require a calm, if not a pleased mind. Only this I took notice of, because the observation could not cost me a minute of an hour, that whilst they were bringing me candles for to rise by, I looked upon a clean vial, that I had laid upon the bed by me, after a piece of our luminous veal had been included in it, and found it to shine vividly at that time, which was between four and five of the clock this morning; since when I have made no one observation, or trial.

POSTSCRIPT.

19. NEAR two days after I had made the fore-mentioned observations, those horrid symptoms of my neice's disease, that had so much alarmed the physicians, and me, being, through God's goodness, considerably abated, I began to resume the thoughts of our shining veal; and though having in the hurry I was in forgotten to take any order about it, I found it was already dispósed of; yet the piece, I lately mentioned to have been included in a vial, being preserved in it, I looked upon it the third day (inclusively) after we had first observed the meat, it was cut off from, to be luminous; and I found it to shine in the dark as vigorously as ever. The fourth day its light was also conspicuous; so that I was able, in a dark corner of the room, to shew it, even in the day time, to three or four very ingenious physicians, all of them, save one, members of the Royal Society; and I presume, I need not remind you, that the following night, I invited you to be a spectator of it, though before that time the light had begun to decay, and the offensive smell to grow somewhat strong: which seems to argue, that the disposition, upon whose account our veal was luminous, may very well consist both with its being, and not being, in a state of putrefaction, and consequently, is not likely to be derived meerly from the one, or the other. The fifth day, in the morning, looking upon it when I awaked, and before the curtains were opened, it seemed to shine better, than it had done the day preceding. The same night also, it was manifest enough, though not vivid, in the dark. When I awaked, the sixth day in the morning, after the sun was risen, I could, within the curtains, perceive a glimmering light. But the seventh day, which was yesterday, I could not, late at night, discern any light at all.

You saw too much in what a condition I was, when you did me the favour to visit me, to expect, that I should presume to entertain you with any speculations about the cause of these unusual apparitions of light. It is true indeed, that in some notes, I formerly mentioned to you, I endeavoured to make it probable, that whether light depend upon a particular kind of impulse, propagated through a transparent medium, or upon a diffusion of extremely little parts from the luminous body, or upon the action of some other corporeal agent; whatever the efficient be, the effect is produced in a mechanical way. But though I had these papers by me, yet, to determine what peculiar kind of motions, or other operations, nature really employed in the production of a light, which seemed not clearly, by what I shall presently note, referable either to the particular and settled constitution of the animals, whose flesh shined, (as in our glow-worms, and some American flies,) or to that intestine and unusual motion of the parts, that causes, or accompanies putrefaction in rotten wood, or fishes; since, upon the first and liveliest appearance of the light, there was not any, (at least, that could be taken notice of by the senses:) To determine this, I say, it seemed to me so difficult a task, that I shall willingly leave the solution of such abstruse phænomena, as some of ours, unattempted; especially since I may, God permitting, make an historical mention of them the day after to-morrow, at the meeting of the Royal Society; where, I doubt not, much more, and more to the purpose, will be said, and considered, than I have vanity to think myself capable of offering. Only, for the prevention of some needless conjectures, to which, without this previous advertisement, one might upon plausible grounds indulge, I shall, in the mean while add, and conclude with one observation more, which may possibly take off our thoughts from striving to deduce the shining of our veal from the peculiar nourishment, or constitution, or properties, of that individual calf, whose flesh, &c. was luminous. For, having several nights sent purposely into the larder, to observe, whether any veal, since brought thither, or any other meat, did afford any light, a negative answer was always brought me back; save at one time, which happened to be within less than forty-eight hours of that, at which the luminousness of the veal had been first taken notice of; for at this time there was, in the same larder, a conspicuous light seen in a pullet, that hung up there, which having caused to be brought up into a darkened place in my chamber, in the night-time, I perceived four or five luminous places; which were not indeed near so large as those of the veal, but were little less vived than they. All of these I took notice to be either upon, or near the rump; and that, which appeared most like a spark of fire, shone at the very tip of that part. Yet was not this fowl mortified, nor at all ill scented, but so fresh, that the next day I found it very good meat. But whether this may reasonably lead to a suspicion, that the peculiar constitution of the air in that larder, and at that time, may as well deserve to be taken into consideration, as the peculiar nature of the animals, whose flesh did shine, is a question, that I, who have scarce time to name it, must not presume to do, any more than name. And therefore, as soon as I have begged your pardon for this tedious, though hasty scribble, I shall, without ceremony, subscribe myself, &c.

A

NEW EXPERIMENT,

CONCERNING

An Effect of the varying Weight of the ATMOSPHERE upon some Bodies in the Water; suggesting a Conjecture, that the very Alterations of the Air, in point of Weight, may have considerable Operations, even upon Men's Sickness or Health.

First published in the PHILOSOPHICAL TRANSACTIONS, N°. XCI. p. 5156, for *February* 24, 16$\frac{72}{73}$.

THOUGH many things have, by ingenious men, been already observed, as to the power and operations of the atmosphere's weight upon liquors, that are exposed to it in Torricellian tubes, or other vessels, closed at one end, and near the top, either empty or unfilled with any visible body; yet men seem not to have much enquired, what effects the very variation of this weight of the atmosphere may have on the liquors which it presses, in other vessels than such as baroscopes and pumps. And yet when I remember, how much of air appears by our engine to be invisibly harboured in the pores, not only of water, but of the blood, serum, urine, gall, and other juices of the human body; and that (as I have elsewhere experimentally shewn) the pressure of the atmosphere, and the spring of the air, work upon liquors, and on bodies immersed in those liquors, as well as upon solid ones, immediately exposed to the air, I am prone to suspect, that the very alterations of the atmosphere, in point of weight, may, in some cases, have some not contemptible operations, even upon men's sickness or health; as when the ambient air, for instance, grows suddenly very much lighter than it was before, or than it was wont to be, the spirituous and aereal particles, that are plentifully harboured in the mass of blood, will naturally swell that liquor, and so may distend the greater vessels, and not a little alter the celerity and manner of the circulation of the blood by the capillary arteries and veins. By which alteration, that divers changes may happen in the body, will not seem improbable to those, that know in general, how important a thing the manner of the circulation of the blood may be there, though, as to its particular effects, I leave them to the speculation of physicians; and shall only add, that to keep this conjecture of mine (for I propose it as no other) from seeming as groundless as extravagant, I will annex an experiment, that you will not perhaps dislike, just as I find it registered among some of my loose papers.

I caused to be blown, at the flame of a lamp, three small round glass bubbles, about the bigness of hazel-nuts, and furnished each of them with a short and slender stem, by whose means they were so nicely poised in water, that a very small change of weight would make them either emerge, if they but lightly leaned on the bottom of the vessel, or sink, if they floated on the top of the water.

THIS being done at a time, when the atmosphere was of a convenient weight, (and such a season is not ordinarily difficult to be chosen, within some reasonable time, to him, that wants neither attention, nor a good baroscope) I put them in a wide-mouthed glass, furnished with common water, and leaving them in a quiet place, where yet they were frequently in my eye, and were suffered to continue many weeks, or some months, I observed, as I expected, that sometimes they would be at the top of the water, and remain there for divers days, or perhaps weeks; and sometimes would fall to the bottom, and after having continued there for some time, longer or shorter, they would again emerge. And though sometimes, especially if I removed the vessel, that contained them, to a southern window, they would rise to the top, or fall to the bottom of the water, according as the air was hot or cold; yet it was not difficult to distinguish these motions from those produced by the varying gravity of the atmosphere. For when the beams of the sun, or heat of the ambient air, by rarifying the air included in the bubbles, made that air drive out some of the water, and consequently made the whole bubble, consisting of glass, air, and water, somewhat lighter than a bulk of water equal to it, though the bubble did necessarily swim as long as the included air was thus rarified, yet when the absence

of

of the ſun, or any other cauſe, made the air loſe its adventitious warmth, there would enſue a condenſation of the air again, and thereupon an intruſion of more water (to ſucceed the air) into the glaſs, and conſequently a ſinking of the bubble; and this would commonly happen at night, if it did not happen ſooner. But when it was upon the account of the varying weight of the atmoſphere, that the bubbles either roſe or fell, it appeared by the baroſcope, that the atmoſphere was ſo heavy, or ſo light, that they ought to do ſo. Inſomuch, that I divers times predicted, whether I ſhould find the mercury in the baroſcope high or low, by obſerving the ſituation and poſture of the bubbles; and conſulting that inſtrument, it verified my conjectures. And though, whilſt the atmoſphere was not too conſiderably either light or heavy, the changes of the air, as to heat or cold, would, as I was ſaying, place the bubbles ſometimes at the top, and ſometimes at the bottom of the water, within the compaſs of a day; yet if the atmoſphere were either very heavy, or very light, the bubbles would continue at the bottom, or at the top of the water for many days together, in caſe the atmoſphere did not in all that time change its gravity. And I remember, that I did, for curioſity's ſake, when the quickſilver was high in the baroſcope, put the glaſs two or three days in a ſouth window about noon, and for a good while after, and that in ſun-ſhining weather; and yet even then the bubbles did not emerge, though it appeared by a good ſealed weather-glaſs, which I kept in the ſame window, that the ambient air was much warmer than at other times, when I had obſerved the bubbles to keep at the top of the water.

N. B. 1. It being very difficult to poiſe ſeveral bubbles preciſely, as well one as another, I thought it not ſtrange, that all the three bubbles did not conſtantly (though for the moſt part they did) riſe and fall together; but ſometimes two of them, and now and then, though ſeldom, one alone would ſink or emerge, when the change of the weight of the atmoſphere was not conſiderable enough to operate ſenſibly upon the reſt, and of ſuch inſtances, I have had opportunity to obſerve one or two within theſe laſt three days; and therefore it is not amiſs, to poiſe a greater number of bubbles together, that after trial made of all, the fitteſt may be choſen. Which advertiſement will appear the more proper, becauſe of what is to be added in the following note.

2. I have obſerved it ſometimes to happen, that a bubble, that floated when it was firſt poiſed, would, after a while, ſubſide, without any manifeſt cauſe; or if it were made to ſink by ſuch a cauſe, it would continue at the bottom of the water, though that cauſe were removed: which difficult phænomenon ſeeming to depend upon a kind of imbibition made of certain particles of an aereal nature, by the water, the conſideration of it belongs to another place, not to this; where it may ſuffice, that the experiment did ſometimes actually anſwer expectation, as that above-related did; wherein my main drift is to ſhew, that ſince, as the atmoſphere was heavier or lighter, it is capable to work upon bodies under water, ſo as to procure their ſinking, or their emerſion; the air, though a fluid a thouſand times lighter, muſt lean or preſs upon the water itſelf, by whoſe intervention it produces theſe effects; which confirms what I elſewhere teach, that the atmoſphere is incumbent, as a heavy body, upon the terraqueous globe.

3. Besides the other circumſtances, upon whoſe account this experiment may fail of ſucceſs, the ſeaſon of the year, wherein it is tried, may, for aught I know, be conſiderable. For which reaſon I ſhall here add this advertiſement, that I chuſe, but do not confine myſelf, to make my trials about the beginning of the ſpring, as a time, wherein notable alterations of the air, as well as to weight, as to other things, are the likelieſt to be frequent,

ESSAYS

OF THE

STRANGE SUBTILTY,

GREAT EFFICACY,

DETERMINATE NATURE

OF

EFFLUVIUMS.

To which are annexed

NEW EXPERIMENTS to make FIRE and FLAME Ponderable:

TOGETHER WITH

A Difcovery of the PERVIOUSNESS of GLASS.

An ADVERTISEMENT to the READER.

IT is hoped, the reader will not think it ftrange, not to meet with, in the following papers, a more clofe and uniform contexture of the paffages, that make them up, if he be feafonably informed of the rife and occafion of penning them, which was this. The author having many years ago written an effay about an experiment he made of nitre, by whofe phænomena he endeavoured to exemplify fome parts of the corpufcular philofophy, efpecially the production of qualities; he afterwards threw together divers occuring thoughts and experiments, which he fuppofed might be employed by way of notes, to prove or illuftrate thofe doctrines, and efpecially thofe, that concerned the qualities of bodies; and among thefe obferving thofe, that are called occult, to be fubjects uncultivated enough, (at leaft, in the way, that feemed to him proper,) he propofed to handle them more largely than moft of the reft; and in order to that defign he judged it almoft neceffary to premife fome confiderations and experimental collections about the nature and power of effluviums, about the pores of bodies and figures of corpufcles, and about the efficacy of fuch local motions, as are wont either to be judged very faint, or to be paffed by unheeded. For he had often looked upon thefe three doctrines, of effluvia, of pores and figures, and of unheeded motions, as the three principal keys to the philofophy of occult qualities. But having hereupon made fuch collections, as upon review appeared too large to pafs for notes on fo fhort a text, he was induced to draw them * into the form (they now appear in) of effays; but he would not put himfelf to the trouble of doing it, with care to keep them from retaining much of their firft want of exact method and connection. Nor was the author folicitous to finifh them up, in regard that his other ftudies and occafions made him perceive, that in what he had defigned about occult qualities, he had cut himfelf out more work, than probably he fhould, during many years, have opportunity to fet upon in earneft, and complete. And in this condition thefe papers lay for divers years, (as is well known to feveral, that faw them, or even tranfcribed fome of them,) and might have continued to do fo, if the author had not been

* And fome that were publifhed anno 1669, under The title of the Atmofpheres of Confiftent Bodies.

been induced to let them come abroad, partly by considering, that though the subjects, (however he handled them) were as well important as curious, yet he did not find himself prevented by others in what he had to publish about them; and partly by the references he had made to them in some other papers, that he had promised his friends, wherein several things here delivered are vouched, and others supposed. And because the notes concerning the porosity of greater bodies, and the figurations of minute particles, together with the paper about unregarded motions, having been long laid aside among other neglected papers, were some of them missing, and others so misused, that they could not easily be made ready to accompany those, that now come abroad; the author, that he might keep this book from having its dimensions too disproportionate, was content to add to the thickness of it, by subjoining one of those little tracts, that lay by him, concerning flame, because of the affinity betwixt the preceding doctrine about effluviums in general, and experiments, that shew, in particular, the subtilty and efficacy of those of fire and flame. And though to that tract itself there belong another, designed to examine, whether the matter of what we call the sun-beams, may be brought to be ponderable; yet supposing this hitherto cold and wet summer to be like to be as unfriendly to the trials to be made with burning-glasses, as of late years some other summers have proved, he was easily prevailed with, not to make those experiments, that were ready, wait any longer for those, that probably will not in a short time be so; especially since those, that now come abroad, have no dependency upon the others.

OF THE STRANGE SUBTILTY OF EFFLUVIUMS.

CHAP. I.

WHETHER we suppose, with the antient and modern atomists, that all sensible bodies are made up of corpuscles, not only insensible, but indivisible; or whether we think with the Cartesians, and (as many of that party teach us) with *Aristotle*, that matter, like quantity, is indefinitely, if not infinitely divisible; it will be consonant enough to either doctrine, that the effluvia of bodies may consist of particles extremely small. For if we embrace the opinion of *Aristotle*, or *Des-Cartes*, there is no stop to be put to the subdivision of matter into fragments still lesser and lesser. And though the Epicurean hypothesis admit not of such an interminate division of matter, but will have it stop at certain solid corpuscles, which, for their not being further divisible, are called atoms, ἄτομοι; yet the assertors of these do justly think themselves injured, when they are charged with taking the motes, or small dust, that fly up and down in the sun-beams, for their atoms; since, according to these philosophers, one of those little grains of dust, that is visible only, when it plays in the sun-beams, may be composed of a multitude of atoms, and exceed many thousands of them in bulk. This the learned *Gassendus* in his notes on *Diogenes Laertius* makes probable by the instance of a small mite, which, though scarce distinctly discernible by the naked eye, unless when it is in motion, does yet, in a good microscope, appear to be a compleat animal, furnished with all necessary parts; which I can easily allow, having often in cheese-mites very distinctly seen the hair growing upon their legs. And to the former instance, I might add, what I have elsewhere told you of a sort of animals far lesser than cheese-mites themselves, namely those, that may be oftentimes seen in vinegar. But what has been already said, may suffice for my present purpose, which is only to shew, that the wonderful minuteness I shall hereafter ascribe to effluvia, is not inconsistent with the most received theories of naturalists. For otherwise, in this essay, the proofs I mean to employ, must be taken, not *à priori*, but *à posteriori*. And the experiments and observations, I shall employ on this occasion, will be chiefly those, that are referrible to one of the following heads.

1. THE strange extensibility of some bodies, whilst their parts yet remain tangible.

2. THE multitude of visible corpuscles, that may be afforded by a small portion of matter.

3. THE smallness of the pores, at which the effluvia of some bodies will get in.

4. THE small decrement of bulk, or weight, that a body may suffer by parting with great store of effluvia.

5. THE

5. The great quantity of space, that may be filled, as to sense, by a small quantity of matter, when rarified or dispersed.

But though to these distinct heads I shall design distinct chapters, yet you must not expect to find the instances solicitously marshalled, but set down in the order they occurred to me; such a liberty being allowable in a paper, where I pretend not to write treatises, but * notes.

CHAP. II.

Among many things, that are gross enough to be the objects of our touch, and to be managed with our hands, there are some, that may help us to conceive a wonderful minuteness in the small parts they consist of.

I do not remember what *Cardan*, and since him, another writer, have delivered about the thinness and slenderness, to which gold may be brought. And therefore, without positively assenting to, or absolutely rejecting what may have been said about it by others, I shall only borrow, on this occasion, what I have mentioned on † another, upon my own observation; namely, that silver, whose ductility and tractility are very much inferior to those of gold, was, by my procuring, drawn out to so slender a wire, that, when we measured it, which was somewhat troublesome to do, with a long and accurate measure, we found, that eight yards of it did not yet fully counterpoise one grain: so that we might add a grain more without making the scale, wherein it was put, manifestly preponderate, notwithstanding the tenderness of the balance. Whence we concluded, that a single grain of this wire amounted to twenty-seven foot, that is, three hundred and twenty-four inches. And since experience informs us, that half an English inch can, by diagonal lines, be divided into one hundred parts, great enough to be easily distinguished, even for mechanical uses, it follows, that a grain of this wire-drawn silver, may be divided into sixty-four thousand eight hundred parts, and yet each of these will be a true metalline, though but slender and short, cylinder, which we may very well conceive to consist yet of a multitude of minuter parts. For though I could procure no gilt wire near so slender as our newly-mentioned silver-wire; yet I tried, that some, which I had by me, was small enough to make one grain of it fourteen foot long: at which rate, an ounce did amount to a full mile, consisting of one thousand geometrical paces, of five foot a-piece, and seven hundred and twenty foot over and above. And if now it be permitted to suppose the wire to have been, as in probability it might have been, further drawn out to the same slenderness with the above-mentioned silver wire, the instance will still be far more considerable; for in this case, each of those little cylinders, of which sixty-four thousand eight hundred go to the making of one grain, will have a superficial area, which, except at the basis, will be covered with a case of gold; which is not only separable from it by a mental operation, but perhaps also by a chemical one. For I remember, that from very slender gilt wire, though I could get none so slender as this of meer silver, I did more than once, for curiosity's sake, so get out the silver, that the golden films, whilst they were in a liquor that plumped them up, seemed to be solid wires of gold; but when the liquor was withdrawn, they appeared, as indeed they were, to be oblong, and extremely thin and double membranes of that metal, which, with an instrument, that had been delicate enough, might have been ripped open, and displayed, and been made capable of further divisions and sub-divisions. To this I shall add, that each of the little silver cylinders I lately spake of, must not only have its little area, but its solidity; and yet I saw no reason to doubt, but that it might be very possible, if the artificer had been so skilful and willing, as I wished, to have drawn the same quantity of metal to a much greater length, since even an animal substance is capable of being brought to a slenderness much surpassing that of our wire, supposing the truth of an observation of very credible persons, critical enough in making experiments, which, for a confirmation, and an improvement of our present argument, I shall now subjoin. An ingenious gentlewoman of my acquaintance, wife to a learned physician, taking much pleasure to keep silk-worms, had once the curiosity to draw out one of the oval cases, (which the silk-worms spins, not, as it is commonly thought, out of its belly, but out of the mouth, whence I have taken pleasure to draw it out with my fingers,) into all the silken wire it was made up of, which, to the great wonder, as well of her husband, as herself, who both informed me of it, appeared to be, by measure, a great deal above three hundred yards, and yet weighed but two grains and a half: so that each cylindrically shaped grain of silk may well be reckoned to be at least one hundred and twenty yards long.

Another way, I remember, I also employed to help men, by the extensibility of gold, the better to conceive the minuteness of the parts of solid bodies.

We took six beaten leaves of gold, which we measured one by one with a ruler purposely made for nice experiments, and found them to have a greater equality in dimensions, and to be nearer true squares, than could be well expected: the side of the square was in each of them exactly enough three inches and $\frac{2}{8}$, or $\frac{1}{4}$, which number being reduced to a decimal fraction, viz. $\frac{3125}{1000}$, and multiplied by itself, affords $\frac{1015625}{100000}$ for the area, or superficial content of each square leaf: and this multiplied by 6, the number of the leaves amounts to $\frac{6313750}{100000}$ square inches, for the area of the six leaves. These, being carefully weighed in a pair of tender scales, amounted all of them to one grain and a quarter: and so one grain of this foliated gold

* This Essay was designed to be but a part of the author's notes upon his Essay about Salt-petre.

† In a paper about Improbable Truths.

gold was extended to somewhat above fifty inches; which differed but about a fifth part from an experiment of the like nature, that I remember I made many years ago in a pair of exact scales; and so small a difference may very well be imputed to that of the pains and diligence of the gold-beaters, who do not always work with equal strength and skill, nor upon equally fine and ductile gold.

Now if we recal to mind what I was lately saying, of the actual divisibility of an inch into an hundred sensible parts, and suppose an inch so divided, to be applied to each side of a square inch of the leaf-gold newly mentioned, it is manifest, that by subtle parallel lines, drawn between all the opposite points, a grain of gold must be divisible into five hundred thousand little squares, very minute, indeed, but yet discernible by a sufficient sharp-sighted eye. And if we suppose an inch to be divided into two hundred parts, as I lately told you it was in a ruler I employ, then, according to the newly-recited way, the number of the squares, into which a single grain is capable of being divided, will amount to no less than two millions.

There is yet another way, that I took to shew, that the extensibility, and consequently the divisibleness of gold, is probably far more wonderful, than by the lately mentioned trial it appears.

For this purpose, I went to a great refiner, whom I used to deal with for purified gold and silver, and enquired of him, how many grains of leaf-gold he was wont to allow to an ounce of silver, when it was to be drawn into gilt wire as slender as an hair. To this he answered me, that eight grains was the proportion he allowed to an ounce, when the wire was to be well gilt; but if it were to be more slightly gilt, six grains would serve the turn. And to the same purpose I was answered by a skilful wire-drawer. And I remember, that desiring the refiner to shew me an ingot of silver, as he did at first gild it; he shewed me a good fair cylindrical bar, whereon the leaf-gold, that overlaid the surface, did not appear to be, by odds, so thick as fine Venetian paper; and yet comparing this with gilt wire, which I also desired to see, the wire appeared to be the better gilt of the two; possibly, because the gold, in passing through the various holes, was by the sides of them not only extended, but polished, which made it look more vividly than the unpolished leaves, that gilded the ingot.

So that, if we suppose an ounce of the gilt wire formerly-mentioned, to have been gilt with six grains of leaf-gold, it will appear, by an easy calculation, that at this rate one ounce of gold, employed on gilding wire of that slenderness, would reach between ninety and an hundred miles. But if now we further suppose, as we lately did, that the slender silver wire, mentioned at the beginning of this chapter, were gilt; though we should allow it to have (because of its exceeding slenderness,) not, as the former, six grains, but eight grains of leaf-gold to an ounce of silver, it must be acknowledged, that an hollow cylinder, or sheath of gold, weighing but eight grains, may be so stretched, that it will reach to no less than sixty times as much in weight of silver wire, as it covers: [I said sixty times, for so often is eight contained in four hundred and eighty, the number of grains in an ounce;] and consequently, a grain of that wire having been found to be twenty-seven foot long, the ounce of gold would reach to seven hundred seventy-seven thousand six hundred foot, that is, an hundred fifty-five miles and above a half. And if we yet further suppose this superficial, or hollow cylinder of gold, to be slit all along, and cut into as slender lists or thongs as may be, we must not deny, that gold may be made to reach to a stupendious length. But we need not this last supposition, to make what preceded it an amazing thing: which yet, though it be indeed stupendous, and seem incredible, ought not at all to be judged impossible; being no more than what, upon the suppositions and observations above laid down, does evidently follow.

CHAP. III.

After what has been said of the minuteness of tangible objects, it will be proper to subjoin some instances of the smallness of such as yet continue visible. But, in regard these corpuscles are singly too little to have any common measure applied to any of them, we must make an estimate of their minuteness, by the number of those, into which a small portion or fragment of matter may be actually divided, the multitude of these being afforded by so inconsiderable a quantity of matter, sufficiently declaring, that each of them, in particular, must be marvelously little.

Among the instances, where the smallness of bodies may be deduced from what is immediately the object of sight, it may not be unfit to take notice of the evaporation of water, which though it be granted to consist of gross particles, in comparison of the spirituous and odoriferous ones of divers other liquors, as of pure spirit of wine, essential oils of spices, &c. yet to shew, that a small quantity of it may be dispersed into a multitude of manifestly visible corpuscles, I thought upon, and more than once tried, the rarefaction of it into vaporus by help of an æolipile, wherein, when I made the experiment the last time, I took the pains to register the event as follows.

We put an ounce of common water into an æolipile, and having put it upon a chafing-dish of coals, we observed the time, when the streams of vapours began to be manifest. This stream was, for a good while, impetuous enough, as appeared by the noise it made, which would be much increased, if we applied to it, at a convenient distance, a kindled brand, in which it would blow up the fire very vehemently. The stream continued about a quarter of an hour, sixteen minutes or better, but afterwards the wind had pauses and gusts

for

for two or three minutes before it quite ceaſed. And by reaſon of the ſhape of the æolipile, (which being framed chiefly for other purpoſes, was not ſo convenient for this) a great portion of the vapours condenſed in the upper part of it, and fell down in drops; ſo that ſuppoſing, that they alſo had come out in the form of wind, and the blaſt had not been intermitted toward the latter end, I gueſſed it might have continued uninterruptedly eighteen or twenty minutes. Note, that applying a meaſure to the ſmoke, that came out very viſible in a form almoſt conical, where it ſeemed to have an inch or more in diameter, it was diſtant from the hole of the æolipile about twenty inches; and five or ſix inches beyond that, though it were ſpread ſo much, as to have four or five inches in diameter, yet the not uniform, but ſtill-cohering clouds, which was the form, wherein the vapours appeared, were manifeſt and conſpicuous.

After the rarefaction of water, when it is turned into vapours, we may conſider that of fewel, when it is turned into flame; to which purpoſe, I might here propoſe ſeveral trials, as well of our own as others, about the prodigious expanſion of ſome inflammable bodies upon their being actually turned into flame. But in this place to mention all theſe, would perhaps too much intrench upon another paper; and therefore I ſhall here propoſe to your conſideration but one inſtance, and that very eaſy to be tried; of which I find this account among my *adverſaria*.

Having oftentimes burnt ſpirit of wine, and alſo oil in glaſs lamps, that for certain uſes were ſo made, that the ſurface of the liquor was ſtill circular, it was obvious to obſerve, how little the liquor would ſubſide by the waſte, that was made of it, in about half a quarter of an hour. And yet if we conſider, that the naked eye, after ſome exerciſe, may, as I have often tried, diſcern the motions of a pendulum, that ſwings faſt enough to divide a ſingle minute of an hour into two hundred and forty parts, and conſequently half a quarter of an hour into one thouſand eight hundred parts; if we alſo conſider, into how many parts of the time employed by a pendulum, the vibrations, ſlow enough to be diſcernible by the eye, may be mentally ſubdivided; and if we further conſider, that, without intermiſſion, the oil is preyed upon by an actual flame, and the particles of it do continually furniſh a conſiderable ſtream of ſhining matter, that with a ſtrange celerity is always flying away; we may very well conceive, that thoſe parts of flame, into which the oil is turned, are ſtupendouſly minute, ſince, though the waſting of the oil is in its progreſs too ſlow to be perceived by the eye, yet it is undoubted, that there is a continual decrement of the depth of the oil, the phyſical ſurfaces whereof are continually and ſucceſſively attenuated and turned into flame; and the ſtrange ſubtilty of the corpuſcles of flame would be much the ſtronglier argued, if we ſhould ſuppoſe, that inſtead of common oil the flame were nouriſhed by a fewel ſo much more compact and durable, as is that inflammable ſubſtance made of a metalline body, of whoſe laſtingneſs I have elſewhere made particular mention *, after having taught the way of preparing it.

Having in a pair of tender ſcales carefully weighed out half a grain of good gunpowder, we laid it on a piece of tile, and whelmed over it a veſſel of glaſs (elſewhere deſcribed, and often mentioned) with a braſs plate to cover the upper orifice of it. Then having fired the gunpowder, we obſerved, that the ſmoke of it did opacate, and, as to ſenſe, ſo fill the whole cavity of the glaſs, though its baſis were eight inches, its perpendicular height above twenty inches, and its figure far more capacious, than if it were conical; and this ſmoke, not containing itſelf within the veſſel, iſſued out at two or three little intervals, that were purpoſely left between the orifice of the veſſel and the plate, that lay upon it. This cover we then removed, that we might obſerve how long the ſmoke would continue to aſcend; which we found it would do for about half a quarter of an hour, and during near half that time, viz. the three firſt minutes, the continually aſcending ſmoke ſeemed to be, at its going out, of the ſame diameter with the orifice at which it iſſued; and it would aſcend ſometimes a foot, ſometimes half a yard, ſometimes two foot, or more, into the air, before it would diſperſe and vaniſh into it.

Now if we conſider, that the cavity of this round orifice was two inches in diameter, how many myriads of viſible corpuſcles may we eaſily conceive thronged out at ſo large an outlet, in the time above-mentioned, ſince they were continually thruſting one another forwards? and into ſo many viſible particles of ſmoke muſt we admit, that the half grain of powder was ſhattered, beſide thoſe multitudes, which, having been turned into actual flame, may probably be ſuppoſed to have ſuffered a comminution, that made them become inviſible. And though I ſhall not attempt ſo hopeleſs a work, as to compute the number of theſe ſmall particles; yet to make an eſtimate, whereby it would appear to be exceeding great, I thought fit to conſider, how great the proportion was between the ſpaces, that to the eye appeared all full of ſmoke, and the dimenſions of the powder, that was reſolved into that ſmoke. Cauſing then the glaſs to be filled with common water, we found it to contain above two and twenty pints of that liquor, and cauſing one of thoſe meaſures to be weighed, it was found to weigh ſo near a pound (of ſixteen ounces) that the computation of the whole water amounted to at leaſt one hundred and ſixty thouſand grains, and conſequently three hundred and twenty thouſand half grains. To which if we add, that this gunpowder would readily ſink to the bottom of water, as being (by reaſon of the ſalt-petre and brimſtone, that make up at leaſt ſix parts of ſeven of it) in

* In ſome papers about Flame.

ſpecie heavier than it, and in likelihood twice as heavy (for it is not eaſy to determine it exactly) we may probably gueſs the ſpace to which the ſmoke reached, to exceed five hundred thouſand times that, which contained the unfired powder; and this, though the ſmoke, being confined in the veſſel, was thereby kept from diffuſing itſelf ſo far, as by its ſtreaming out it ſeemed likely, that it would have done.

To theſe inſtances from inanimate bodies I ſhall ſubjoin one more taken from animals. Whereas then men have with reaſon wondered, that ſo ſmall a body as a cheeſe-mite, which by the naked eye is oftentimes not to be taken notice of, unleſs it move, (if even then it be ſo) ſhould, by the microſcope, appear to be an animal furniſhed with all neceſſary parts; whereas this, I ſay, has given juſt occaſion to conclude, that the corpuſcles, that make up the parts of ſo ſmall an animal, muſt themſelves be extremely ſmall; I think the argument may be much improved by the following conſideration. Thoſe, that have had the curioſity to open from time to time eggs, that are ſat upon by a hatching hen, cannot but have obſerved, how ſmall a proportion, in reference to the bulk of the whole egg, the chick bears; when that, which the excellent *Harvey* calls *punctum ſaliens*, diſcloſes the motion of the heart, and the colour of the blood; and that even about the ſeventh or eighth day, the whole chick now viſibly formed bears no great proportion to the whole egg, which is to ſupply it with aliment, not only for its nouriſhment, but ſpeedy growth for many days after.

To apply this now to the matter in hand; having ſeveral times obſerved, and ſhewn to others, that cheeſe-mites themſelves are generated of eggs, if we conceive, that in theſe eggs, as in ordinary ones, the animal at its firſt formation bears but a ſmall proportion to the bulk of the whole egg, the remaining part being to ſuffice for the food and growth of the embryo probably for a pretty while; ſince, if an ingenious perſon, that I deſired to watch them, did not miſinform me, they uſed to be about ten or twelve days in hatching; this whole egg itſelf will be allowed to be but little, in reference to the mite it came from, how extremely and unimaginibly minute may we ſuppoſe thoſe parts to be, that make up the alimental liquors, and even the ſpirits, that paſſing through the nerves, or analogous parts, ſerve to move the limbs and ſenſories of but, as it were, the model of ſuch an animal, as, when it reſts, would not, perhaps, itſelf, to the naked eye be ſo much as viſible; and in which we may preſume the nobler ſort of ſtabler parts to be of an amazing ſlenderneſs, if we conſider, that, though in other hairy animals, the optick, or ſome other of the larger nerves do, I know not how many times, in thickneſs and circuit, ſurpaſs a hair of the ſame animal; yet in a cheeſe-mite, though none of the largeſt of thoſe creatures, we have divers times manifeſtly ſeen, as is before intimated, ſingle hairs, that grow upon the legs.

ANOTHER way there is, that I employed to give men cauſe to think, that the inviſible effluvia of bodies, that wander through the air, may be ſtrangely minute; and this was, by ſhewing, how ſmall a fragment of matter may be reſolved into particles minute enough to aſſociate themſelves in ſuch numbers with a fluid ſo much more denſe than air, as water is, as to impart a determinate colour to the whole liquor. What I did with cochineal in proſecution of this deſign, my experiments about colours may inform you; but I ſhall now relate the ſucceſs of an attempt made another way, for which perhaps ſome of your friends, the chemiſts, will thank me; though I was not ſolicitous to carry on the experiment very far with gold, not becauſe I judged that leſs diviſible into a number of coloured particles, but becauſe I found, as I expected, that the paleneſs of the native colour of the gold may make it in the end leſs conſpicuous, though, if I had then had by me a menſtruum, as I ſometimes had, that would diſſolve gold blood-red, perhaps the experiment with gold would have ſurpaſſed that, which it is now time I ſhould begin to relate, as ſoon as I have hinted to you by the way, that, for variety's ſake, I made a trial with copper calcined *per ſe*, that I might not be accuſed of having omitted to employ a metal, whoſe body chemiſts ſuppoſe to be much opened by calcination. And though the event were notable, even in compariſon of that of the experiment made with cochineal, yet my conjectures inclined me much to prefer the way deſcribed in the following account.

WE carefully weighed out in a pair of tender ſcales one grain of copper not calcined, but barely filed; and becauſe, as we made choice of this metal for its yielding in moſt menſtruums a blue, which is a deep and conſpicuous colour, we alſo choſe to make a ſolution, not in aqua fortis, or aqua regis, but the ſpirit of ſal armoniack (as that is an urinous ſpirit,) having found by former trials, that this menſtruum would give a far deeper ſolution than either of the others. This lovely liquor, of which we uſed a good proportion, that all the copper might be thoroughly diſſolved, we put into a tall cylindrical glaſs of about four inches in diameter, and by degrees poured to it of diſtilled water, which is more proper in this caſe than common water, which has oftentimes an inconvenient ſaltiſhneſs, till we had almoſt filled the glaſs, and ſaw the colour grow ſomewhat pale, without being too dilute to be manifeſt; and then we warily poured this liquour into a conical glaſs, that it might be the more eaſy to fill the veſſel ſeveral times to the ſame height. This conical glaſs we filled to a certain mark four times conſecutively, weighing it, and the liquor too, as often in a pair of excellent ſcales purpoſely made for ſtatical experiments, and which, though ſtrong enough to weigh ſome pounds in each ſcale, would, when not too much loaden, turn with about one grain. Theſe ſeveral weights of the glaſs, together with the contained liquor, we added together, and then carefully weighing

weighing the empty glaſs again, we deducted four times its weight from the above-mentioned ſum, and thereby found the weight of the liquor alone, to be that, which reduced to grains, amounted to 28,534, ſo that a grain of copper, which is not full half ſo heavy in ſpecie as fine gold, communicated a tincture to 28,534 times its weight.

BUT now, if you pleaſe to take notice, that the ſcope of my experiment was to ſhew, into what a number of parts one grain of copper might be divided; you will allow me to conſider, as I did, that this multitude of parts muſt be eſtimated by the proportion, not ſo much in weight as in bulk, of the tinging metal to the tinged liquor; and conſequently, ſince that divers hydroſtatical trials have informed me, that the weight of copper to the weight of water of the ſame bulk is *proximè* as nine to one, a grain-weight of copper is in bigneſs but the ninth part of as much water as weighs a grain; and ſo the formerly-mentioned number of the grains of water muſt be multiplied by nine, to give us the proportion between the tinging and tinged bodies, that is, that a ſingle grain of copper, gave a blueneſs to above 256,806 parts of limpid water, each of them as big as it. Which, though it may ſeem ſtupendous, and ſcarce credible; yet I thought fit to proſecute the experiment ſomewhat farther, by pouring all the liquor out of the tall cylindrical glaſs into another clean veſſel, whence filling the conical glaſs twice, and emptying it as often into the ſame cylindrical glaſs, the third time I filled the conical glaſs with colourleſs diſtilled water, and pouring that alſo into the cylindrical glaſs, we found the mixed liquor to have yet a manifeſt, though but a pale blueneſs. And laſtly, throwing away what was in the cylindrical glaſs, we poured into it, out of the ſame conical glaſs, equal parts of diſtilled colourleſs water, and of the tincted liquor we had formerly ſet a-part in the clean veſſel; and found, that though the colour were very faint and dilute, yet an attentive eye could eaſily diſcern it to be blueiſh; and ſo it was judged by an intelligent ſtranger, that was brought in to look upon it, and was deſired to diſcover of what colour he thought it to be. Whereby it appears, that one grain of copper was able to impart a colour to above double the quantity of water above-mentioned.

THIS experiment I have allowed myſelf to be the longer and more particular in relating, both becauſe I know not, that any ſuch has been hitherto either made or attempted, and becauſe it will probably gratify your chemiſts, that love to have the tinctures of metals believed very diffuſive; and becauſe, if circumſtances were not added, it would ſeem to you as well incredible, as perhaps it does ſeem ſtupendous, that a portion of matter ſhould be able to impart a conſpicuous colour to above 256,806 times its bulk of water, and a manifeſt tincture to above 385,200, (for ſo it did, when the proportion of the tinged part to the whole mixture, made of it, and the untinged part, was as 2 to 31,) and a faint, but yet diſcernible and diſtinguiſhable colour, to above five hundred and thirteen thouſand ſix hundred and twenty times its bulk of water.

CHAP. IV.

IT were eaſy for me (*Pyroph.*) to give you ſeveral inſtances, to ſhew, that the effluvia of liquors may get in at the pores of bodies, that are reputed of a cloſe texture; but I ſhall at preſent forbear to mention ſuch examples, not only becauſe they belong to another place*, where I take notice of them, but becauſe many ſuch would not ſeem ſo remarkable, nor be ſo conſiderable to our preſent purpoſe, as a few taken from bodies, that are not fluid.

And firſt, it is delivered by writers of good credit, that ſeveral perſons, (for the experiment does not hold in all) by barely holding for ſome time dried cantharides in their hands, have been put to much pain at the neck of the bladder, and have had ſome other parts miniſtering to the ſecretion of urine ſenſibly injured. That this is true, I am induced to believe, by what I have elſewhere related to you of the unwelcome experiment I had of the effect of cantharides applied but outwardly to my neck, and that unknown to me, upon the urinary paſſages; and that theſe operations are due to material effluxes, which, to get into the maſs of blood, muſt paſs through the pores of the ſkin, you will not, I preſume, put me to prove.

SCALIGER Exercit. 186. relates, that in *Gaſcony*, his country, there are ſpiders of that virulency, that if a man treads upon them to cruſh them, their poiſon will paſs through the very ſoles of his ſhoes. Which ſtory, notwithſtanding the reputation of the author, I ſhould perhaps have left unmentioned, becauſe of a much ſtranger about ſpiders, which he re-relates in the ſame ſection, but that I met with one that is analogous in the diligent *Piſo*'s late hiſtory of *Braſil*; where, having ſpoken of another venemous fiſh of that country, and the antidotes he had ſucceſsfully uſed to cure the hurts it inflicts, he proceeds to that fiſh the natives call *Amoreatim*, of one kind whereof, called by the Portugals *Peize Sola*, his words are theſe; *Quæ mira ſanè efficacia non ſolum manum vel leviſſimo attactu, ſed & pedem, licet optimè calceatum, piſcatoris incautè piſciculum conterentis, paralyſi & ſtupore afficit, inſtar torpedinis Europææ, ſed minus durabili.* Lib. 5. cap. 14.

WHAT I ſhall ere long have occaſion to tell you of the power of the *Torpedo*, and ſome other animals, to affect the hand and arm of him that ſtrikes them, ſeems applicable to the matter under conſideration: for, though their affecting the ſtriker at a diſtance may very well be aſcribed to the ſtupefactive, or other venemous exhalations, that expire (and perhaps are as it were darted) from the animal irritated by the ſtroke, and are breathed in together with the air they infect; yet their benumbing, or otherwiſe affecting the arm that ſtruck

* A Diſcourſe of pores of bodies, and figures of corpuſcles.

ſtruck them, rather than any other part, ſeems to argue, that the poiſonous ſteams get in at the pores of the ſkin of the limb, and ſo ſtupify, or otherwiſe injure, the nervous and muſculous parts of it.

OTHER examples belonging to this ſection, may be referred hither from divers other places in theſe papers about occult qualities, and therefore I ſhall only add here, that moſt remarkable proof, That ſome emanations, even of ſolid bodies, may be ſubtil enough to get through the pores, even of the cloſeſt bodies; which is afforded us by the effluvia of the loadſtone, which are by magnetical writers ſaid to penetrate, without reſiſtance, all kind of bodies. And though I have not tried this in all ſorts, yet having tried it metals themſelves, I am apt to think, the general rule admits of very few exceptions, eſpecially, if that can be fully made out, which is affirmed about the perviouſneſs of glaſs to the effluxions of the loadſtone. For, not only glaſs is generally reputed to be as cloſe a body as any is, but (which weighs more with me) I have by trials purpoſely made, had occaſion to admire the cloſeneſs of very thin pieces of glaſs. But the reaſon, why I juſt now expreſſed myſelf with an If, was, becauſe I was not entirely ſatisfied with the proof wont to be acquieſced in, of the perviouſneſs of glaſs; namely, that in dials and ſea-compaſſes, that are covered with plates of glaſs, the needle may be readily moved to and fro by a loadſtone held over it. For theſe plates being commonly but faſtened on with wax, or at beſt with cement, a ſceptick may pretend, that the magnetical effluvia paſs not through the glaſs, but through that much more pervious matter, that is employed to ſecure the commiſſures, only from the acceſs of the air. To put then the matter paſt doubt, I cauſed ſome needles to be hermetically ſealed up in glaſs-pipes, which being laid upon the ſurface of water (whereon, by reaſon of the bigneſs of the cavities, they would lightly float,) the included needles did not only readily feel the virtue of an externally applied loadſtone, (though but a weak one) but complied with it ſo well, that I could eaſily, by the help of the needle, lead, without touching it, the whole pipe, this was ſhut up in, to what part of the ſurface of the water I pleaſed. And I alſo found, that by applying a better loadſtone to the upper part of a ſealed pipe, and a needle in it, I could make the needle leap up from the lower part, as near to the loadſtone, as the interpoſed glaſs would give it leave.

BUT I thought it would be more conſiderable, to manifeſt, that the magnetical effluvia, even of ſuch a dull body, as the globe of the earth, would alſo penetrate glaſs. And though this ſeem difficult to be tried, becauſe no ordinary loadſtone, nor any iron touched by it, was to be employed to work on the included iron; yet I thought fit to attempt it after this manner. I took a cylindrical piece of iron, of about the bigneſs of ones little finger, and between half a foot and a foot long, (for I had formerly obſerved, that the quantity of unexcited iron furthers its operation upon excited needles,) and having hermetically ſealed it up in a glaſs-pipe but very little longer than it, I ſuppoſed, that if I held it in a perpendicular poſture, the magnetical effluvia of the earth, penetrating the glaſs, would make the lower extream of the iron anſwerable to the north pole; and therefore having applied this to the point of the needle in a dial, or ſea-compaſs, that looked toward the north, (for authors mean not all the ſame thing by the northern pole of a needle, or loadſtone,) I preſumed it would, according to the laws magnetical, (elſewhere mentioned) drive it away, which accordingly it did. And having for farther trial inverted the included iron, (ſo that the end, which was formerly the lowermoſt, was now the uppermoſt) and held it in a perpendicular poſture, juſt under the ſame point of the needle, that extream of the iron-rod, which before had driven away this point, being by this inverſion become, in a manner, a ſouth-pole, did (according to the ſame laws) attract it: by which ſudden change of poles, merely upon the change of ſituation, it alſo appeared, that the iron owed its virtue only to the magnetiſm of the earth, not that of another loadſtone, which would not have been thus eaſily alterable. And this experiment I the more particularly relate, becauſe this is not the only place, where I have occaſion to make uſe of it.

CHAP. V.

ANOTHER proof of the great ſubtilty of effluviums may be taken from the ſmall decrement of weight or bulk, that a body may ſuffer by parting with great ſtore of ſuch emanations.

THAT bodies, which infuſed in liquors impregnate them with new qualities ſuitable to thoſe of the immerſed bodies, do ſo by imparting to them ſomewhat of their own ſubſtance, will, I preſume, be readily granted by thoſe, that conceive not, how one body ſhould communicate to another a ſolitary and naked quality, unaccompanied by any thing corporeal to ſupport and convey it. But I would not have you think, *Pyrophilus*, that the only matter of fact I have to countenance this notion, is that experiment, which has convinced divers chemiſts and phyſicians, otherwiſe not friends to the corpuſcular philoſophy, that medicines may operate without any conſumption of themſelves. For though divers of theſe, ſome of them learned men, have confidently written, that glaſs of antimony, and crocus metallorum, being either of them infuſed in a great proportion of wine, will make it vomitive; and if that liquor be poured off, and new be poured on, every new portion of ſuch liquor will be impregnated with the ſame virtue, and this though the liquor be changed a thouſand times, and yet the antimonial glaſs or crocus will continue the ſame, as well in weight as virtue; and though thence ſome of them, eſpecially chemiſts, argue, that ſome metals work without imparting any thing ſubſtantial, but only, as *Helmont* ſpeaks of ſome of his arcana, by irradiation: yet, I confeſs, I have

have some doubts, whether the experiment have been competently tried, and shall not fully acquiesce in what has been said, till some skilful experimenter deliver it upon his own trial, and acquaint us too, with what instruments, and what circumspection he made it. For besides, that the ingeniousest physicians I have questioned about it, acknowledged the taste, and sometimes the colour of the wine, to be altered by the infused mineral, I could not acquiesce in the affirmation of an ordinary chemist, or apothecary, or even physician, if he should barely aver, that he had weighed an antimonial medicine before it was put to infuse, and after the infusion ended, and observed no decrement of weight. For I have had too much experience (as I elsewhere mention) of the difficulty of making exact statical trials; not to know, that such scales, as are wont to be employed by chemists and apothecaries in weighing drugs, are by no means fit to make trials with the nicety, which that I am speaking of requires: it being easy, even with the better sort of such unaccurate scales, especially if they be not suspended from some fixed thing, but held with the hand, to mistake half a grain, or a grain; and perhaps a greater quantity, and at least more, than by divers of the experiments of this essay appears necessary to be spent upon the impregnating of a considerable proportion of liquor, with corporeal effluxions. Besides that if, when the beaten crocus, or glass, be taken out of the wine to be weighed again, the experimenter be not cautious enough to make allowance for the liquor, that will adhere to the medicament, it is plain, that he may take notice of no decrement of weight, though there may be really effluviums of the mineral amounting to several grains, imbibed by the liquor. And though he be aware of this, and dry the powder, yet it is not so easy, even for a skilful man, to be sure, that none of the more viscous particles of the liquor stick to the mineral, and being sensible upon the balance, though not to the eye or hand, repair the recess of those emetick corpuscles, that diffused themselves into the menstruum. And the sense of these difficulties put me upon the attempting to make so noble an experiment with excellent scales, and the care that it deserves: but, after a long trial, an unlucky accident frustrated at last my endeavours. But though, till competent relators give us an account of this matter upon their own trial, and repeat the infusion very much oftner, than, for aught I find, any man has yet done, I must not acquiesce in all, that is said of the impregnation of wine, or other liquors by antimonial glass and crocus metallorum; yet, that after divers repeated infusions, the mineral substance should not be sensibly diminished in bulk or virtue, may well suffice to make this instance, though not the only or chief, that may be brought for our purpose, yet a pertinent one to it. For, that there is a powerful emetick quality imparted to the liquor, is manifest by experience; and that the mineral does not impart this virtue, as it were, by irradiation, but by substantial effluxion, seems to me very probable; not only because I conceive not, how this can be done otherwise, but because, as it is noted above, the wine does oftentimes change colour by being kept a competent time upon the mineral, as if it drew thence a tincture; and even when it is not discoloured, I think it unsafe to conclude, that the menstruum has not wrought upon it. For I have kept good spirit of vinegar, for a considerable time, upon finely powdered glass of antimony made *per se*, without finding the spirit to be all tinged, though it is known, that antimonial glass is soluble in spirit of vinegar, as mine afterwards appeared to be, by a longer digestion in the same liquor. But there may be a great number of minute particles dissolved in the menstruum before they be numerous enough to change the colour of it. And with this agrees very well what is observed, that though too great a quantity of the prepared antimony be put into the liquor, yet it will not be thereby made too strongly emetick. For the wine, being a menstruum, will, like other menstruums, be impregnated but to a certain measure, without dissolving the overplus of the matter, that is put into it; and Mars, which is a harder and heavier body than glass of antimony, is it self in part soluble in good Rhenish or other white wine, (and that in no long time,) and sometimes even in water.

I do not therefore reject the emetick infusion, as unfit to have a place in this chapter, but till the experiment have been a little more accurately made, I think it inferiour, as to our purpose, to some of the instances to be met with in the next chapter, and perhaps also to that mentioned by *Helmont*, and tried by more than one of my acquaintance, concerning the virtue of killing worms, that mercury imparts to the water or wine, wherein it has been long enough infused, or else for a while decocted. Though quicksilver given in substance is commended as an effectual medicine against worms, not only by many professed * spagyrists, but by divers † methodists of good note. And though some other things, chemical and philosophical, keep me from being of their opinion, who think, that in this case the mercury impregnates the liquor, as it were, by irradiation, rather than in a corporeal manner; yet the eye does not perceive, that even limpid water takes any thing from clean and well-purged mercury, which we know, that divers corrosive liquors themselves will not work upon.

To this instance I must add one, that is yet freer from exceptions, which is, that having for curiosity sake suspended in a pair of exact scales, that would turn with a very small part of a grain, a piece of ambergreese bigger than a walnut, and weighing betwixt an hundred and six score grains, I could not in three days and a half, that I had opportunity to make the trial, discover, even upon that balance, any

* As Quercetanus, Libavius, Zabata, Burggravius.

† As Vidius, Paræus, Cæsalpinus, &c.

decrement of weight in the ambergreese; though so rich a perfume, lying in the open air, was like in that time to have parted with good store of odoriferous steams. And a while after suspending a lump of assa fœtida five days and a half, I found it not to have sustained any discernible loss of weight, though, in spite of the unfavourable cold weather, it had about it a neighbouring atmosphere replenished with fœtid exhalations. And when twelve or fourteen hours after, perhaps upon some change of weather, I came to look upon it, though I found, that in that time the æquilibrium was somewhat altered, yet the whole lump had not lost half a quarter of a grain; which induced me to think, that there may perhaps be steams discernible even by our nostrils, that are far more subtil than the odorous exhalations of spices themselves. For having, in very good scales, suspended in the month of *March* an ounce of nutmegs, it lost in about six days five grains and a half. And an ounce of cloves, in the same time, lost seven grains and five-eighths.

You will perhaps wonder, why I do not prefer, to the instances I make mention of in this chapter, that, which may be afforded by the load-stone, that is acknowledged continually to emit multitudes of magnetical steams without decrement of weight. But though I have not thought fit to pass this wholly under silence, yet I forbear to lay so much stress on it, not only because my balances have not yet satisfied me about the effluvia of load-stones, (for I take them not all to be equally diffusive of their particles,) but because I foresee it may be doubted, whether load-stones, like odorous bodies, do furnish afresh of their own, all the corpuscles, that from time to time issue from them; or, whether they be not continually repaired, partly by the return of the magnetical particles to one pole, that sallied out of the other; and partly by the continued passage of magnetical matter, supplied by the earth, or other mundane bodies, which make the pores or channels of the load-stone their constant thorough-fares.

I doubt not but it will make it more probable, that a small quantity of matter being scattered into invisible effluvia, may be exceedingly rarified and expanded, if it can be made appear, that this little portion of matter shall, for a considerable time, emit multitudes of visible parts, and that in so close an order among themselves, as to seem in their aggregate but one entire liquor, endowed with a stream-like motion, and a distinct superficies, wherein no interruption is to be seen, even by an eye placed near it. To devise this experiment, I was induced, by considering, that hitherto all the total dissolutions, that have been made of pigments, have been in liquors naturally cold, and consisting probably of much less subtil, and certainly of much less agitated parts, than that fluid aggregate of shining matter, that we call flame; whereas I argued, that if one could totally dissolve a body composed of parts so minute, as those of a metal, into actual flame, and husband its flame so, as that it should not immoderately waste, I should thereby dissolve the metal in a far more subtil menstruum than our common water, or aqua fortis, or aqua regis, or any other known menstruum I have yet employed. And consequently, the attenuation and expansion of the metal in this truly igneous menstruum would much surpass, not only what happens in ordinary metalline solutions, but possibly also what I have noted in the third chapter of this essay, about the strange diffusion of copper dissolved in spirit of urine and water. In prosecution of this design, I so prepared one single grain of that metal, by a way, that I elsewhere teach, that it was dissolved in about a spoonful of an appropriated menstruum. And then having caused a small glass lamp to be purposely blown to contain this liquor, and fitted it with a socket and wick, we lighted the lamp, which, without consuming the wick, burnt with a flame large enough, and very hot, and seemed to be all the while of a greenish blue, as if it were but a finer and shining solution of copper. And yet this one grain of prepared metal tinged the flame, that was from moment to moment produced, during no less than half an hour and six minutes. And now if we consider, that in this flame there was an uninterrupted succession of multitudes of coloured particles newly extricated, and flying off in every of those many parts wherein a minute of time may either actually or mentally be divided; and if we consider flame as a light and very agitated body, passing with a stream upwards through the air, and if we also consider the quantity of liquor, that would (as I shall by and by tell you) run through a pipe of a much lesser diameter than that flame, within the compass of the fore-mentioned time: what a quantity of the streaming fluid, we call flame, if it could have been preserved, and collected into one body, may we suppose, would appear to have issued out of one grain of copper in the space of thirty-six minutes; and what a multitude of metalline corpuscles may we suppose to have been supplied for the tinging of that flame, during so long a time? since a cylindrical stream of water falling but through a very short pipe of glass, constantly supplied with liquors, did pass at such a rate, that though the aqueous cylinder seemed more slender by half, or perhaps by two-thirds, or better, than the flame, yet we estimated, by the help of a minute-watch, and a good pair of scales that, if I had had conveniencies to let it run long enough, the water effluxed in thirty-six minutes, the time of the flame's duration, would have amounted to above nine gallons, or, reckoning a pint of water to contain a pound of sixteen ounces, seventy-two pounds.

CHAP. VI.

THE last sort of instances I shall propose to shew the strange subtilty of effluvia, is of such as discover the great quantity of space, that may, by a small quantity of matter, when rarified or dispersed, be either filled

as

as to ſenſe, or, at leaſt, made (as they ſpeak) the ſphere of its activity.

To manifeſt this truth, and thereby as well confirm the foregoing chapter, as make out what is deſigned in this, I ſhall endeavour to ſhew, and help your imagination to conceive, how great a ſpace may be impregnated with the effluxions of a body, oftentimes without any ſenſible, and oftener without any conſiderable decrement in bulk, or weight, of the body that affords them. And in order to this, though I ſhall not pretend to determine preciſely how little the ſubſtances, I am to inſtance in, would waſte upon the balance, becauſe you will very eaſily ſee, they are not that way to be examined; yet I preſume, you will as eaſily grant, that the decrement of weight would be but inconſiderable, ſince, of ſuch light ſubſtances, the loſs even of bulk is ſo; which laſt clauſe I ſhall now attempt to make good, by ſetting down ſome obſervations, partly borrowed from the writings of approved phyſicians, and partly, that my friends and I have made about the durable evaporation of ſuch ſmall particles of the effluxions of animals, as are actually not to be diſcerned by the eye to have any of thoſe things ſticking to them, which are ſo very long in flying ſucceſſively away.

It is wont to be ſomewhat ſurprizing to men of letters, when they firſt go a hawking with good ſpaniels, to obſerve, with how great ſagacity thoſe dogs will take notice of, and diſtinguiſh by the ſcent, the places where partridges, quails, &c. have lately been. But I have much more wondered at the quick ſcent of an excellent ſetting-dog, who, by his way of ranging the fields, and his other motions, eſpecially of his head, would not only intimate to us the kinds of game, whoſe ſcent he chanced to light on, but would diſcover to us where partridges have been, though perhaps without ſtaying in that place, ſeveral hours before, and aſſiſt us to gueſs how long they had been gone before we came.

I have had ſtrange anſwers given me in *Ireland*, by thoſe, who make a gain, if not an entire livelihood, by killing of wolves in that country, (where they are paid ſo much for every head they bring in) about the ſagacity of that peculiar race of dogs they employ in hunting them; but not truſting much to thoſe relators, I ſhall add, that a very ſober and diſcreet gentleman of my acquaintance, who has often occaſion to employ blood-hounds, aſſures me, that if a man have but paſſed over a field, the ſcent will lie, as they ſpeak, ſo as to be perceptible enough to a good dog of that ſort for ſeveral hours after. And an ingenious hunter aſſures me, that he has obſerved, that the ſcent of a flying, and heated deer, will ſometimes continue upon the ground from one day to the next following.

And now we may conſider theſe three things; firſt, that the ſubſtance left upon the graſs, or ground, by the tranſient tread of a partridge, hare, or other animal, that does but paſs along his way, does probably communicate to the graſs, or ground, but ſome of thoſe effluxions, that tranſpire out of his feet, which being ſmall enough to eſcape the diſcernment of the eye, may probably not amount to one grain in weight, or perhaps not to the tenth part of it. Next, that the parts of fluid bodies, as ſuch, are perpetually in motion, and ſo are the inviſible particles, that ſwim in them, as may appear by the diſſolution of ſalt, or ſugar, in water, and the wandering of aqueous vapours through the air, even when the eye perceives them not. And thirdly, that though the atmoſphere of one of theſe ſmall parcels of the exhaling matter we are ſpeaking of may oftentimes be exceeding vaſt, in compariſon of the emittent body, as may be gueſſed by the diſtance, at which ſome ſetters, or blood-hounds, will find the ſcent of a partridge, or deer; yet in places expoſed to the fre eair, or wind, it is very likely, that theſe ſteams are aſſiduouſly carried away from their fountain, to maintain the fore-mentioned atmoſphere for ſix, eight, or more hours, that is, as long as the ſcent has been obſerved to lie, there will be requiſite a continual recruit of ſteams ſucceeding one another: and, that ſo very ſmall a portion of matter, as that, which we were ſaying the *fomes* of theſe ſteams may be judged to be, being ſenſibly to impregnate an atmoſphere incomparably greater than itſelf, and ſupply it with almoſt continual recruits, we cannot but think, that the ſteams it parts with, muſt be of an extreme, and ſcarce conceivable minuteneſs.

And we may further conſider, that the ſubſtances, which emit theſe ſteams, being ſuch as newly belonged to animals, and were, for the moſt part, tranſpired through the pores of their feet, muſt be in likelihood a far more evaporable and diſſipable kind of bodies, than minerals or aduſt vegetables, ſuch as gunpowder is made of; ſo that if the grains of gunpowder emit effluviums capable of being, by ſome animals, perceived at a diſtance by their ſmell, one may probably ſuppoſe, that the ſmall grains of this powder may hold out very many times longer to ſupply an atmoſphere with odorable ſteams, than the corpuſcles left on the ground by tranſient animals.

Now though it be generally agreed on, that very few birds have any thing near ſo quick a ſenſe of ſmelling, as ſetting-dogs, or blood-hounds, yet, that the odour of gunpowder, eſpecially when aſſiſted by the ſteams of the caput mortuum of powder formerly fired in the ſame gun, may by fowls be ſmelled at a notable diſtance, particularly when the wind blew from me towards them, I often perſuaded myſelf I obſerved, eſpecially as to crows, when I went a ſhooting; and was confirmed in that opinion, both by the common tradition, and by ſober and ingenious perſons much exerciſed in the killing of wild-fowl, and of ſome four-footed beaſts.

I had forgotten to take notice of one obſervation of the experienced *Julius Palmarius*: whence we may learn, that beaſts may leave upon the vegetables, that have touched their bodies for any time, ſuch corpuſcles, as, though

though unheeded by other animals, may, when eaten by them, produce in them ſuch diſeaſes as the infected animals had. For this author writes, in his uſeful tract *De morbis contagioſis*, that he obſerved horſes, beeves, ſheep, and other animals, to run mad upon the eating of ſome of the ſtraw on which ſome mad ſwine had lain.

AND now to reſume and proſecute our former diſcourſe, you may take notice, that the effluvia, mentioned to have been ſmelt by animals, are, though inviſible, yet big enough to be the objects of ſenſe; ſo that it is not improbable, that among the ſteams, that no ſenſe can immediately perceive, there ſhould be ſome far more ſubtil than theſe, and conſequently capable of furniſhing an atmoſphere much longer, without quite exhauſting the effluviating matter, that afforded them

Lib. VI. Obſer. 22. FORESTUS, an uſeful author, recites an example of peſtilential contagion long preſerved in a cobweb.

ALEXANDER BENEDICTUS writes alſo, that at *Venice* a flock-bed did for many years harbour a peſtiferous malignity to that degree, that when afterwards it came to be beaten, it preſently infected the by-ſtanders with the plague.

Lib. IV. de Feb. cap. 3. AND the learned *Sennertus* himſelf relates, that in the year one thouſand five hundred and forty-two, there did in the city of *Uratiſlavia*, vulgarly *Breſlaw*, where he afterwards practiſed phyſick, die of the plague, in leſs than ſix months, little leſs than ſix thouſand men, and that from that time, the peſtilential contagion was kept folded up in a linen cloath about fourteen years, and at the end of that time being diſplayed in another city, it began a plague there, which infected alſo the neighbouring towns, and other places.

Lib III. Con. 17. TRINCAVELLA makes mention of a yet laſtinger contagion, which occaſioned the death of ten thouſand perſons, that lay lurking in certain ropes, with which, at *Juſtinopolis*, thoſe, that died of the plague, had been let down into the graves.

BUT though none of theſe relations ſhould, to ſome criticks, appear ſcarce credible, it may be objected, that all theſe things, wherein this contagion reſided, were kept cloſe ſhut up, or at leaſt were not expoſed to the air. Wherefore having only intimated, that the exception, which I think is not irrational, would, though never ſo true, but leſſen the wonder of theſe ſtrange relations, without rendering them unfit for our preſent purpoſe, I ſhall add, that though it is the opinion of divers learned phyſicians, that the matter harbouring contagion cannot laſt above twenty, or a few more days, if the body it adheres to be expoſed to the free air and the wind; and though I am not forward to deny, that their judgment may hold in ordinary caſes; yet I muſt not deny neither, that a contagion may ſometimes happen to be much more tenacious, and obſtinate: of which I ſhall give but that one, almoſt recent inſtance, obſerved by the learned *Diemerbroek*, in his own apothecary, who Lib. IV. de Peſte.
having but removed with his foot, from one ſide to the other of a little arbour in his garden, ſome ſtraw, that had lain under the pallet, on which near eight months before a bed had lain, wherein a ſervant of the apothecary's, that recovered, had been ſick of the plague; the infectious ſteams preſently invaded the lower part of his leg, and produced a pungent pain and bliſter, which turned to a peſtilential carbuncle, that could ſcarce be cured in a fortnight after, though, during that time, the patient were neither feveriſh, nor, as to the reſt of his body, ill at eaſe. This memorable inſtance, together with ſome others of the like kind, that our author obſerved in the ſame city of *Nimmeguen*, obtained, not to ſay, extorted, even from him, this confeſſion; which I add, becauſe it contains ſome conſiderable, and not yet mentioned circumſtances of the recited caſe: *Hoc exemplo medicorum doctrina de contagio in fomite latente ſatis confirmatur. Mirum tamen eſt, hoc contagium tanto tempore in prædicto ſtramine potuiſſe ſubſiſtere, utpote quod tota hyeme ventis & pluviis*, (he adds in another place) *nivibus & frigori, expoſitum fuiſſet.*

AND now I will ſhut up this chapter with an inſtance, that ſome will think, perhaps, no leſs ſtrange than any of the reſt; which is, that though they, that are ſkilful in the perfuming of gloves, are wont to imbue them with but an inconſiderable quantity of odoriferous matter, yet I have by me a pair of *Spaniſh* gloves, which I had by the favour of your fair and virtuous ſiſter *(F.)* that were ſo ſkilfully perfumed, that partly by her, partly by thoſe, that preſented them her as a rarety, and partly by me, who have kept them ſeveral years, they have been kept about eight or nine and twenty years, if not thirty, and they are ſo well ſcented, that they may, for aught I know, continue fragrant divers years longer. Which inſtance if you pleaſe to reflect upon, and conſider, that ſuch gloves cannot have been carried from one place to another, or ſo much as uncovered, as they muſt often have been, in the free air, without diffuſing from themſelves a fragrant atmoſphere, we cannot but conclude thoſe odorous ſteams to be unimaginably ſubtile, that could for ſo long a time iſſue out, in ſuch ſwarms, from a little perfumed matter lodged in the pores of a glove, and yet leave it richly ſtocked with particles of the ſame nature; though, eſpecially by reaſon of ſome removes, in which I took not the gloves along with me, I forgot ever ſince I had them, to keep them ſo much as ſhut up in a box.

OF THE GREAT EFFICACY OF EFFLUVIUMS.

CHAP. I.

THEY, that are wont, in the estimates they make of natural things, to trust too much to the negative informations of their senses, without sufficiently consulting their reason, have commonly but a very little and slight opinion of the power and efficacy of effluviums; and imagine, that such minute corpuscles (if they grant, that there are such,) as are not, for the most part of them, capable to work upon the tenderest and quickest of senses the sight, cannot have any considerable operation upon other bodies. But I take this to be an error, which, as it very little becomes philosophers, so it has done no little prejudice to philosophy it self, and perhaps to physick too. And therefore though the nature of my design at present did not require it, yet the importance of the subject would invite me to shew, that this is as ill-grounded as prejudicial a supposition.

AND indeed if we consider the subject attentively, we may observe, that though it be true, that, *cæteris paribus*, the greatness of bodies doth, in most cases, contribute to that of their operation upon others, yet matter or body being, in its own precise nature, an unactive or moveless subject, one part of the mass acts upon another, but upon the account of its local motion, whose operations are facilitated and otherwise diversified by the shape size, situation and texture both of the agent and of the patient. And therefore if corpuscles, though very minute, be numerous enough, and having a competent degree of motion, even these small particles, especially if fitly shaped, when they chance to meet with a body, which the congruity of its texture disposes to admit them at its pores, and receive their either friendly or hostile impressions, may perform such things in the patient, as visible and much grosser bodies, but less conveniently shaped and moved, would be utterly unable on the same body to effect.

AND that you may with the less difficulty allow me to say, that the effluviums of bodies, as minute as they are, may perform considerable things, give me leave to observe to you, that there are at least six ways, by which the effluviums of a body may notably operate upon another; namely, 1. By the great number of emitted corpuscles. 2. By their penetrating and pervading nature. 3. By their celerity, and other modifications of their motion. 4. By the congruity and incongruity of their bulk and shape to the pores of the bodies they are to act upon. 5. By the motions of one part upon another, that they excite or occasion in the body they work upon, according to its structure. And 6. by the fitness and power they have to make themselves be assisted, in their working, by the mere catholic agents of the universe. And though it may perhaps be sufficiently proved, that there are several cases wherein a body, that emits particles, may act notably upon another body, by this or that single way, of those I have been naming; yet usually the great matters are performed by the association of two, three, or more of them, concurring to produce the same effect. Upon which score, when I shall in the following paper refer an instance, or a phænomenon, to any one of the forementioned heads, I desire to be understood as looking upon that but as the head, to which it chiefly relates, without excluding the rest.

CHAP. II.

TAKING those things for granted, that have, I hope, been sufficiently proved in the former tract about the subtilty of effluviums, I suppose it will readily be allowed, that the emanations of a body may be extremely minute; whence it may be rightly inferred, that a small portion of matter may emit great multitudes of them.

Now, that the great number of agents may in many cases compensate their littleness, especially where they act, or resist *per modum unius*, as they speak, men would perhaps the more easily grant, if they took notice to this purpose of some familiar instances.

WE see, that not only lesser land-floods, that overflow the neighbouring fields, but those terrible inundations, that sometimes drown whole countries, are made by bodies singly so small and inconsiderable as drops of rain, when they continue to fall in those multitudes we call showers.

So the aggregates of such minute bodies as grains of sand, being heaped together in sufficient numbers, make banks, wherewith greatest ships are sometimes split, nay, and serve in most places for bounds to the sea it self.

AND though a single corn of gun-powder, or two or three together, are not of force to do much mischief, yet two or three barrels of those corns, taking fire together, are able to blow up ships and houses, and perform prodigious things.

But instead of multiplying such Instances, afforded by bodies of small indeed, but yet visible bulk, I shall (as soon as I have intimated, that the above-mentioned drops of rain themselves consist of convening multitudes of vapours most commonly invisible in their ascent) endeavour to make out what was proposed, by two or three instances drawn from the operations of invisible particles.

AND first, we see, that though aqueous vapours be looked upon as the faintest and least active effluviums, that we know of; yet when multitudes of them are in rainy weather dispersed thorough the air, and are thereby qualified to work on the bodies exposed to it, their operations are very considerable, not only in the dissolution of salts, as sea-salt, salt of tartar, *&c.* and in the putrefactive changes they produce in many bodies, but in the intumescence they cause in oak and other solid woods; as appears by the difficulty we often find in and before rainy weather, to shut and open doors, boxes, and other wooden pieces of work, that were before fit enough for the cavities they had been adjusted to.

I might here urge, that though the strings of viols and other musical instruments are sometimes strong enough to sustain considerable weights, yet if they be left screwed to their full tension, (as it frequently happens) they are oftentimes, by the supervening of moist weather, made to break, not without impetuosity and noise. But it may suit better with my present aim, if I mention on this occasion, (what I elsewhere more fully take notice of,) being desirous to try, what a multitude even of aqueous steams may do, I caused a rope, that was long, but not thick, and was in part sustained by a pully, to have a weight of lead so fastened to the end of it, as not to touch the ground, and after the weight had leisure allowed it to stretch the cord as far as it could, I observed, that in the moist weather the waterish particles, that did invisibly abound in the air, did so much work upon and shorten the rope, as to make it lift up the hanging weight, which was, if I mis-remember not, about an hundred pounds.

THE invisible steams, issuing out of the walls of a newly plaistered or whited room, are not sensibly prejudicial to those, that do but transiently visit it, or make but a very short stay in it, though there be a charcole-fire in the chimney; but we have many instances of persons, that by lying for a night in such rooms, have been the next morning, or sooner, found dead in their beds, being suffocated by the multitude of the noxious vapours emitted during all that time.

AND here I think it proper to observe, that it may much assist us to take notice of the multitude of effluvia, and make us expect great matters from them, to consider, that they are not emitted from the body, that affords them all at once, as hail-shot out of a gun, but issue from it, as the vaporous winds do out of an æolipile well heated, or waters out of a spring-head in continued streams, wherein fresh parts still succeed one another; so that though as many effluxions of a body, as can be sent out at one time, were numerous enough to act but upon its superficial parts, yet the emanation of the next minute may get in a little farther, and each smallest portion of time supplying fresh recruits, and perhaps urging on the steams already entered, the particles may at length get into a multitude of the pores of the invaded body, and penetrate it to the very innermost parts.

CHAP. III.

I COME now to shew, in the second place, that the subtile and penetrating nature of effluviums may, in many cases, co-operate with their multitude in producing notable effects; and that there are effluviums of a very piercing nature, though we shall not now enquire upon what account they are so, we may evince by several examples. For not only the invisible steams of good aqua-fortis and spirit of nitre do usually in a short time, and in the cold, so penetrate the corks, wherewith the glasses, that contained them, were stopped, as to reduce them into a yellow pap; but also the emanations of mercury have been sometimes found in the form of coagulated, or even of running mercury in the heads or very bones of those gilders, or venereal patients, that have too long, or too unadvisedly, been exposed to the fumes of it, though they never took quicksilver in its gross substance. Chemists too often find in their laboratories, that the steams of sulphur, antimony, arsenick, and divers other minerals, are able to make those stagger, or perhaps strike them down, that without a competent warinesss unlute the vessels, wherein they had been distilled or sublimed; of which I have known divers sad examples. And of the penetrancy, even of animal steams, we may easily be persuaded, if we consider, how soon in many plagues the contagious, though invisible, exhalations are able to reach the heart, or infect other internal parts; though in divers of these cases the blood helps to convey the infection, yet still the morbifick particles must get into the body, before they can infect the mass of blood. And in those stupefactions, that are caused at a distance by the torpedo, the parts mainly affected seem to be the nervous ones of the hand and arm, which are of the most retired and best fenced parts of those members. And there is a spirit of sal armoniack, that I make to smell to, whose invisible steams, unexcited by heat, are of so piercing a nature, that not only they will powerfully affect the eyes and nostrils, and throats, and sometimes the stomachs too (yet without proving vomitive) of the patients they invade, but also when a great cold has so clogged the organs of smelling, that neither sweet nor stinking odours would at all affect them, these piercing steams have not only in a few minutes both made themselves a way, and, which is more, so opened the passages, that soon after the patient has been able to smell other things also. And by the same penetrating spirit, a person of quality was, some time since, restored

restored to a power of smelling, which he had lost for divers years, (if he ever had it equally with other men.) I could easily subjoin examples of this kind, but they belong to other places. And here I shall only add, that the steams of water itself, assisted by warmth, are capable of dissolving the texture of even hard and solid bodies, that are not suspected to be saline; as appears by the philosophical calcination (as chemists call it) wherein solid pieces of harts-horn are brought to be easily friable into powder, by being hung over waters, whilst their steams rise in distillation and without the help of furnaces. The exhalations, that usually swim every night in the air, and almost every night fall to the ground in the form of dews (which makes them be judged aqueous) are in many places of the torrid zone of so penetrating a nature, that, as eye-witnesses have informed me, they would, in a very short time, make knives rust in their sheaths, and swords in their scabbards, nay, and watches in their cases, if they did not constantly carry them in their pockets. And I have known even in *England* divers hard bodies, into which the vapours swimming in the air have insinuated themselves so far, as to make them friable throughout. But of the penetration of effluviums, I have given, in several places, so many instances, that it is not necessary to add any here. And therefore to shew, that, as I intimated at the beginning of this chapter, the penetrancy and the multitude of effluviums may much assist each other, I shall now subjoin; that we must not for the most part look upon effluviums, as swarms of corpuscles, that only beat against the outsides of the bodies they invade, but as corpuscles, which by reason of their great and frequently recruited numbers, and by the extreme smallness of their parts, insinuate themselves in multitudes into the minute pores of the bodies they invade, and often penetrate to the innermost of them; so that, though each single corpuscle, and its distinct action, be inconsiderable, in respect of the multitude of parts, that compose the body to be wrought on; yet a vast multitude of these little agents working together upon a correspondent number of the small parts of the body they pervade, they may well be able to have powerful effects upon the body, that those parts constitute; as, in the case mentioned in the former chapter, the rope would not probably have been enabled to raise so great a weight, though a vehement wind had blown against it, to make it lose its perpendicular streightness, but a vast multitude of watery particles, getting by degrees into the pores of the rope, might, like an innumerable company of little wedges, so widen the pores, as to make the thrids or splinters of hemp, the rope was made up of, swell, and that so forcibly, that the depending weight could not hinder the shortening of the rope, and therefore must of necessity be raised thereby. And I have more than once known solid, and even heavy mineral bodies, burst in pieces by the moisture of the air, though we kept them within-doors carefully sheltered from the rain.

CHAP. IV.

THAT the celerity of the motion of very minute bodies, especially conjoined to their multitudes, may perform very notable things, may be argued from the wonderful effects of fired gunpowder, *aurum fulminans*, of flames, that invisibly touch the bodies they work on, and also whirlwinds, and those streams of invisible exhalations and other aereal particles we call winds. But because instances of this sort suit not so well with the main scope of this tract, I shall not insist on them, but subjoin some others, which, though less notable in themselves, will be more congruous to my present design. That the corpuscles, whereof odours consist, swim to and fro in the air, as in a fluid vehicle, will by most, I presume, be granted, and may be easily proved. But I have elsewhere shewn, that the motion of the effluviums of some sufficiently odorous bodies has too little celerity to make a sensible impression on the organs of smelling, unless those steams be assisted to beat more forcibly upon the nostrils by the air, which hurries them along with it, when it enters the nostrils in the form of a stream, in the act of inspiration. And I have by familiar observation of hunters, fowlers, and partly of my own, made manifest, that setting-dogs, hounds, crows, and some other animals, will be much more affected with scents, or the odorous effluvia of partridges, hares, gunpowder, &c. when the wind blows from the object towards the sensory, than when it sits the contrary way, which way soever the nostrils of the animal be obverted, so the air be imbued with the odorous steams: and consequently the difference seems to proceed from this, that when the nostrils are obverted to the wind, the current of the air drives the steams forcibly upon the sensory, which otherwise it does not.

THAT there is a briskness of motion requisite, and more than ordinarily conducive to electrical attractions, may be argued from the necessity, that we usually find by rubbing amber, jett, and other electrical bodies, to make them emit those steams, by which it is highly probable their action is performed: and though I have elsewhere shewn, that this precedent rubbing is not always necessary to excite all electrical bodies; yet in those, that I made to attract without it, it would operate much more vigorously after attrition; which I conceive makes a reciprocal motion amongst the more stable parts, and does thereby, as it were, discharge and shoot out the attracting corpuscles; whose real emission, though it may be probably argued from what has been already said, seems more strongly provable by an observation, that I made many years ago, and which I have been lately informed to have been long since made by the very learned *Fabri*. The observation was this; that if, when we took a vigorously excited electrick, we did at a certain nick of time (which circumstances may much vary, but was usually almost as soon

as

as the body was well rubbed) place it at a just distance from a suspended hair, or other light body, or perhaps from some light powder; the hair, &c. would not be attracted to the electrick, but driven away from it, as it seemed, by the briskly moving steams, that issue out of the amber, or other light body.

THIS argument I could confirm by another phænomenon or two, of affinity with this, if I should not borrow too much of what I have elsewhere noted about the history of electricity.

I know a certain substance, which, though made by distillation, does in the cold emit but a very mild and inoffensive smell, but when the vessel, that holds it, is heated, though no separation of constituent principles appear to be thereby made, (the body being in all usual trials homogeneous) the effluviums will be so altered, that I remember a virtuoso, that, to satisfy his curiosity, would needs be smelling to it, when it was heated, complained to me, that he thought the steams would have killed him, and that the effluviums of spirit of sal armoniack itself were nothing near so strong and piercing as those.

AND even among solid bodies, I know some, which, though abounding much in a substance, wherein some rank smells principally reside, yet (if they were not chafed) were scarce at all sensibly odorous; but upon the rubbing of them a little one against the other, the attrition making them, as it were, dart out their emissions, would in a minute or two make them stink egregiously.

AND as the celerity of motion may thus give a vigour to the emanations of bodies, so there may be other modifications of motion, that may contribute to the same thing, and are not to be wholly neglected in this place. For as we see, that greater bodies do operate differingly, according to such and such modifications; as there is a great difference between the effects of a dart, or javelin, so thrown, as that its point be always forwards, and the same weapon, if it be so thrown, that during its progressive motion the extremes turn about the center of gravity, or some inward parts, as it happens, when boys throw sticks to beat down fruit from the tops of trees; so there is little doubt to be made, but, that in corpuscles themselves, it is not all one, as to their effects, whether they move with, or without rotation, and whether in such or such a line, and whether with, or without undulation, trembling, or such a kind of consecution; and in short, whether the motion have, or have not this or that particular modification; which, how much it may diversify the effects of the bodies moved, may appear by the motion, that the aerial particles are put into by musical instruments. For though the effects of harmony, discord, and peculiar sounds, be sometimes very great, not only in human bodies, but, as we shall shew in the following tract, in organical ones too; the whole efficacy of musick, and of sounds, that are not extraordinarily loud and different, seems, as far as it is ascribable to sonorous bodies, to depend upon the different manners of motion, whereinto that air is put, that makes the immediate impression on our organs of hearing.

CHAP. V.

I SHOULD now proceed to shew, how the celerity and other modes, that diversify the motion of effluviums, may be assisted to make them operative by their determinate sizes and figures, and the congruity, or incongruity, which they may have upon that score, with the pores of the grosser bodies they are to work on: but I think it not fit to entrench upon the subject of another * tract, where the relation between the figures of corpuscles, and the pores of grosser bodies, is amply enough treated of. And therefore I shall only, in this place, take notice of those effects of lightning, which seem referrible, partly to the celerity and manner of appulse, and partly to the distinct sizes and shapes of the corpuscles, that compose the destructive matter, and to the peculiar relation between the particles of that matter, and the structure of the bodies they invade. I know, that many strange things, that are delivered about the effects of what the Latins call *fulmen*, which our English word lightning does not adæquately render, are but fabulous; but there are but too many, that are not so; some of which I have been an eye-witness of, within less than a quarter of an hour after that the things happened. And though it be very difficult to explicate particularly many of these true phænomena, yet it seems warrantable enough to argue from them, that there may be agents so qualified, and so swiftly moved, that notwithstanding their being so exceedingly minute, as they must be, to make up a flame, which is a fluid body, they must, in an imperceptible time, pervade solid bodies, and traversing some of them, without violating their texture, burn, break, melt, and produce other very great changes in other bodies, that are fitted to be wrought on by them. And of this, I must not forget to mention this remarkable instance; that a person, curious enough to collect many rarities, bringing me one day into the study, where he kept the choicest of them, I saw there, among other things, a fine pair of drinking-glasses, that were somewhat slender, but extraordinarily tall; they seemed to have been designed to resemble one another, and made for some drinking entertainment. But before I saw them, that resemblance was much lessened by the lightning, that fell between them in so strange a manner, that, without breaking either of them, that I could perceive, it altered a little the figure of one of them, near the lower part of the cavity; but the other was so bent, near the same place, as to make it stand quite awry, and give it a posture, that I beheld not without some amazement. And I cannot yet but look upon it as a very strange thing, and no less considerable to our present purpose, that nature should, in

* Of the Pores of Bodies, and Figures of Corpuscles.

in the free air, make of exhalations, and that such as probably, when they ascended, were invisible, such an aggregate of corpuscles, as should, without breaking such frail bodies as glasses, be able in its passage thorough them, that is, in the twinkling of an eye, to melt them; which to do is wont, even in our reverberatory furnaces, to cost the active flames a pretty deal of time.

And this calls into my memory, that upon a time, hearing not far off from me such a clap of thunder, as made me judge and say, that questionless some of the neighbouring places were thunder-struck, I sent presently to make enquiry; which having justified my conjecture, I forthwith repaired to the house, where the mischief was done by something, which those, that pretended to have seen it coming thither, affirmed to be like a flame moved very obliquely. To omit the hurt, that seemed to have been done by a wind, that accompanied it, or was perhaps produced by it, to divers persons and cattle; that, which makes me here mention it, was, that observing narrowly what had happened in an upper room, where it first fell, I saw, that it had, in more than one place, melted the lead in its passage, though that possibly outlasted not the twinkling of an eye, without breaking to pieces the glass-casements, or burning, that I took notice of, either the bed, or hangings, or any other combustible houshould-stuff; though, near the window, it had thrown down a good quantity of solid substance of the wall, through which it seemed to have made its passage in or out. And that, which made me the less scruple to mention this accident, is, that having curiously pryed into the effects of the fulmen, not only in that little upper room, but in other parts of the house, beneath whose lowermost parts it seemed to have ended its extravagant course, I could not but conclude, that if so be it were the same fulmen, it must have more than once gone in and out of the house, and that the line of its motion was neither strait, nor yet reducible to any curve, or mixed line, that I had met with among mathematicians; but that, as I then told some of my friends, it moved to and fro in an extravagant manner, not unlike the irregular and wriggling motion of those fired squibs, that boys are wont to make by ramming gunpowder into quills. But about thunder, more perhaps elsewhere. I shall here only add, that whereas it is a known tradition, which my own observations heedfully made seem now and then to confirm, that vehement thunder, if beer be not very strong, will usually, (for I do not say always,) sour it in a day or two; if this degeneration be not one of the consequences of the great and peculiar kinds of the concussions of the air, that happens in loud thunder (in which case, the phænomenon will belong to the next discourse,) the effect may probably be imputed to some subtil exhalations diffused thorough the air, which, penetrating the pores of the wooden vessels, whose contexture is not very close, imbue the liquor with a kind of acetous ferment; which conjecture I should think much confirmed by a trial, it suggested to me, if I had made it often enough to rely upon it. For considering, that the pores of glass are strait enough to be impervous (for aught I have yet observed) to the steams, or spirituous parts of sulphur, as well as to other odorous exhalations, I thought it worth trying, whether there be any sulphureous steams, or other corpuscles, diffused thorough the air in time of thunder, that would not be too gross to get in at such minute pores as those of glass. And accordingly, having hermetically sealed up both beer and ale apart, I kept them in summer time, till there happened a great thunder, a day or two after which, the beer, which we drank, that was good before, being generally complained of, as soured by the thunder, I suffered my liquors to continue, at least a day or two longer, that the souring steams, if any such there were, might have time enough to operate upon them, and then breaking the glasses, I found not, that the liquors had been soured, though we had purposely forborn to fill the glasses, to facilitate the degeneration of the liquors. Perhaps it will be pardonable, on this occasion, to mention a practice, which is usual in some places, where I have been, and particularly employed by a great lady, that is a great housekeeper, and is very curious and expert in divers physical observations; for, talking with her about the remedies of the souring of beer, and other drinks, by thunder, which is sometimes no small prejudice to her, she affirmed to me, that she usually found the practice, I was mentioning, succeed; and, that before the then last great thunder, of which I had observed the effects upon beer, she preserved hers by putting, at a convenient distance, under the barrels, chaffing-dishes of coals, when she perceived, that the thunder was like to begin, which practice, if it constantly succeed, may put one a considering, whether the fire do not, by rarifying the air, and discussing the sulphureous, or other steams, by altering them, or by uniting with them the exhalations of the coals, or by some such kind of way, render ineffectual these souring corpuscles, which perhaps require a determinate bulk and shape, besides their being crowded very many of them together, to have their full operation on barrelled liquors. But these things are but meer conjectures, and therefore I proceed.

CHAP. VI.

THE fifth way, whereby effluviums may perform notable things, is the motion of one part upon another, that they may excite, or occasion in the body they work on, according to its structure.

I shall, in the following tract, have occasion to say something of the motions, into which the internal parts of inanimate bodies may put one another; but the examples now produced are designed to manifest the efficacy, that effluviums may, on the newly-mentioned accounts, have on organical and living bodies. To which instances, it would yet be proper to premise, that even inanimate and solid bodies may be of such a structure, as to be very much alterable by the appropriated effluviums of other bodies, as may be instanced in the power, that I have known some vigorous load-stones to have, of taking away, in a trice, the attractive virtue of an excited needle, or giving a verticity directly contrary to the former, without so much as touching it.

AND we may pertinently take notice of the attractive virtue of the load-stone, as that, which may afford us an eminent example of the great power of a multitude of invisible effluviums, even from bodies, that are not great, upon bodies, that are inorganical or liveless: for taking it for granted, what both the Epicureans, Cartesians, and almost all other corpuscularian philosophers agree in, that magnetism is performed by corporeal emissions, we may consider, that these passing unresistedly thorough the pores of all solid bodies, and even glass itself, which neither the subtilest odours, nor electrical exhalations, are observed to do, seem to be almost incredibly minute, and much smaller than any other effluviums, though themselves too small to be visible; and yet these so incomparably little magnetical effluxions proceeding from vigorous load-stones, will be able to take up considerable quantities of so ponderous a body as iron; insomuch, that I have seen a load-stone, not very great, that would keep suspended a weight of iron, that I could hardly lift up to it with one arm; and I have seen a little one, with which I could take up above eighty times its weight. And these effluvia do not only for a moment fasten the iron to the stone, but keep the metal suspended as long as one pleases.

THIS being premised, I come now to observe, that the chief effects of effluvia belonging to the fifth head are wrought upon animals, which, by virtue of their curious and elaborate structure, have their parts so connected, and otherwise contrived, that the motions or changes, that are produced in one, may have, by the consent of parts, a manifest operation upon others, although perhaps very distant from it, and so framed, as to declare their being affected by actions, that seem to have no affinity at all with the agents, that work upon the part first affected.

I have shewn at large, in another * treatise, that a human body ought not to be looked upon meerly as an aggregate of bones, flesh, and other consistent parts, but as a most curious, and a living engine, some of whose parts, though so nicely framed, as to be very easily affected by external agents, are yet capable of having great operations upon the other parts of the body they help to compose. Wherefore, without now repeating what is there already delivered, I shall proceed to deliver such effects, as are wrought on human bodies by these effluviums, without any immediate contact of the bodies, that emit them.

AND first, not to mention light, because its being, or not being a corporeal thing, is much disputed, even among the moderns; it is plain, that our organs of smelling are sensibly affected by such minute particles of matter as the finest odours consist of. Nor do they always affect us precisely as odours, since we see, that many persons, both men and women, are by smells, either sweet or stinking, put into troublesome head-achs.

IF it were not almost ordinary, it would be more than almost incredible, that the smell of a pleasing perfume should presently produce, in a human body, that immediately before was well and strong, such faintnesses, swoons, loss of sensible respiration, intumescence of the abdomen, seeming epilepsies, and really convulsive motions of the limbs, and I know not how many other frightful symptoms, that, by the unskilful, are often taken for the effects of witchcraft, and would impose upon physicians themselves, if their own, or their predecessors experience, did not furnish them with examples of the like phænomena produced by natural means. Those symptoms manifest, what the consent of parts may do in a human body; since even morbifick odours, if I may so call them, by immediately affecting the organs of smelling, affect so many other parts of the *genus nervosum*, as oftentimes to produce convulsive motions, even in the extreme parts of the hands and feet.

NOR is the efficacy of effluviums confined to produce hysterical fits, since these invisible particles may be able, and sometimes as suddenly, as perfumes are wont to excite them, to appease them; as I have very frequently, though not with never-failing success, tried, by holding a spirit, I usually make of sal armoniack, under the nostrils of hysterical persons. My remedy did not only often recover, in a trice, those, whose fits were but ordinary, but did, more than once, somewhat to the wonder of the by-standers, relieve, within a minute or two, persons of differing ages, and constitutions, that were suddenly fallen down by fits, that the by-standers judged epileptical, but I, hysterical.

I attribute the good and evil operations of the fore-mentioned steams, rather in general to the consent of the parts, that make up the *genus nervosum*, than to any hidden sympathy, or antipathy, betwixt them and the womb, not only for other reasons, not proper to be insisted on here, but because I have known odours have notable effects even upon men. I know a very eminent person, a traveller, and a man of a strong constitution, but considerably sanguine, who is put into violent head-achs by the smell of musk. And I remember, that one day being with him, and a great many other men of note, about a publick affair, a man, that had a parcel of musk about him, having an occasion to make an application to us, this person was so disordered by the smell, which to most of us was delightful, that, in spite of his civility, he was reduced to make us an apology, and send the perfumed man out of the room; notwithstanding whose recess, this person complained to me, a good while after, of a violent pain in his head, which

* The Usefulness of Experimental Philosophy.

which I perceived had somewhat unfitted him for the transaction of the affair, whereof he was to be the chief manager. I know another person, whose happy muse hath justly made him many admirers, that is subject to the head-ach upon so mild a smell, as that of damask roses, and sometimes even of red roses; insomuch, that walking one day with him in a garden, whose alleys were very large, so that he might easily keep himself at a distance from the bushes, which bore many of them red roses; he abruptly broke off the discourse we were engaged in, to complain of the harm the perfume did his head, and desired me to pass into a walk, that had no roses growing near it.

If it were not for the sex of the person, I could relate an instance, that would be much more considerable, of the operation of roses. For I know a discreet lady, to whom their smell is not unpleasing, (for she answered me, that it was not so at all) but so hurtful, that it presently makes her sick, and would make her swoon, if not seasonably prevented: and she told me, that being once at a court, in which she was a maid of honour, though she herself did not know whence it came, she found herself extremely ill on a sudden, and ready to sink down for faintness; but being then in discourse with a person, whose high quality she payed her profound respect to, her civility, that kept her from complaining, or withdrawing, might have been dangerous, if not fatal to her, had not the princess, who was speaking with her, and who knew her antipathy to roses, taken notice, that her face grew strangely pale, and was covered with a cold sweat. For thereby presently guessing what might be the cause, which the sick lady herself did not, she asked aloud, whether some body had not brought roses (which were then in season) into the bedchamber, which question occasioned a speedy withdrawing of a lady, that stood at a distance off, and had about her roses, which were not seen by the patient, who was by this means preserved from falling into a swoon, though not from being for a while very much discomposed.

But this you may tell me was the case of a woman, who complained her malady affected her heart, not her head. Wherefore returning to what I was speaking of before I mentioned her, I shall proceed to tell you, that as odours may thus give men the head-ach, so I have often found the smell of rectified spirit of sal armoniack to free men, as well as women, from the fits of that distemper; and that sometimes in so few minutes, that the persons relieved could scarcely imagine, they could so quickly be so.

To which I shall not add the trials, that I have successfully made upon myself, because being, thanks be to God, very seldom troubled with that distemper, the occasions I have had of making them have not been many. And though I have not always found so slight a remedy to work the desired cure, yet that it does it often, even in men, is sufficient to shew the efficacy of sanative effluviums.

Now to manifest, that steams do not operate only upon hysterical women, or persons subject to the head-ach, I will add some instances of the effects they may produce upon other persons, and parts.

It is but too well known an observation, that women with child have been often made to miscarry by the stink of an ill-extinguished candle, though perhaps the smoke ascending from the snuff were dissipated into the invisible corpuscles, a good while before it arrived at the nostrils of the unhappy woman; and what violent and straining motions abortions are frequently accompanied with, is sufficiently known already.

I think I have elsewhere mentioned, that a gentleman of my acquaintance, a proper and lusty man, will be put into the fits of vomiting by the smell of coffee, boiled in water. I shall therefore rather mention, that I know a physician, who having been, for a long time, when he was young, frequently compelled to take electuarium lenitivum, one of the gentlest and least unpleasant laxatives of the shops, conceived such a dislike of it, that still, as himself has complained to me, if he smell to it, as he sometimes happens to do in apothecaries shops, it will work (now and then for several times) upwards and downwards with him.

I know another very ingenious person of the same faculty, that has been a traveller by sea and land, who has complained to me, that the smell of the grease of the wheels of a hackney-coach, though it do but pass by him, is wont to make him sick, and ready to vomit.

Every body knows, that smoke is apt to make mens eyes water, and excite, in the organs of respiration, that troublesome and vehement commotion we call coughing. But we need not have recourse at all to visible fumes, for the production of the like effects; since we have often observed them, and repeated sneezings to boot, to proceed from the invisible steams of spirit of sal armoniack, when vials containing that liquor, though they were perhaps but very small, were approached too hastily, or, perhaps, too near to the nostrils.

And because in most of the foregoing instances, the chief effects seem to be wrought, by the consent of parts, on the *genus nervosum*, and the action of one of them upon the other, and thereby upon several other parts of the body, I will subjoin a remarkable instance of the operation of a mild and grateful odour upon the humours themselves, and that in a man.

A famous apothecary, who is a very tall and big man, several times told me, that though he was once a great lover of roses, yet having had occasion to employ great quantities of them at a time, he was so altered by their steams, that now, if he come among the rose-bushes, the smell does much discompose him. And the odour of roses, (I mean incarnate-roses, which we commonly call damask-roses, though they be not the true ones) makes such a colliquation of humours in his head, that it sets him a coughing, and makes him

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run at the nose, and gives him a sore throat; and by an affluence of humours makes his eyes sore, insomuch, that during the season of roses, when quantities of them are brought into his house, he is obliged, for the most part, to absent himself from home.

CHAP. VII.

ONE may shew, on this occasion, that as there might be considerable things performed by effluviums, as they make one part of a living engine work upon another by virtue of its structure, so the action of such invisible agents may, in divers cases, be much promoted by the fabrick and laws of the universe itself, upon this account, that, by the operation of effluvia upon particular bodies, they may dispose and qualify those bodies to be wrought upon, which before they were not fit to be, by light, magnetisms, the atmosphere, gravity, or some other of the more catholick agents of nature, as the world is now constituted. But not to injure another tract, I shall conclude this, when I shall have taken notice, that in the instances hitherto produced, there has been a visible local distance between the body that emits streams, and that on which they work. But if I thought it necessary, it were not difficult to shew, that one might well enough refer to the title of this tract divers effects of bodies, that are applied immediately to ours; such as are blood-stones, cornelians, nephritick stones, lapis Malacensis, and some amulets, and other solid substances, applied by physicians outwardly to our bodies. For in these applications the gross body touches but the skin, and the great effects, which I elsewhere relate myself to have sometimes (though not often, much less always) observed to have followed upon this external contact, or near application, may reasonably be derived from the subtil emanations, that pass thorough the pores of the skin to the inward parts of the body: as is evident in those, who by holding cantharides in their hands, or having them applied to some remote external part, have grievous pains produced in their urinary parts, as it has happened to me, as well as to many others. And to the insinuation of these minute corpuscles, that get in at the pores of the skin, seems to be due the efficacy of some medicines, that purge, vomit, resolve the humours, or otherwise notably alter the body, being but externally applied; of which I could here give several instances, but that they belong more properly to another place, and are not necessary in this, where it may suffice to name the notorious power, that mercurial ointments, or fumes, either together, or apart, have of producing copious salivations, to shew in general, that both the steams, and the emanations of outwardly-applied medicinal bodies, may have some great effects on human ones.

OF THE DETERMINATE NATURE OF EFFLUVIUMS.

CHAP. I.

THE effluviums of bodies, *Pyrophilus*, being for the most part invisible, have been wont to be so little considered by vulgar philosophers, that scarce vouchsafing to take notice of their existence, it is no wonder, that men have not been solicitous to discover their distinct natures and differences. Only *Aristotle*, and, upon his account, the schools, have been pleased to think, that the two grand parts of our globe do sometimes emit two kinds of exhalations, or steams; the earthy part affording those, that are hot and dry, which they name fumes, and very often, simply, exhalations; and the aqueous part, others, that are (not as many of his disciples mistake him to have taught, cold and moist, but) hot and moist *, which they usually call vapours, to discriminate them from the fumes, or exhalations, though otherwise, in common acceptation, those appellations are very frequently confounded.

Lib. I. Meteor. cap. 3, & 4.

BUT, though the *Aristotelians* have thus perfunctorily handled this subject, it would not become corpuscularian philosophers, who attribute so much as they do to the insensible particles of matter, to acquiesce in so slight and jejune an account of the emanations of bodies. And since we have already shewn, that besides the greater and more simple masses of terrestrial and aqueous matter newly mentioned, there are very many mixed bodies, that emit effluviums, which make, as it were, little atmospheres about divers of them, it will be congruous to our doctrine and design, to add in this place, that besides the slight and obvious differences, taken notice of by *Aristotle*, the steams of bodies may be almost as various, as the bodies themselves, that

* Cap 3. Ἔστι γὰρ ἀτμίδος μὲν φύσις, ὑγρὸν καὶ θερμόν.

that emit them; and that therefore we ought not to look upon them barely under the general and confused notion of smoke, or vapours, but may probably conceive them to have their distinct and determinate natures, oftentimes, though not always, suitable to that of the bodies from whence they proceed.

And indeed, the newly-mentioned divisions of the schools gives us so slight an account of the emanations of bodies, that, methinks, it looks like such another, as if one should divide animals into those, that are horned, and those, that have two feet: for, besides that the distinction is taken from a difference, that is not the considerablest, there are divers animals, as many four-footed beasts, and fishes, that are not comprised in it; and each member of the division comprehends I know not how many distinct sorts of animals, whose differences from one another are many times more considerable, than those, that constitute the two supreme genus's, the one having bulls and goats, and rhinoceros's, and deer, and elks, and certain sea-monsters, whose horns I have seen; and the other genus comprising also a greater variety, namely, a great part of four-footed beasts, and, besides men, all the birds (for aught we know) whether of land or water. And as it would give us but a very slender information of the nature of an elk, or an unicorn, to know, that it is an horned beast; or of the nature of a man, an eagle, or a nightingale, to be told, that it is an hornless beast; so it will but very little instruct a man in the nature of the steams of quicksilver, or of opium, to be told, that they are vapours hot, or rather cold, and moist; or of the steams of amber, or cantharides, or cinnamon, or tobacco, to be told, that they are hot and dry. For, besides that there may be effluviums, which, even by their elementary qualities, are not of either of these two supreme genus's, (for they may be cold and dry, or cold and moist,) these qualities are often far from being the noblest, and consequently those, that deserve to be most considered in the effluviums of this, or that body; as we shall by and by have occasion to manifest.

CHAP. II.

AND here it may not be improper to mention an experiment, that, I remember, I divers years since employed to illustrate the subject of our present discourse.

I considered then, that fluid bodies may be of very unequal density, and gravity, as is evident in quicksilver, water, and pure spirit of wine; which, notwithstanding their great difference in specifick gravity, may yet agree in the conditions requisite to fluid bodies. Therefore presuming, that by what I could make appear visible in one, what happens analogically in the other, may be ocularly illustrated, I took some ounces of roch-allom, and as much of fine salt-petre. I took some ounces of each, because, if the quantity of the ingredients be too small, the concoagulated grains will be so too, and the success will not be so conspicuous. These being dissolved together in fair water, the filtrated solution was set to evaporate in an open-mouthed glass, and being then left to shoot in a cool place, there were fastned to the sides, and other parts of the glass, several small crystals, some octoedrical, which is the figure proper to roch allom, and others of the prismatical shape of pure salt-petre; besides some other saline concretions, whose being distinctly of neither of these two shapes, argued them to be concoagulations of both the salts. And this we did, by using such a degree of celerity in evaporating the liquor, as was proper for such an effect. For, by another degree, which is to be employed, when one would recover the salts more distinctly and manifestly, the matter may, as I found by trial, be so ordered, that the aluminous salt may, for the most part, be first coagulated by itself, and then, from the remaining liquor, curiously shaped crystals of nitre may be copiously obtained.

Trials like this we also made with other salts, and particularly with sea-salt, and with allom and vitriol; the phænomena of which you may meet with in their due places. For the recited experiment may, I hope, alone serve to assist the imagination to conceive, how the particles of bodies may swim to and fro in a fluid, (which the air is,) and though they be little enough to be invisible, may, many of them, retain their distinct and determinate natures, and their aptness to cohere upon occasion; and others may, by their various occursions and coalitions, unite into lesser corpuscles, or greater bodies, differing from the more simple particles, that composed them, and yet not of indeterminate, though compounded figures.

CHAP. III.

THESE things being premis'd, we may now proceed to the particular instances of the determinate nature of effluviums; and these we may not inconveniently reduce to the three following heads, to each of which we shall assign a distinct chapter; the first of these I shall briefly treat of in this third chapter, and treat somewhat more largely of the others in the two following.

In the first place then, that the effluviums of many bodies retain a determinate nature oftentimes in an invisible smallness, and oftener in such a size, as makes them little enough to fly or swim in the air; may appear by this, that these effluvia being, by condensation, or otherwise, re-united, they appear to be of the same nature with the body, that emitted them. Thus in moist weather, the vapours of water, that wander invisibly through the air, meeting with marble-walls, or pavements, or other bodies, by their coldness and other qualifications, fit to condense and retain them, appear again in the form of drops of water; and the same vapours return to the visible form of water, when they fall out of the air in dews, or rains.

Quick-silver it self, if it be made to ascend in distillation with a convenient degree of of fire, will almost all be found again in the receiver in the form of running mercury. Which strange and piercing fluid is in some cases so disposed to be stripped of its disguises, and re-appear in its own form, that divers artificers, and especially gilders, have found, to their cost, that the fumes of it need not be, as in distillation, included in close vessels to return to their pristine nature, mercury having been several times found in the heads, and other parts of such people, who have, in tract of time, been killed by it, and sometimes made to discover itself during the lives of those, that dealed so much in it; of which I elsewhere give some instances. Wherefore I shall only observe at present, that it is a common practice, both among gilders, and some chemists, that, when they have occasion to make an amalgam, or force away the mercury from one by the fire, they keep gold in their mouths, which by the mercurial fumes, that wander through the air, will now and then, by that time it is taken out of their mouths, be turned white almost, as if it had been silvered over.

A mass of purified brimstone being sublimed, the ascending fumes will condense into what the chemists call *flores Sulphuris*, which is true sulphur of the same nature with that, formerly exposed to sublimation; and may readily, by melting, be reduced into such another mass.

AND to give you another like example of dry bodies; I tried, that by subliming good camphire in close vessels, it would all, as to sense, be raised into the upper vessel, or part of the subliming-glass, in the form of dry camphire, as it was before.

NAY, though a body be not by nature, but art, compounded of such differing bodies, as a metal and another mineral, and two or three salts; yet, if upon purification of the mixture from its grosser parts, the remaining and finer parts be minute enough and fitly shaped, the whole liquor will ascend, and yet in the receiver altogether recover its pristine form of a transparent fluid, composed of differing saline and mineral parts. This is evident in the distillation of what chemists call butter, or oil of antimony, very well rectified. For this liquor will pass into the receiver diaphanous and fluid, though, besides the particles of the sublimate, (which is it self a factitious compounded body) it abounds with antimonial corpuscles, carried over and kept invisible by the corroding salts; whatever *Angelus Sala*, and those chemists, that follow him, have affirmed to the contrary, as might be easily here proved, if this were a fit place to do it in.

I found by enquiring of an ingenious person, that had an interest in a tin-mine, that I was not deceived in guessing, that tin it self, though a metal, whose ore is of a very difficult fusion, and which I have by it self kept long upon the cupel without finding it to fly away, would yet retain its metalline nature in the form of fumes or flowers. For this experienced gentleman answered me, that divers times they would take great store of a whitish sublimate from the upper part of the furnaces or chimnies, where they brought their ore to fusion, or wrought further upon it; and that this sublimate, though perhaps elevated to the height of an ordinary man, would, when melted down, afford at once many pounds of very good tin. On which occasion I shall add, that I have my self, more than once, raised this metal in the form of white corpuscles by the help of an additament, that did scarce weigh half so much as it.

CHAP. IV.

THE second way, by which we may discover the determine nature of effluviums, is, by the difference, that may sometimes be observed in their sensible qualities. For these effluviums, that are endowed with them, proceed from the same sort of bodies, and yet those afforded by one kind of bodies being in many cases manifestly differing from those, that fly off from another, this evident disparity in their exhalations argues their retaining distinct natures, according to those of the respective bodies whence they proceed.

I will not now stay to examine, whether in the steams, that are made visibly to ascend from the terrestrial globe by those grand agents and usual raisers of them, the sun, and the agitation of the air, the eye can manifestly distinguish the diversity of colours: but in some productions of art, such different colours may be discovered in the exhalations, even without the application of any external heat to raise them. For, when spirit of nitre, for example, has been well rectified, I have often observed, that even in the cold the fumes would play in the unfilled part of the stopped vials it was kept in, and appear in it of a reddish colour, and if those vessels were opened, the same fumes would copiously ascend into the air, in the form of a reddish or orange-tawny smoke. spirit, or oil of salt also, if it be very well dephlegmed, though it will scarce in the cold visibly ascend in the empty part of a vial, whilst it is kept well stopped; yet, if the free air be allowed access to it, it will, in case it be sufficiently rectified, fly up in the form of a whitish fume. But this is inconsiderable, in comparison of what happens in a volatile tincture of sulphur I have elsewhere taught you to make with quick-lime. For, not only upon a slight occasion, the vacant part of the vial will be filled with white fumes, though the glass be well stopped; but upon the opening the vial these fumes will copiously pass out at the neck, and ascend into the air in the form of a smoke, more white, than perhaps you ever saw any. And both this and that of the spirit of saltpetre do, by their operation, as well as smell, disclose what they are; the latter being of a nitrous nature, (as is confessed) and the former, of a sulphureous: in so much, that having, for curiosity's sake, in a fitly shaped glass, caught a competent quantity of the ascending white fumes, I found them to have convened into bodies transparent and geometrically figured, wherein it was easy to discover, by their sensible qualities, that there were store of sulphureous particles mixed with the saline ones. That the liquors of vegetables, diſtilled *in balneo*, or in water, are not wont to retain any thing of the colour of the bodies, that afforded them, is a thing eaſy to be obſerved in diſtillations made without retorts or the violence of the fire. But it may be worth while to make trial, whether the eſſential oil of wormwood aſcend coloured like the plant whence it is firſt drawn over with water in the alembic, or rectified *in balneo*. For I forgot to take notice of it, when upon ſome particularities, I obſerved in that plant, my curioſity led me to find, that not only in the firſt diſtillation in a copper alembic, tinned on the inſide, the oil came over green, but by a rectification purpoſely made in a glaſs veſſel, the purified liquor was not deprived of that colour.

THE mention of theſe eſſential oils, as chemiſts call thoſe, that are drawn in alembics, leads me to tell you, that, though theſe liquors be but effluvia of the vegetables they are diſtilled from, condenſed again in the receiver into liquors; yet, as ſubtile as they are, many of them retain the genuine taſte of the bodies, whence the heat elevated them; as you will eaſily find, if you will taſte a few drops of the eſſential oil of cinnamon, for example, or of wormwood diſſolved by the intervention of ſugar, or ſpirit of wine, in a convenient quantity of water, wine, or beer. For, by this means you have the natural taſte of this ſpice or herb. And wormwood is a plant, whoſe effluvia do ſo retain the nature of the body, that parts with them, that I muſt not forbear to alledge here an obſervation of mine, that may ſhew you, that it is poſſible, though not uſual, that even without the help of the fire, the expirations of a body may communicate its taſte. For, among other things, that I had occaſion to obſerve about ſome quantity of wormwood laid up together, I remember, I took notice, and made others do the like, that coming into a room, where it was kept, not only the organs of ſmelling were powerfully wrought upon by the corpuſcles, that ſwarmed in the air, but alſo the mouth was ſenſibly affected with a bitter taſte. Perhaps you will ſcarce think it worth while, that after this inſtance I ſhould add, that I found the expirations of amber, kept a while in pure ſpirit of wine, taſte upon the tongue like amber it ſelf, when I chewed it between my teeth. But I chooſe to mention this inſtance, becauſe it will connect thoſe lately mentioned with another ſort, very pertinent to our preſent purpoſe. For, the expirations, that I have obtained from amber, both with pure ſpirit of wine, and a more peircing menſtruum, did manifeſtly retain in both thoſe liquors a peculiar ſmell, with which I found it to affect the noſtrils, when, for trial's ſake, I excited the electrical faculty of amber by rubbing. And as for odours, it is plain, that the eſſential oils of chemiſts, well drawn, do many of them retain the peculiar and genuine ſcent of the ſpices or herbs, that afforded them. And that theſe odours do really conſiſt of, or reſide in certain inviſible corpuſcles, that fly off from the viſible bodies, that are ſaid to be endowed with ſuch ſmells, I have elſewhere proved at large; and it may ſufficiently appear from their ſticking to divers of the bodies they meet with and their laſting adheſion to them.

OTHER examples may be given of the ſettled difference of effluviums directly perceivable by human organs of ſenſe, as dull as they are; which laſt expreſſion I add, becauſe I ſcarce doubt, but that, if our ſenſories were ſufficiently ſubtile and tender, they might immediately perceive in the ſize, ſhape, motion, and perhaps colour too of ſome now inviſible effluviums, as diſtinguiſhable differences, as our naked eyes in their preſent conſtitution ſee, between the differing ſorts of birds, by their appearances, and their manner of flying in the air, as hawks, and partridges, and ſparrows, and ſwallows. To make this probable I will not urge, that in fine white ſand, whoſe grains by the unaſſiſted eye are not wont to be diſtinguiſhed by any ſenſible quality, I have often obſerved in an excellent miſcroſcope, a notable diſparity as to bulk, figure, and ſometimes as to colour: and that in ſmall cheeſe-mites, which the naked eye can very ſcarcely diſcern, ſo far is it from diſcovering any difference between them, one may (as was noted in the laſt eſſay) plainly ſee, beſides an obvious difference in point of bigneſs, many particular parts, on whoſe accounts, the ſtructure of thoſe moving points may difference them from each other. And I have ſometimes ſeen a very evident diſparity, even in point of ſhape, between the very eggs of theſe living atoms, as a poet would perhaps ſtile them. But theſe kinds of proofs, (as I was ſaying) I ſhall forbear to inſiſt on, that I may proceed to countenance my conjecture by the effects of the effluviums, that are properly ſo called, upon animals.

AND firſt, though the touch be reckoned one of the moſt dull of the five ſenſes, and be reputed to be far leſs quick in men, than in divers other animals; yet the groſs organs of that may, in men themſelves, even by accident, be ſo diſpoſed, as to be ſuſceptible of impreſſions from effluvia: of this, in another paper, I give ſome inſtances. And I know not, whether divers of the preſages of weather, to be obſerved in ſome animals, and the aches, and other pains, that in many crazy and wounded men, are wont to fore-run great changes of weather, do not often (for I do not ſay always) proceed, at leaſt in part, from inviſible, and yet incongruous effluxions, which, either from the ſubterraneal parts, or from ſome bodies above ground, do copiouſly impregnate the air. And, on this occaſion, it will not be impertinent to mention here what an experienced phyſician, being (if I much miſremember not) the learned *Diemerbroek*, relates concerning himſelf, who having been infected with the plague by a patient, that lay very ill of it, though by God's bleſſing, which he particularly acknowledges, upon a ſlight, but ſeaſonable remedy, he was very quickly cured, and that without the breaking of any tumor; yet it left ſuch a change in ſome parts of his body, that he ſubjoins this memorable paſſage, *Ab illo periculo ad contagioſos mihi appropinquanti in emunctoriis ſucceſſit dolor, vix fallax peſtis indicium.*

TWO

About Cosmical Suspicions

Two or three other obſervations of the like nature you meet with in another of my papers. And I ſhall now add, that I know an ingenious gentlewoman (wife to a famous phyſician) who was of a very curious and delicate complexion, that has ſeveral times aſſured me, that ſhe can very readily diſcover, whether a perſon, that comes to viſit her in winter, came from ſome place, where there is any conſiderable quantity of ſnow; and this ſhe does, as ſhe tells me, not by feeling any unuſual cold (for if the ground be frozen, but not covered with ſnow, the effect ſucceeds not,) but from ſome peculiar impreſſion, which ſhe thinks, ſhe receives by the organs of ſmelling. I might add, that I know alſo, as I may have formerly told you, a very ingenious phyſician, who falling into an odd kind of fever, had his ſenſe of hearing thereby made ſo very nice and tender, that he very plainly heard ſoft whiſpers, that were made at a conſiderable diſtance off, and which were not in the leaſt perceived by the healthy by-ſtanders, nor would have been by him before his ſickneſs. Which ſickneſs I mention as the thing, that gave his organs of hearing this preter-natural quickneſs, becauſe, when the fever had quite left him, he was able to hear but at the rate of other men. And I might tell you too, that I know a gentleman of eminent parts and note, who, during a diſtemper he had in his eyes, had his organs of ſight brought to be ſo tender, that both his friends, and himſelf alſo, have aſſured me, that when he waked in the night, he could for a while plainly ſee and diſtinguiſh colours, as well as other objects, diſcernible by the eye, as was more than once tried, by pinning ribbands, or the like bodies, of ſeveral colours, to the inſide of his curtains in the dark. For if he were awakened in the night, he would be able to tell his bed-fellow, where thoſe bodies were placed, and what colour each of them was of.

I have mentioned theſe inſtances only to ſhew you, that if our ſenſories were more delicate and quick, they would be ſufficiently affected by objects, that, as they are generally conſtituted, make no impreſſions at all upon them. For otherwiſe I know, that the ſpecies (as they call them) both of ſounds and colours are not held by many of the moderns, (from whom in that I diſſent not,) to be ſo much corporeal effluxions, trajected through the medium, as peculiar kinds of local motion conveyed by it. Therefore, I ſhall now confirm the conjecture I would countenance, by the diſcrimination made by the organs of other animals, of ſuch effluvia, as to us men are not only inviſible, but inſenſible. And therefore, partly to ſtrengthen what I delivered, and partly to confirm what I am now diſcourſing of, it will not be impertinent to ſubjoin two or three relations, that I had from perſons of very good credit, whom I thought likely to make me no unſatisfactory returns to my queſtions, about things they were very well verſed in.

A perſon of quality, to whom I am near allied, related to me, that to make a trial, whether a young blood-hound was well inſtructed, (or as the huntſmen call it, made) he cauſed one of his ſervants, who had not killed, or ſo much as touched any of his deer, to walk to a country-town, four mile off, and then to a market-town, three miles diſtant from thence; which done, this nobleman did, a competent while after, put the blood-hound upon the ſcent of the man, and cauſed him to be followed by a ſervant or two, the maſter himſelf thinking it alſo fit to go after them to ſee the event; which was, that the dog, without ever ſeeing the man he was to purſue, followed him by the ſcent to the above-mentioned places, notwithſtanding the multitude of market people, that went along in the ſame way, and of travellers, that had occaſion to croſs it. And when the blood-hound came to the chief market-town, he paſſed through the ſtreets, without taking notice of any of the people there, and left not, till he had gone to the houſe, where the man, he ſought, reſted himſelf, and found him in an upper room, to the wonder of thoſe that followed him. The particulars of this narrative, the nobleman's wife, a perſon of great veracity, that happened to be with him when the trial was made, confirmed to me.

ENQUIRING of a ſtudious perſon, that was keeper of a red-deer park, and verſed in making blood-hounds, in how long time, after a man or deer had paſſed by a graſſy place, one of thoſe dogs would be able to follow him by the ſcent? he told me, that it would be ſix or ſeven hours: whereupon an ingenious gentleman, that chanced to be preſent, and lived near that park, aſſured us both, that he had old dogs of ſo good a ſcent, that if a buck had the day before paſſed in a wood, they will, when they come where the ſcent lies, though at ſuch a diſtance of time after, preſently find the ſcent and run directly to that part of the wood where the buck is. He alſo told me, that though an old blood-hound will not ſo eaſily fix on the ſcent of a ſingle deer, that preſently hides himſelf in a whole herd, yet if the deer be chaſed a little till he be heated, the dog will go nigh to ſingle him out, though the whole herd alſo be chaſed. The above-named gentleman alſo affirmed, that he could eaſily diſtinguiſh, whether his hounds were in chaſe of a hare, or a fox, by their way of running, and their holding up their noſe higher than ordinary, when they purſue a fox, whoſe ſcent is more ſtrong. Theſe relations will not be judged incredible by him, that reflects on ſome of the inſtances, that have already, in the foregoing eſſay, been given of the ſtrange ſubtilty of effluvia: to which I ſhall now add, that I remember, that to try, whether I could, in ſome meaſure, make art imitate nature, I prepared a body of a vegetable ſubſtance, which, though it were actually cold, and both to the eye and touch dry, did for a while emit ſuch determinate and piercing, though inviſible, exhalations, that having, for trial's ſake, applied to it a clear metalline plate, and that of none of the very ſofteſt kind neither, for about one minute of an hour, I found, that, though there

had

had been no immediate contact between them, I having purposely interposed a piece of paper to hinder it, yet there was imprinted on the surface of the plate a conspicuous stain of that peculiar colour, that the body, with whose steams I had imbued the vegetable substance, was fitted to give a plate of that mixed metal. And though it be true, that in some circumstances, the lately-mentioned instances about blood-hounds have a considerable advantage of this I have now recited, yet that advantage is much lessened, not to say countervailed, by some circumstances of our experiment. For, not to repeat, that the emittent body was firm and cold, the effect produced by the effluvium, that guided the setting-dog, was wrought upon the sensory of a living and warm animal; and such an one, whose organs of smelling are of an extraordinary tender constitution above those of men, and other animals, and probably the impression was but transient; whereas, in our case, the invisible steams of the vegetable substance wrought upon a body, which was of so strong and inorganical a texture, as a compounded metal, though it were fenced by being lapped up in paper, notwithstanding which these steams invaded it in such numbers, and so notably, as to make their operation on it manifest to the eye, and considerably permanent too; since, coming to look upon the plate after the third day, I found the induced colour yet conspicuous, and not like suddenly to vanish.

Hitherto, in this chapter, I have argued from the constant and settled difference of the sensible qualities of effluviums, that they do not always lose their distinct natures, when they seem to have lost themselves by vanishing into air. But before I dismiss this subject, I must consider an objection, which I know may be made against the opinion we have been countenancing. For it may be alledged, that there may be many cases, wherein the effluviums of bodies are, in their passage through the air, sensibly altered, or do affect the organs of sense, otherwise than each kind of them apart would do: nor is this difficulty altogether irrational. For it seems consonant enough to experience, that some such cases should be admitted; and therefore, in the foregoing discourse I have, where I thought it necessary, forborn to express myself in such general and absolute terms, as otherwise I might have done. But as for such cases, as I have insisted upon, and many more I shall now represent, that the objected alterations need not hinder, but that effluviums, at their first parting from the bodies, whence they take wing, if I may so speak, may retain as much of the nature of those bodies, as we have ascribed to them; since the subsequent change may very probably be deduced from the combinations, or coalitions, of divers steams associating themselves in the air, and acting upon the sensory, either altogether and conjointly, or at least so near it, that the sense cannot perceive their operations as distinct. This I shall elucidate, but not pretend to prove, by what happens in sounds and tastes. For if, by way of instance, in a musical instrument, two strings tuned to an eighth, be touched together, they will strike the ear with a sound, that will be judged one, as well as pleasing, though each of the trembling strings make a distinct noise, and the one vibrates as fast again as the other. And if, into oil of tartars *per deliquium*, you drop a due proportion of spirit of nitre, and exhale the superfluous moisture, the acid and alcalizate corpuscles, that were so small, as to swim invisibly in those liquors, will convene into nitrous concretions, whose taste will be compounded of, but very differing from, both the tastes of the acid and tartareous particles; which particles may yet, for the most part, by a skilful distillation, be divorced again. And so, if to a strong solution of pot-ashes, or salt of tartar, you put as much in weight of sal armoniack, as there is of either of those fixed salts contained in the liquor; you may, besides a subtil urinous spirit, that will easily come over in the distillation, obtain a dry *caput mortuum*, which is almost totally a compounded salt, differing enough from either of the ingredients, especially the alcalizate, as well in taste, as in some other qualities: This salt, freed from its fæces, being that diuretick salt, I several years ago gave quantities of to some chemists and physicians, from the most of whom I received great thanks, accompanied with the more acceptable accounts of the very happy success they had employed it with, though usually but in a small dose, as from six, eight, or ten grains, to a scruple. But this being mentioned only upon the by, I shall proceed to tell you, that since I intimated to you already, that I would mention examples of sounds and tastes, only to illustrate what I have been delivering; I shall now add some instances by way of proof, of the coalition and resulting change of steams in the air. It is easily observable in some nosegays, where the differing flowers happen to be conveniently mixed, that in the smell afforded by it, at a due distance, the odours of the particular flowers are not perceived, but the organ is affected by their joint action, which makes on it a confused, but delightful impression. And so, when in a ball of pomander, or a perfumed skin, musk, and amber, and civet, and other sweets, are skilfully mixed, the coalition of the distinct effluvia of the ingredients, that associate themselves in their passage through the air, produce in the sensory one grateful perfume, resulting from all those odours. But if you take spirit of fermented urine, and spirit of wine, both of them phlegmatick, and mix them together, they will incorporate like wine and water, or any other such liquors, without affording any dry concretions. But if you expose them in a convenient vessel but to the mild heat of a bath, or lamp, the ascending particles will associate themselves, and adhere to the upper part of the glass in the form of a white, but tender sublimate, consisting both of urinous and vinous spirits, associated into a mixture, which differs from either of the liquors, not only in consistence, taste, and smell, but in some considerable operations performable by this odd mixture; which this is not the place to take further notice of. And if spirit of salt, and spirit of nitre be, by distillation, elevated in the form of fumes, so ordered, as to convene into one liquor in the receiver, this liquor will readily dissolve crude gold, though neither the spirit of nitre alone, nor that of salt, would do so.

AND that you may have an ocular proof of the possibility of the distinctness and subsequent commixture of steams in the air, I shall now add an experiment, which I long since devised for that purpose, and which I soon after shewed to many curious persons, most of whom appeared somewhat surprized at it. The experiment was; that I took two small vials, the one filled with spirit of salt, but not very strong, the other with spirit of fermented urine, or of sal armoniack very well rectified: these vials being placed at some distance, and not being stopped, each liquor afforded its own smell, at a pretty distance, by the steams it emitted into the air, but yet these steams were invisible. But when these vials, (which should be of the same size) came to be approached very near to each other, though not so, as to touch; as when the two liquors are put together in the form of liquors, they will notably act upon one another; so their respective effluviums meeting in the air, would, answerably to the littleness of their bulk, do the like, and, by their mutual occursions, become manifestly visible, and appear moving in the air like a little portion of smoke, or of a mist, which would quickly cease, if either of the vials were removed half a foot, or a foot from the other. And I remember, that, to add to the oddness of the phænomenon, I sometimes made a drop of the spirit of salt hang at the bottom of a little stick of glass, or some other convenient body, and held this drop thus suspended in the orifice of a vial, that had spirit of sal armoniack in it, and was furnished with a somewhat long neck; for by this means, it happened as I expected, that the ascending urinous particles, though invisible before, invading plentifully the acid ones of the drop, produced a notable smoke, which, if the drop were held a little above the neck of the glass, would most commonly fly upwards to the height of a foot, or half a yard: but if the drop were held somewhat deep within the cavity of the neck, a good part of the produced smoke would oftentimes fall into the cavity of the vial, which was left in great part empty, sometimes in the form of drops, but usually in the form of a slender and somewhat winding stream, of a white colour, that seemed to flow down just like a liquor from the depending drop, till it had reached the spirit of sal armoniack; upon whose surface it would spread itself like a mist. But this only upon the by. As for the main experiment itself, it may be, as I have found, successfully tried with other liquors than these; but it is not necessary, in this place, to give an account of such trials; though perhaps, if I had leisure, it might be worth while to consider, whether these coalitions of differing sorts of steams in the air, and the changes resulting thence of their particular precedent quantities, may not assist us to investigate the causes of divers sudden clouds and mists, and some other meteorological phænomena, and also of divers changes, that happen in the air, in reference to the coming in and ceasing of several either epidemical, or contagious diseases, and particularly the plague, that seem to depend upon some occult temperature and alterations of the air, which may be copiously impregnated by the differing subterraneal (not to add here, sideral) effluviums, that not unfrequently ascend into it, or otherwise invade it, with pestiferous, or other morbifick corpuscles, and sometimes with others of a contrary nature, and sometimes too, perhaps, neither the one sort of steams, which may be supposed to have imbued the air, is in itself deleterious; nor the other salutary, but becomes so upon their casual coalition in the air. You will perhaps think this conjecture of the resultancy of pestilential steams the less improbable, if I here add that odd observation, which was frequently made in the formerly-mentioned plague at *Nimmeguen*, by a physician so judicious as *Diemerbroek*, whose words are these; *Illud notatu dignum sæpissimè observavimus, nempe in illis ædibus, in quibus nulla adhuc pestis erat, si linteamina sordida aquâ & sapone nostrate (ut in Belgio moris est) illic lavarentur, eo ipso die, vel interdum postridie, duos tres-ve simul peste correptos fuisse, ipsique ægri testabantur fœtorem aquæ saponatæ illis primam & maximam alterationem intulisse. Hoc ipsum quoque in meo ipsius hospitio infelix experientia docuit, in quo post lota linteamina statim gravem alterationem perceperunt plærique domestici, & proximè sequenti nocte tres peste correptæ, ac brevi post mortuæ fuere.* I omit the instances he further sets down to confirm this odd phænomenon, of which, though perhaps some other cause may be divided, yet, that I lately assigned, seems at least a probable one, if not the most probable; since, as it is manifest by daily experience, that the smell occasioned by the washing of foul linen with the soap commonly used in the *Netherlands*, produces not the plague; so, by our learned author's observation it appears, either, that there were not yet any pestilential effluxions in the air of those places, which, on the occasions of those washings, became infected, or at least, that by the addition of the fetid effluvia of the soapy water, those morbifick particles, that were dispersed through the air before, had not the power to introduce a malignant constitution into the air, and to act as truly pestilential, till they were enabled to do so, by being associated with the ill-scented effluvia of the soap.

Tract. de Peste, Lib. II. cap. 3.

WHETHER also salutary, and, if I may so call them, alexipharmical corpuscles may not be produced in the air by coalition, might be very well worth our enquiry: especially if we had a competent historical account of the yearly ceasing of the plague at *Grand Cayro*. For, as I have elsewhere noted out of the learned *Prosper Alpinus*, who practised physick there; and as I have also been informed by some of my my acquaintance, who visited that vast city, that almost in the midst of summer, as soon as the river begins to rise *, the plague has its malignity suddenly checked, even as to those, that are already infected, and soon after ceases; so if other circumstances contradict not, one might guess, that this strange phænomenon may be chiefly occasioned by some nitrous, or other corpuscles, that accompany the overflowing nile, and by associating themselves with what *Hippocrates* somewhere calls νοσερὰς ἀποῤῥοίας, disable them to produce their wonted pernicious effects. To which hypothesis suits well what is delivered by more than one traveller into *Egypt*, and more particular by our ingenious countryman Mr. *George Sandys*, who not only takes notice, that about the time of the overflowing of *Nilus*, whose abounding with nitre has been observed even by the antients, there is a certain moistening emanation diffused thorough the air. To prove, says he †, speaking of the overflowing of *Nilus*, that it proceedeth from a natural cause, this one, though strange, yet true experiment will suffice. Take of the earth of *Egypt* adjoining to the river, and preserve it carefully, that it neither come to be wet nor wasted, weigh it daily, and you shall find it neither more nor less heavy until the seventeenth of *June*, at which day it beginneth to grow more ponderous, and augmenteth with the augmentation of the river, whereby they have an infallible knowledge of the state of the deluge, proceeding without doubt from the humidity of the air, which having a recourse through all passable places, and mixing therewith, increaseth the same, as it increaseth in moisture.

THAT these sanative steams perform their effects merely because they are moist, I presume naturalists will scarce pretend; but that they may be of such a nature, as by their coalition with the morbifick corpuscles, to increase their bulk and alter their figure, or precipitate them out of the air, or clog their agility, or pervert their motions, and, in a word, destroy all, or some, at least, of those mechanical affections, which made those corpuscles pestilential: that, I say, these antidotal vapours (if I may so call them) may have these effects upon those, that formerly were morbifick, and that so there may result from the association of two sorts of particles, whereof one was of a highly noxious nature, a harmless mixture, might here be made probable by several things; but that I hope, what I have lately recited about the coalitions of the effluvia of spirit of salt, and of urine, (liquors known to be highly contrary to each other) is not already forgotten by you.

AND the experiment, with which I am to conclude this essay, will, perhaps, make you think it possible, that the pestiferous steams, that have already passed out of the air, and invaded, but not too much vitiated, the bodies of men, may have their malignity much debilitated by the supervening of these antidotal particles. For in that experiment you will find, that the steams emitted into the air from the liquor there described, though that were actually cold, were able to reach, and manifestly to operate, (and that probably by way of precipitation,) upon corpuscles, that were fenced from them by the interposition of other bodies, not more porous than those of living men. Whether the fume of sulphur, which by many is extolled to prevent the infection of the air, do, by its acid, or other particles, disarm, if I may so speak, the pestilential ones, I have not now time to enquire: no more than whether in *Ireland*, and some few other countries, that breed or brook no poisonous animals, that hostility may proceed, at least, in great part, from the peculiar nature of the soil, which both from its superficial and deeper parts, constantly supplies the air with corpuscles destructive to venemous animals. And some other particulars, that may be pertinently enough considered here, you may find treated on in other papers. And therefore at present I shall only intimate, in a word, that having purposely made a visible and lasting stain on a solid body barely by cold effluvia, I did, by the invisible and cold steams of another body, make, in two or three minutes, a visible change in the colour of that stain.

AND as for the other part of the conjecture, viz. that meteors may sometimes be produced by the occursions of subterraneal effluvia, some of them of one determinate nature, and some of another, I think I could, to countenance it, give you divers instances of the plentiful impregnation of the air at some times, and in some places, with steams of very differing natures, and such as are not so likely to be attracted by the heat of the sun, as to be sent up from the subterraneal regions, and sometimes from minerals themselves. But for instances of this kind, I shall, for brevity sake, refer you to another paper ‖, where I have purposely treated of this subject, and particularly shewn, that though usually the effluxions, that come from under-ground, are ill-scented, yet they are not always so; and also, that sulphureous exhalations, even from cold, and, for the most part, aqueous liquors, may retain their determinate nature in the air, and act accordingly upon solid bodies themselves, to whose constitution those effluvia chance to be proportionate.

BUT one memorable story, not mentioned in that discourse, is too much to our present purpose to be here omitted, especially having met with it in so approved an author as the experienced *Agricola*, who having mentioned out of antient historians the raining of white and red liquors, which they took (erroneously I doubt not) for milk and blood, subjoins, § *Ut autem majorem fidem habeamus annalium monumentis facit*

* The plague, which here miserably rageth, upon the first of the flood doth instantly cease; in so much, as when five hundred die at *Cayro* the day before, which is nothing rare (for the sound keep company with the sick, holding death fatal, and, to avoid them, irreligion,) not one doth die the day following; says Mr. *Sandys* in his travels, Lib. II.

† Mr. *Sandys* in the book above-cited.

‖ An Essay of Subterraneal Exhalations.

§ *Agric.* de nat. eorum, quæ effluunt è terra, Lib. XII. pag. 236.

facit res illa decantata, quæ patrum memoriâ (in another place he specifies the year of our Lord) *in* Suevia *accidit*; *aer enim ille stillavit guttas, quæ lineas vestes crucibus rubris quasi sanguineis imbuebant.* Which I the rather mention, because it does not only prove what I alledge it for; but may keep what is lately and very credibly reported to have happened in divers places of the kingdom of *Naples*, soon after the fiery eruption of *Vesuvius*, from being judged a phænomenon either altogether fabulous, (as doubtless many have thought it,) or a prodigy without all example, as is presumed, even by those, that think it not miraculous. And to this I add, that it will be the less improbable, that the more agile corpuscles of subterraneal salts, sulphurs and bitumens, may be raised into the air, and keep distinct natures there, if so fixed a body, as common earth itself, can be brought to swim in the air. And yet of this the worthy writer newly quoted gives us, besides what annals relate, this testimony upon his own knowledge: † *Certè hîc Kempnicii undecimum abhinc annum mense Septembri effluxerunt imbres, sic cum terra lutea commisti, ut eâ passim plateas scilicet stratas viderem conspersas.*

And to shew you, that in some cases the particles even of vegetable bodies may not so soon perish in the air, as they vanish there, but may retain distinct natures at a greater distance, than one would think, from the bodies, that copiously emit them; I shall add, that having desired an ingenious gentleman, that went on a considerable employment to the *East-Indies*, to make some observations for me in his voyage; he sent me, among other things, this remark: that having sailed along the coast of *Ceylon*, (famous for cinnamon-trees and well-scented gums,) though they coasted it almost a whole day, the wind, that then chanced to blow from the shore, brought them a manifestly odoriferous air from the island, though they kept off many miles (perhaps twenty or twenty-five) from the shore. Nor should this be thought incredible, because the diffusion seems so disproportionate to that of other bodies dissolved by fluids; as, for instance, though salt be an active body, and resoluble into abundance of minute particles, yet one part of salt will scarce be tastable in an hundred parts of water. For sensibly to affect so gross an organ, as that of our taste, there is usually required in sapid particles a bigness far exceeding that, which is necessary to the making bodies fit objects for the sense of smelling, and which is here mainly to be considered, there is a great difference between the power a body has to impregnate so thin and fine a fluid as air, whose parts are so rare and lax, and that, which it has to imgregnate liquors, such as water or wine, whose parts are so constipated as to make it, not only visible and tangible, but ponderous. On which occasion I remember, that having had a curiosity to try how far a sapid body could be diluted, without ceasing to be so, I found by trial, that one drop of good chemical, and, as artists call it, essential oil of cinnamon, being duly mixed, by the help of sugar, with wine, retained the determinate taste of cinnamon, though it were diffused into near a quart of wine. So that, making a moderate estimate, I concluded, that upon the common supposition, according to which a drop is reckoned for a grain, one part of oil had given the specifick taste of the spice, it was drawn from, to near fourteen thousand parts of wine. By comparing which experiment, with what I noted about the proportion of salt requisite to make water taste of it, you will easily perceive, that there may be a very great difference, in point of diffusiveness, between the little particles, that make bodies sapid: which may serve to confirm, both some part of the first chapter of the foregoing essay of the subtilty of effluvia, and what I was lately saying, to shew it possible, that antimonial glass might impart store of steams to the emetick wine, without appearing, upon common scales, to have lost of its weight; since we see, that one drop of so light a body as oil, may communicate not insensible effluvia, but tastable corpuscles to near a quart of liquor. But this is not all, for which I mention our experiment: for I must now add, that besides the almost innumerable sapid parts of a spicy drop communicated to the wine, it thence diffused a vast number of odorous particles into the air, which both I, and others, perceived to be imbued with the distinct scent of cinnamon, and which, perhaps, the liquor would have been found able to have aromatized for I know not how long a time, if I had had leisure to prosecute the observation.

CHAP. V.

The third and last way I shall mention of shewing the determinate nature of effluviums, is to be taken from the consideration of their effects upon other bodies, than the organs of our senses; (for of their operations upon these, we have already spoken in the foregoing chapter.) For the effects, that certain bodies produce on others by their effluviums, being constant and determinate, and oftentimes very different from those, which other agents, by their emissions, work upon the same, and other subjects, the distinct nature of the corpuscles emitted may be thence sufficiently gathered.

We may, from the foregoing tract of the subtilty of effluvia, borrow some instances very pertinent to this place. For the temporary benumbedness, or stupefaction, for example, produced in the fisherman's foot by the * effluvia of the fish *Amoreatim*, mentioned by the ingenious *Piso*, manifests, that those stupefying emanations retained a peculiar and venemous nature during their whole passage through the shoe, stocking, and skin, interposed betwixt the fish, and the nervous part of the foot benumbed by it. And though there are very few other bodies in the world, that are minute

† *Agric.* de nat. eorum, quæ è terra effluunt, Lib. XII. pag, 263.
* See the Essay of the Subtilty of Effluviums, Chap. IV.

nute enough to paſs through the pores of glaſs, it is apparent, by the experiment there recited of the oblong iron hermetically ſealed up in a glaſs-pipe, that the magnetical effluvia of the earth may retain their peculiar and wonderful nature in a ſmallneſs, that qualifies them to paſs freely through the pores of glaſs itſelf.

But that I may neither repeat what you have already met with in the foregoing tract, nor anticipate what I have to ſay in the next; I will employ in this chapter ſome inſtances, that may be ſpared from both.

That divers bodies of a venemous nature may exerciſe ſome ſuch operations upon others, by their effluviums tranſmitted through the air, as they are wont to do in their groſs ſubſtance, is a truth, whereof though I have not met with many, yet I have met with ſome examples among phyſicians.

Lib. VI. parte 7. cap. 1. The learned *Sennertus* obſerves, as a known thing, that the apprentices of apothecaries have been caſt into profound ſleeps, when in diſtilling opiat and hypnotick liquors they have received in at their noſtrils the vapours exhaling from thoſe bodies.

It is recorded by the * writers about poiſons, that the root and juice of mandragora caſts thoſe, that take it, into a deep ſopor not unlike a lethargy. And though the apples of the ſame plant be thought to be much leſs malignant; yet *Levinus Lemnius* relates, that it happened to him more than once, that having laid ſome mandrake-apples in his ſtudy, he was by their ſteams made ſo ſleepy, that he could hardly recover himſelf; but the apples being taken away he regained alacrity, and threw off all drowſineſs.

Among all poiſons, there is ſcarce any, whoſe phænomena are in my opinion more ſtrange than thoſe, that proceed from a mad dog; and yet even this poiſon, which ſeems to require corpuſcles of ſo odd and determinate a nature, is recorded by phyſicians to have been conveyed by exhalations. *Aretæus* writes, as a learned modern quotes him, *Quòd à rabido cane, qui in faciem, dum ſpiritus adducitur, tantùmmodò inſpiraverit, & nullo modo momorderit, in rabiem homo agatur.* And as there are relations, among phyſicians, of animals, that have become rabioſi by having eaten of the parts or excrements of rabid animals; ſo

Lib III. Acutor. Morbor. *Cælius Aurelianus*, who writes, that ſome have been made to run mad, not by being bitten, but wounded only with the claws of a mad dog, tells us alſo of a man, that fell into a hydrophobia (which is wont to be a high degree of the rabies, and by ſome of the ancienter writers was employed to ſignify that diſeaſe) without being bitten by a mad dog, but infected *ſolo odore ex rabido cane attracto*. By which odours, in this, and other narratives of poiſons, I underſtand not a bare ſcholaſtick ſpecies, but a ſwarm of effluvia, which moſt commonly are all, or at leaſt ſome of them, odorous. And though it may juſtly ſeem ſtrange to many, that the venom of a mad dog ſhould be communicated otherwiſe than by biting, which is ſuppoſed to be the only way he can infect by, it may appear leſs improbable, becauſe *Matthæus de Gradibus* names a perſon, who, he ſays, proved infected after many days, by only having put his hand into the mouth of a mad dog, who did not bite him. And the formerly mentioned *Matthiolus* relates, that he ſaw two, that were made rabid without any wound by the ſlabber of a mad dog, with which they had the misfortune to be beſmeared.

Sennertus himſelf affirms of a painter of his acquaintance, that, when he had opened a box, in which he had long kept included realgar, a noxious mineral, ſometimes uſed by painters, and not unknown to chemiſts, and had unfortunately ſnuffed in the ſteams of it, he was ſeized with a giddineſs in his head and fainting fits, his whole face alſo ſwelling, though by taking of antidotes he eſcaped the danger. Lib. VI. Part 6. cap. 2.

Divers other examples we have met with in the writings of phyſicians, which I forbear to add to theſe, becauſe, I confeſs, I very much doubt the truth of them, though the deliverers of ſome of them be men of note. But the probability of moſt of the things already cited out of credible authors may be ſtrengthened by what I ſhall now ſubjoin, as a further proof of the diſtinct nature of effluvia; of which it will be a very conſiderable proof, if medicines, which are of a milder and more familiar nature and operation than poiſons, ſhall yet be able in ſome caſes to retain, in their inviſible particles ſwimming in the air, the ſame, though not ſo great, power of purging, which is known to belong to them when their groſs body is taken in at the mouth. Of this I have elſewhere, on another occaſion, given ſome examples. To which I ſhall now add, that I know a doctor of phyſick, that is uſually purged by the odours or exhalations of a certain electuary, whoſe cathartick operation, when it is taken in ſubſtance, is wont to be but languid. And another doctor of my acquaintaince cauſing good ſtore of the root of black hellebore to be long pounded in a mortar, moſt of thoſe, that were in the room, and eſpecially the party that pounded it, were thereby purged, and ſome of them ſtrongly enough. And the learned *Sennertus* ſomewhere affirms, that ſome will be purged by the very odour of colocynthis. And it is not to be paſſed by unregarded, that in the caſes I have alledged, exhalations, that are endowed with occult qualities, (for thoſe of cathartick medicines are reckoned among ſuch) aſcend into the air, without being forced from the bodies they belonged to by an external heat.

And if I would in this place alledge examples of the operations of ſuch effluvia, as do not paſs into the air, but yet operate only by the contact of the external parts of the body, I could give inſtances, not only of the purgative, but the emetick qualities of ſome medicines, exerted without their being taken in at the mouth, or injected with inſtruments.

There are alſo other ſorts of examples, than thoſe hitherto mentioned, that argue a determinate nature in the effluxions of ſome

* In Explicatione Herbarum Biblicarum, cap. 2.

bodies emitted into the air. Approved writers tell us, that the ſhadow of a walnut-tree, with the leaves on it, is very hurtful to the head; and ſome inſtances they give us, of great miſchief it has ſometimes done. And though the ſhadow, as ſuch, is not likely to be guilty of ſuch bad effects; yet the effluvia of the neighbouring plant may be noxious enough to the head. For I, that was not at all prepoſſeſſed with an opinion, that it was ſo, and therefore, without ſcruple, reſorted to the ſhade of walnut-trees in a hot country, was, by experience, forced to think it might give others the head-ach, ſince it did to me, who, thanks be to God, both was, and am ſtill very little ſubject to that diſtemper. And this brings into my mind an obſervation, that I have met with among ſome ingenious travellers into the *Weſt-Indies*, who obſerve in general, and, of late, a countryman of our own affirms it in particular, of the poiſonous manchinello-tree, that birds will not only forbear to eat of the fruit of venemous plants, but, as to ſome of them, will not ſo much as light on the trees: which I therefore mention, becauſe, probably, nature inſtructs them to avoid ſuch trees by ſome noxious ſmell, or other emanation, that offends the approaching birds. And I remember, that ſome of our navigators give it for a rule to thoſe, that happen to land in unknown iſlands, or coaſts, that they may venture to eat of thoſe parts of fruits which they can perceive the birds, like kind taſters, to have been pecking at before.

NICOLAUS FLORENTINUS (cited by *Sennertus*) tells us of a certain Lombard, that having in a houſe, that he named, at *Florence*, burned a great black ſpider at the flame of a candle, ſo unwarily, that he drew in the ſteams of it at his noſtrils, preſently began to be much diſordered, and fell into a fainting fit, and, for the whole night, had his heart much diſaffected, his pulſe being ſo weak, that one could ſcarce perceive he had any; though afterwards he was cured by treacle, diamoſc, and the powder of zedoary, mixed together.

AND I remember, that being ſome years ago in *Ireland*, I gathered a certain plant (peculiar to ſome parts of that country) which the natives call Maccu-buy, becauſe of ſtrange traditions, that go about it; the chief of which I found, by trial, not to be true: but yet being ſatisfied, that its operations were odd, and violent enough, I was willing to gratify the chief phyſician of the country, who was deſirous I ſhould propoſe to him ſome ways of correcting it; and whilſt I was ſpeaking of one, that required the pounding of it, he told me on that occaſion, that intending to make an extract of it with vinegar, he cauſed his man to beat it well in a mortar, which the man ſoon repented he had begun to do: and the doctor himſelf, though at a pretty diſtance off, was ſo wrought upon by the corpuſcles, that iſſued out into the air, that his head, and particularly his face, ſwelled to an enormous and diſfiguring bulk, and continued tumid for no inconſiderable time after.

I have not leiſure to ſubjoin many more inſtances, to ſhew the determinate nature of effluviums, ſmall enough to wander through the air; nor perhaps will it be neceſſary, if you pleaſe but to conſider theſe two things. The firſt, that many odoriferous bodies, as amber, muſk, civet, &c. as they will, by the adheſion of their whole ſubſtance, perfume ſkins, linen, &c. ſo they will, in time, perfume ſome bodies diſpoſed to admit their action, though kept at a diſtance from them. And the other is, that in peſtilential fevers, and divers other contagious ſickneſſes, as the plague, ſmall-pox, or meaſles, the ſame determinate diſeaſe is communicable to ſound perſons, not only by the immediate contact of the infected party, but without it, by the contagious ſteams, that exhale from his body into the air. And having ſaid this, and deſired you to reflect upon it, I ſhall conclude this chapter with an experiment, that, poſſibly, will not a little confirm a great part of it.

CONSIDERING then with myſelf, how I might beſt deviſe a way of ſhewing to the very eye, that effluvia, elevated without the help of heat, and wandering in the air, may both retain their own nature, and, upon determinate bodies, produce effects, that a vulgar philoſopher would aſcribe to occult qualities; I remembred, that I had found by trials (made to other purpoſes) that volatile and ſulphureous ſalts would ſo work upon ſome acid ones, ſublimed with mercury, as to produce an odd diverſity of colours, but chiefly an inky one; on which account, I judged it likely, that my aim would be anſwered by the following experiment.

I took an ounce, or better, of ſuch a volatile tincture of ſulphur, as I have elſewhere * taught you to make of quick-lime, ſulphur, and ſal armoniack, and ſtopped it up in a vial capable of containing at leaſt twice as much; then taking a paper, whereon ſomething had been written with inviſible ink, I laid it down ſix inches off of the vial, which being unſtopped, began, upon the acceſs of the fire, to emit white fumes into it, and by theſe, what was written upon the paper, notwithſtanding its diſtance from the liquor, quickly became very legible, though not quite ſo ſuddenly, as if a paper, written with the ſame clear liquor, were held at the like diſtance directly over the orifice of the vial. And having cauſed ſeveral pieces of clean paper to be written on, with a new pen dipped in the clear ſolution of ſublimate, made in water, it was pleaſant to ſee, how divers of the letters of ſeveral of theſe papers, being placed within ſome convenient diſtance of the vial, would be made plainly legible, and ſome of them more, ſome leſs blackiſh, according to their diſtances from the ſmoking liquor, and other circumſtances. But it was more ſurprizing to ſee, that when I held, or laid ſome of theſe

* The liquor here mentioned is, for the main, the ſame with that deſcribed by the author in his book of Colours.

these papers, though with the written ſide upwards, juſt upon, or over the orifice of the vial, though the contained liquor did not, by ſome inches, reach ſo high, yet the latent letters would become not only legible, but conſpicuous, in about a quarter of a minute of an hour, meaſured by a good watch fit for the purpoſe, as more than one trial aſſured me. And as it may be obſerved, that in ſome circumſtances the ſmoking liquor, and the ſolution of ſublimate, will make an odd precipitate, almoſt of a ſilveriſh colour, ſo in one, or two of our trials, we found a like colour produced, by the ſteams of that liquor, in ſome of the colourleſs ink. Nor is it ſo neceſſary to employ a viſibly ſmoking liquor, for the denigrating of inviſible ink at a diſtance. For I have, to that purpoſe, with good ſucceſs, though not equal to that I have recited, employed a couple of liquors, wherein there was neither ſulphur, nor ſal armoniack, nor ſublimate. What other trials I made with our volatile tincture of ſulphur, it is not neceſſary here to relate; only one experiment, which you will poſſibly think odd enough, I ſhall not omit; becauſe it will not only confirm the precedent trials, but alſo much of the foregoing eſſay, by ſhewing the great ſubtilty, and penetrating power of effluviums, that ſeem rather to iſſue out very faintly, than to be darted out with any briſkneſs.

Causing then ſomething to be written with diſſolved ſublimate upon a piece of paper, we folded the paper with the written ſide inwards, and then incloſed this in the midſt of ſix ſheets of paper, laid one upon another, not placed one within another, and folded up in the form of an ordinary letter, or packet, to be ſealed, that the edges of the encloſing paper, being inſerted one within the other, the fumes might not get into this written paper, but by penetrating through the leaves themſelves: this done, that ſide of the packet, on which there was no commiſſure, and on which, were it to be ſent away, the ſuperſcription ſhould be written, was laid upon the orifice of the vial, which (as was before intimated) was ſome inches higher than the ſurface of the liquor, and left there about ten minutes; after which, taking off the folded papers, and opening them, we found, that the ſteams had pervaded all the leaves, in which the written paper had been encloſed. For, though the leaves did not appear ſtained or altered, yet the formerly latent characters appeared conſpicuous. I have not time to diſcourſe, whether, and how far, this experiment may aſſiſt us to explain ſome odd effects of thunder, or of that ſtrange phænomenon, (glanced at in the foregoing chapter,) which is ſaid to have happened lately in the kingdom of *Naples*, after the great eruption of *Veſuvius*, which is ſaid to have been followed by the appearing of the croſſes formerly mentioned, ſome of which have been found on the innermoſt parts of linen, that had been carefully folded up. But of theſe, and the like things, I ſay, I have now no time to diſcourſe, whether any thing derivable from our experiment may be pertinently applied to their explication. For which reaſon, I ſhall add no more, than that afterwards, for further trial, we took a printed book, that chanced to be at hand, and which we judged the fitteſt for our purpoſe, becauſe the leaves being broad, they might the better preſerve a ſmall paper to be placed in the midſt of them, from being acceſſible to the exhalations ſide-wiſe, and having put the deſigned paper into this book, and held it to the orifice of the vial, though there were no leſs than twelve leaves between them, yet thoſe letters, that happened to be the moſt rightly placed, were made inky in the ſhort ſpace of three minutes, at the utmoſt; though this liquor had been ſo long kept, and ſo often unſtopped to try concluſions with it, that it had probably loſt a good part of the moſt ſpirituous and piercing particles.

NEW EXPERIMENTS

TO MAKE

FIRE AND FLAME

STABLE AND PONDERABLE.

A PREFACE; ſhewing the Motive, Deſign, and Parts of the enſuing TRACT.

THE inducements, which put me upon the attempt, expreſſed in the title of this eſſay, were chiefly theſe:

FIRST, I conſidered, that the interſtellar part of the univerſe, conſiſting of air and æther, or fluids analogous to one of them, is diaphanous; and that the æther is, as it were, a vaſt ocean, wherein the luminous globes, that here and there, like fiſhes, ſwim by their own motion, or like bodies in whirlpools are carried about by the ambient, are but very thinly diſperſed, and conſequently, that the proportion, that the fixed ſtars, and planetary bodies, bear to the diaphanous part of the world, is exceeding ſmall, and ſcarce conſiderable, though we ſhould admit the ſun, and fixed ſtars, to be opacous bodies, upon the account of their terminating our ſight: which diffident expreſſion I employ, becauſe I have elſewhere ſhewn, by two or three experiments, purpoſely deviſed, that a body may appear opacous to our eyes, and yet allow free paſſage to the beams of light.

I further conſidered, that there being ſo vaſt a diſproportion between the diaphanous part of the world, and the globes, about which it is every way diffuſed, and with which it is ſometimes in great portions mingled, as in the water, which, together with the earth, makes up the globe we inhabit; and the nature of a diaphanous body's being ſuch, that when the ſun, or any other luminous body, illuſtrates them, that, which we call light, does ſo penetrate, and mix itſelf *per minima*, with them, that there is no ſenſible part of the tranſparent body unenlightened; I thought it worth the enquiry, whether a thing, ſo vaſtly diffuſed as light is, were ſomething corporeal, or not? and whether, in caſe it be, it may be ſubjected to ſome other of our ſenſes, beſides our ſight, whereby we may examine, whether it hath any affinity with other corporeal beings, that we are acquainted with here below?

I did not not all this while forget, that the Peripateticks make light a meer quality, and that *Carteſius* ingeniouſly endeavours to explicate it, by a modification of motion in an ætherial matter: but I remembered too, that the Atomiſts of old, and of late the learned *Gaſſendus*, and many other philoſophers, aſſert light to be corporeal; and, that ſome years ſince, though I declined to paſs my judgment about the queſtion, yet I had employed arguments, that appeared plauſible enough to ſhew, that it was not abſurd to ſuppoſe, that the ſun, which is the fixed ſtar moſt known to us, might be a fiery body. And therefore doubting, whether the corporeity of light would be in haſte determined by meer ratiocinations, I thought it very well worth the endeavouring to try, whether I could do any thing towards clearing the diſpute of it by experiments; eſpecially being perſuaded, that, though ſuch an attempt ſhould be ineffectual, it would but leave the controverſy in its former ſtate, without prejudicing either of the contending hypotheſes; and yet, if it ſhould prove ſucceſsful, the conſequences of it would be very great and uſeful towards the explicating of divers phænomena in divers parts of natural philoſophy, as in chemiſtry, botanicks, and (if there be any ſuch) the allowable part of aſtrology. (Nor perhaps would it be impoſſible, by the help of ſlight theorical alterations, to reconcile the experiments, I deſigned, to either of the above-mentioned hypotheſes, and ſo, as to the explication of light, to one another.)

To compaſs then, what I aimed at, I thought it was fit, in the firſt place, to try what I could do by the union of the ſun-beams, they being on all hands confeſſed to be portions (as I may ſo ſpeak) of true and celeſtial light: and then I thought fit to try, what could be obtained from flame; not only, becauſe that is acknowledged to be a luminary, but becauſe I hoped the difficulties, I foreſaw in the other trials, might be, in ſome meaſure, avoided in thoſe made with flame; and if both ſorts of them ſhould ſucceed, the latter and former would ſerve to confirm each other. According to the method

method I proposed of handling these two subjects, I should begin with some account of what I attempted to perform in the sun-beams. But the truth is, that when I chanced to fall upon the enquiry, that occasioned this paper, besides that the time of the year itself was not over-favourable, the weather proved so extraordinary dark, and unseasonable, that it was wondered at; so that, though I was furnished with good burning-glasses, and did several times begin to make trials upon divers bodies, as lead, quick-silver, antimony, &c. yet the frequent interposition of clouds, and mists, did so disfavour my attempts, that, however they were not all alike defeated, yet I could not prosecute the greatest part of them to my own satisfaction. And therefore, being unwilling to build on them as yet, I shall reserve an account of them for another opportunity; and now proceed to the mention of that sort of experiments, which depending less on casualties, it was more in my power to bring to an issue.

I know, I might have saved both you, and myself, some time and pains, by omitting several of these trials, and by a more compendious way of delivering the rest. But I rather chose the course I have taken; partly, because the novelty, and improbabilities of the truth I deliver, seem to require, that it be made out by a good number of trials; partly, because I thought it might not be altogether useless to you, and your friends, to see upon what inducements the several steps were made in this enquiry; partly, because I was willing to contribute something towards the history, that now, perhaps, will be thought fit to be made of the increment, or decrement, that particular bodies may receive by being exposed to the fire; and partly, in fine, because the incongruity of the doctrine here asserted to the opinions of the schools, and the general prepossessions of mankind, made me think it fit, by a considerable variety, as well as number of experiments, to obviate, as far as may be, the differing objections, and evasions, wherewith a truth, so paradoxical, may expect to be encountered.

NEW EXPERIMENTS, &c.

THOUGH there be among the following trials, a diversity, that invites me, as to rank them into four or five differing sorts, so to assign them as many distinct sections; yet, for the conveniency of making the references, there will be occasion to make betwixt them, I shall wave the distinction, and set them down in one continued series.

AND because I am willing to comply with my haste, as well as to deal frankly, and without ceremony, with you, I shall venture to subjoin the naked transcrips of my experiments, as I had in an artless manner set them down, with many others, for my own remembrance, among my *adversaria*, without so much as retrenching some circumstances, that relate less to my present argument, than to some other purposes.

I shall then begin with the mention of a couple of experiments, which, though they might conveniently enough be referred to another paper, yet I shall here set them down, because it seems very proper to endeavour to shew in the first place, that flame itself may be, as it were, incorporated with close and solid bodies, so as to increase their bulk and weight.

TRIALS of the First Sort.

EXPERIMENT I.

[A PIECE of copper-plate not near so thick as a half-crown, and weighing two drachms and twenty-five grains, was so placed, with its broad part horizontal, in a crucible, whose bottom had a little hole in it, for fumes to get out at, that it could not be removed from its position, nor be easily made to drop down, or lose its level to the horizon, though the crucible were turned upside down: then about an ounce and half of common sulphur being put into a taller and broader crucible, that, wherein the copper stuck, was inverted into the orifice of it, that the sulphur being kindled, the flame, but not the melted brimstone in substance, might reach the plate, and have some vent beyond it at the above-mentioned hole. This brimstone burned about two hours, in which time it seemed all to have been resolved into flame, no flowers of sulphur appearing to have sublimed into the inside of the upper crucible; and though the copper-plate were at a considerable distance from the ignited sulphur, yet the flame seemed to have really penetrated it, and to have made it visibly swell, or grow thicker; which appeared to be done by a real accession of substance; since, after we had wiped off some little adhering sordes, and with them divers particles of copper, that stuck close to them, the plate was found to weigh near two and thirty grains more than at first, and consequently to have increased its former weight by above a fifth part.]

EXPERIMENT II.

[HAVING, by refining one ounce of sterling silver with salt-petre, according to our way, reduced it to seven drachms or somewhat less; we took a piece of the thus purified silver, that weighed one drachm wanting two grains, and having ordered it as the copper-plate had been in the former experiment, after the flame of above one ounce and a quarter of sulphur, (that quantity chancing to be suitable to the capacity of the crucible) had, for about an hour and a half, beat upon it, the silver-plate seemed to the eye somewhat swelled, and the lower surface of it, that was next the flame, was brought to a great smoothness, the weight being increased to one drachm five grains and three quarters; which increase of weight falling so short of that, which was gained by the copper, I leave it to you to consider, whether the difference may be attributed to the closeness and compactness of the silver, argued by its being heavier in specie than copper; or to the greater congruity of the pores of copper to be wrought on by the fiery menstruum; or to some other cause.]

IF you should here ask me, by what rational inducements I could be led to entertain so extravagant an expectation, as, that such a light and subtile body as flame should be able to give an augmentation of weight to such ponderous bodies as minerals and metals; I shall now, to avoid making anticipations here, or needless repetitions hereafter, return you only this answer; that the expectation you wonder at, may justly be entertained upon the same, or such like inducements, as you may easily discover in another paper, entitled *Corollarium Paradoxum*. For, supposing upon the grounds there laid, that flame may act upon some bodies as a menstruum, it seems no way incredible, that, as almost all other menstruums, so flame should have some of its own particles united with those of the bodies exposed to its action: and the generality of those particles being, (as it is shewn in the paradox about the fewel of flames,) either saline, or of some such piercing and terrestrial nature, it is no wonder, that being wedged into the pores, or being brought to adhere very fast to the little parts of the bodies exposed to their action, the accession of so many little bodies, that want not gravity, should, because of their multitude, be considerable upon a balance, whereon one or two, or but few of these corpuscles, would have no visible effect.

I could here, if it were expedient, mention some odd scruples about the preceding experiments, and some also of the subsequent; but, lest you should, with some other of my friends, upbraid me with being too jealous and sceptical, I will not trouble you with them; but proceed to the next sort of trials, wherein, though the matter were not always manifestly beaten on by a shining flame; yet it was wrought on by that, which would be called flame, by those, who take not that word strictly, but in a latitude, and which this igneous substance may more properly be stiled, than it can be called common fire, this being visibly harboured in burning coals, or other gross materials, from which our metals were fenced. And I have elsewhere shewn, by experiment, that visibility is not in all cases necessary to actual flame, particularly, when the eye receives a predominant impression from another light.

TRIALS of the Second Sort.

EXPERIMENT III.

INTO a crucible, whose sides had been purposely taken down to make it very shallow, was put one ounce of copper-plates; and this being put into our cupelling-furnace, and kept there two hours, and then being taken out, we weighed the copper (which had not been melted) having first blown off all the ashes, and we found it to weigh one ounce and thirty grains.

EXPERIMENT IV.

[SUPPOSING that copper, being reduced to filings, and thereby gaining more of superficies in proportion to its bulk, would be more exposed to the action of the fire, than when it is in places, as it was formerly, we took an ounce of that metal in filings, and putting them upon a very shallow crucible, and under a muffler, we kept them there about three hours, (whilst other things, that required so long a time were cupelling;) and afterwards taking them off, we found them of a very dark colour, not melted, but caked together in one lump, and encreased in weight (the ashes and dust being blown off) no less, than about forty-nine grains. Part of which increment, above that obtained by the copper-plates in the former experiment, may not improbably be due to the longer time, that in this experiment the filed copper was kept in the fire.]

EXPERIMENT V.

[BEING willing to see, whether calcined hart's-horn, that I did not find easy to be wrought on by corrosive menstruums, would retain any thing of the flame, or fire, to which it should be exposed; we weighed out one ounce of small lumps of hart's-horn, that had been burnt till they appeared white, and having put them into a crucible, and kept them in a cupelling-furnace for two hours, whilst some metals were driving off there by the violence of the fire; we found, that when they were taken out, they had lost six or seven grains of their former weight; perhaps either because, notwithstanding the external whiteness of the lumps, the internal parts of some of them might not be so exquisitely calcined, but retain some oleaginous or other volatile substance; or because, having omitted to ignite them well before they were weighed, they may have since their first calcination imbibed some moist particles of the air. Which conjecture seemed the likelier, because having kept them a while in the scales they were weighed in, they did within two or three hours make it somewhat preponderate. On which occasion I shall add, that, at the same time, with the hart's-horn we put in one ounce of well-heated brick,

and

and kept that likewise in the furnace for above two hours; at the end of which, weighing it whilst it continued hot, we did not find it to have either sensibly got or lost; but, some time after, it seemed upon the ballance to have imbibed some, though but very little, moisture from the air.]

EXPERIMENT VI.

[UPON a good cupel we put one ounce of English tin of the better sort, and having placed it in the furnace under a muffler, though it presently melted, yet it did not forsake its place, but remained upon the concave surface of the cupel, till at the end of about two hours, it appeared to have been well calcined; and then being taken out and weighed by it self, the ounce of metal was found to have gained no less than a drachm.]

EXPERIMENT VII.

[AN ounce of lead was put upon the cupel, made of calcined hart's-horn, and placed under the muffler, after that the cupel was first made hot, and then weighed. This lead did not enter into the cupel, but was turned into a pretty kind of litharge on the top of it, and broke the cupel, whereby some part of the cupel was lost in the furnace, and yet the rest, together with the litharge, weighed seven grains more than the ounce of lead and the heated cupel did, when they were put in.]

BUT because, though this trial shewed, that some weight was gained either by the metal or cupel, or both, yet it did not by this appear, what either of them acquired; it seemed fit to subjoin a further trial.

EXPERIMENT VIII.

[WE took a cupel about two ounces in weight, made of about ten parts of bone-ashes, and one of charcoal-ashes, made up together with ale. This was by itself put in a cupelling-furnace, under a muffler; and the laborant, well versed in weighing, was ordered to take it out, when it was throughly and highly heated, and to weigh it whilst it was in that condition, I being then present: this being done, it was forthwith placed again under the muffler, where some metalline bodies were cupelling, and kept there for about two hours; at the end of which time it was taken out red-hot, and presently put into the same ballance, as before, which was already fastened to a gibbet; where having caused the adhering ashes to be blown off, I found, that whereas, when it was first taken from under the muffler, we had but two ounces and two grains, now the same weight being put into the opposite scale, it had gained very near one and twenty grains. And here note, that it was not without some cause, that I was careful to have the cupel weighed red-hot. For I had a suspicion, that, notwithstanding the dryness of the bone, it might receive some little alteration of weight by imbibing some little particles wandering in the air; which suspicion the event justified. For leaving the cupel counterpoised to cool in the balance, in a short time it began sensibly to preponderate; and suffering it to continue there nine or ten hours, till we had occasion to use the balance, I found it at the end of that time to be about three grains heavier than before.]

THIS was not the only trial we made about the augmenting the weight of cupels; but this being the fairest, and exempt from those mischances, from which the other were not altogether free; I shall content myself to have set down this: in the mention of which I thought fit to take notice of the increase of the weight of the cupel after it had lain in the scales, and also that we weighed it at first, whilst it was throughly hot, because those circumstances, as not being suspected, may easily be left unthought on, even by skilful experimenters; and yet the weighing of the cupel, when it had been well nealed, and the not weighing it soon enough after it is taken from the fire, may keep those, that shall reiterate this experiment, from making it cautiously and accurately enough. For if the former circumstance be omitted, that, which the cupel may seem to have lost of its substance, was nothing but the adventitious moisture of the air; and if the latter circumstance be neglected, the weight it may seem to have gained from the fire, was indeed due to the waterish particles of the air. I could wish also, that trial were made, whether the success would be the same in cupels made in differing sorts of bone-ashes, and other materials, wont to be employed for that purpose. For that I had not opportunity to do.

EXPERIMENT IX.

IRON being a metal, that experience had informed me will more easily be wrought on by fluids, that have particles of a saline nature in them, than is commonly believed; it was not unreasonable to expect, that flame would have a greater operation on it, (especially if it were before-hand reduced to small parts) than on any of the bodies hitherto described. Which supposition will be confirmed by the short ensuing note.

[FOUR drachms of filings of steel being kept two hours on a cupel under a muffler, acquired one drachm six grains and a quarter increase of weight.]

EXPERIMENT X.

[A Piece of silver, refined in our own laboratory, being put upon a cupel under a muffler, and kept there for an hour and half, whilst other things were refining, was taken out and weighed again, and, whereas before it weighed three drachms, thirty-two grains and a quarter, it now weighed, in the same scales, three drachms, thirty-four grains and a half, or but little less.]

FINDING this memorial among divers others about the weight of bodies, exposed to the fire, I thought it not amiss to annex it in this

this place; though finding it to be but single, I would not have it to be relied on, till further trial have been made to discover, whether it was more than a casual and anomalous experiment; and if the silver had not been refined, I should have suspected, that the copper, that was blended with it, as it is usually blended with common silver, might have occasioned the increase of weight.

POSTSCRIPT.

SINCE the foregoing experiment was first set down, meeting with an opportunity to reiterate the trial once more, we did it with half an ounce of filings of silver, well refined with lead in our own laboratory, and kept it about three hours upon the cupel; after the end of which time taking it out, we found it to be of a less pleasant colour than it was of before, and melted (though not so perfectly) into a lump, which weighed four drachms and six grains; and yet, the success being so odd, and, if it prove constant, of such moment, I could wish the trial were further repeated in differing quantities of the metal.

EXPERIMENT XI.

[WE took a drachm of filings of zink or spelter, and having put it upon a cupel under a muffler, we kept it there in a cupelling-fire about three hours, (having occasion to continue the cupellation so long for other trials;) then taking it off the cupel, we found it to be caked into a brittle and dark coloured lump, which looked as if the filings had been calcined. This being weighed in the same scales gained full six grains, and so a tenth part of its first weight.]

EXPERIMENT XII.

AMONG our various trials upon common metals, we thought fit to make one or two upon a metal brought us from the *East-Indies*, and there called Tutenâg, which name being unknown to our European chemists, I have elsewhere endeavoured to give some account of the metal it self; whence I shall borrow the ensuing note, as directly belonging to our present purpose.

[Two drachms of filings of tutenâg being put upon a cupel, and kept under the muffler, for about two hours, the filings were not melted into a lump of metal, but looked as if ceruss and minium being powdered had been mingled together; some of the parts appearing distinctly white, and others red: the calx being put into the balance appeared to have gained twenty-eight grains and a quarter. Another time the experiment being re-iterated with the like circumstances, we found, that two drachms of the filed tutenâg gained the like increase of weight, abating less than one grain.]

So that this Indian metal seems to have gained more in the fire, in proportion to its weight, then any we have hitherto made trial of.

EXPERIMENT XIII.

[BEING desirous to confirm, by a clear experiment, what I elsewhere deliver contrary to the vulgar opinion of those, that believe, that in all cupellations almost all the lead, that is employed about them, does, together with the baser metals, that are to be purged off from the silver or gold, fly away in smoke, as indeed in some sort of cupellations a good proportion may be blown off that way: we took two ounces of good lead, and one drachm of filings of copper, and having caused a cupel to be ignited, and nimbly taken out of the furnace, and weighed, whilst it was very hot, it was presently put back, together with the two metals laid on it, into the cupelling-furnace, where having been kept for about two hours, it was taken out again, and it was found, according to what (as I elsewhere * note) uses to happen in such circumstances, to have nothing on the surface of it worth weighing distinctly in the scales, in which the cupel, with what was sunk into it, amounted to four ounces, three drachms and eleven grains, which wanted but nine grains of the whole weight of the cupel and the two metals, when they were all three together committed to the fire.] So that, though we make a liberal allowance for the increment of weight, that may with any probability, be supposed to have been attained by the cupel, and what was put upon it, yet it will easily be granted, that very much the greater part of the metals was not driven off in fumes, but entered into the substance of the cupel.

TRIALS of the Third Sort.

AFTER having shewn, that either flame, or the analogous effluxions of the fire, will be, what chemists would call, corporified with metals and minerals exposed naked to its action; I thought it would be a desirable thing to discover, whether this flame, or igneous fluid, were subtile enough to exercise any such operation upon the light bodies, sheltered from its immediate contact, by being included in close vessels; but it being very difficult to expose bodies in glasses, to such vehement fires, without breaking or melting the glass, and thereby losing the experiment; I thought fit, first, to employ crucibles carefully luted together, that nothing might visibly get in or out; and of that attempt, I find among my notes, the following account.

EXPERIMENT XIV.

[WE took an ounce of steel, freshly filed from a lump of that metal, that the filings might not be rusty, and having included them betwixt two crucibles, as formerly, kept

* Essay the Sixth of the Usefulness of Natural Philosophy.

kept them for two hours in a ſtrong fire, and ſuffered them to continue there till the fire went out; the crucibles being unluted, the filings appeared hard caked together, and had acquired a dark colour, ſomewhat between black and blue, and were increaſed five grains in weight.]

THE foregoing experiment being the firſt I mention of this kind, it will not be amiſs to confirm it, by annexing the following memorial.

[AN ounce of filings of ſteel being put between the crucibles luted together, after they had been kept about an hour and half in the fire, were taken out, and being weighed, were found to have gained ſix grains.]

EXPERIMENT XV.

[TWO ounces of copper-plate were put into a new crucible, over which a leſſer was whelmed, and the commiſſures were cloſed with lute, that nothing might fall in. After the ſame manner, two ounces of tin were included betwixt crucibles, and alſo two ounces of lead; theſe being put into the cupelling-furnance, were kept in a ſtrong fire about an hour and a half, while ſomething elſe was trying there. And then being taken out, the event was, that the copper-plates, though they ſtuck together, were not quite melted, and ſeemed, ſome of them, to have acquired ſcales like copper put into a naked fire, and the two ounces had gained eight grains in weight. The lead had broke through the bottom of the crucible, and thereby hindered the deſigned obſervation. The tin acquired ſix grains in weight, and was, in part, brought to a pure white calx, but much more of it was melted into a lump of a fine yellow colour, almoſt like gold, but deeper.] The proſecution of this trial, as to the copper-plates, you will meet with in Experiment XXI. to which I therefore refer you.

N. B. BECAUSE lead, in cupellation, enters the cupel, we were willing to try, if we could ſo far hinder it from doing ſo, as to make ſome eſtimate, what change of weight the operation of the fire would make in it: and therefore being able already to make a near gueſs, how much a quantity of tin may gain by being calcined on a cupel, and remembering alſo, from ſome of my former trials, the indiſpoſition, which tin gives lead to cupellation, we mixed a drachm of tin with two ounces of lead, and expoſing the mixture (in a cupel) to the fire under a muffler, we firſt brought it to fuſion, and then it ſeemed at the top dry and ſwelled, and diſcoloured; notwithſtanding which, having continued the operation a good while, becauſe of other things, that were to be done with the ſame fire, we were not lucky enough to bring the experiment to an iſſue worth the relating here, in reference to the ſcope above-propoſed, though in relation to another, the ſucceſs was welcome enough.]

EXPERIMENT XVI.

[SUPPOSING, that if copper were beaten into thinner plates than thoſe we lately uſed, and kept longer in the fire, this would have a more conſiderable operation upon them, we took one ounce of very thinly hammered pieces of copper, and putting them betwixt two crucibles (one whelmed over another) as in Experiment XV. with ſome lute at the corners of the juncture, to keep the fire from coming immediately at the metal, we kept them in the cupelling-furnace about three hours, and then disjoining the veſſels, we found the metal covered with a dark and brittle ſubſtance, like that deſcribed in the above-recited experiment. Which ſubſtance, when ſcaled off, diſcloſed a finely coloured metal, which, together with theſe burned ſcales, amounted to one and twenty grains above the weight, that was firſt put in.]

IF, when theſe things were doing, I had been furniſhed with a very good lute, which is no ſuch eaſy thing to procure, as chemiſts, that have not frequently employed vulgar lutes, are wont to think; I would have made a trial of the enſuing experiment, for a good while, in the naked fire, notwithſtanding that divers metalline minerals will ſcarce be brought to fuſion in glaſſes, eſpecially without ſuch a fire, whoſe violence makes them break the veſſels. For I thought, that by making a fit choice of the metals to be employed, I could prevent that inconvenience; but wanting the accommodations I deſired, and yet preſuming, that in a ſand-furnace, I might by degrees adminiſter heat enough to melt ſo fuſible a metal as fine tin, and keep it in fuſion; I reſolved to make ſome trials, firſt upon that, and then upon another metal. For though I was not ſure of being then able to proſecute the experiment far enough; yet I hoped, I might, at leaſt, ſee ſome effects of my firſt trial, which would enable me to gueſs, what I was to expect from a compleat one.

EXPERIMENT XVII.

[WE took then a piece of fine block-tin, and in a pair of good ſcales weighed out carefully half a pound of it: this we put into a choice glaſs-retort, and kept it for two days, or thereabouts, in a ſand-furnace, which gave heat enough to keep the metal in fuſion, without cracking the glaſs. Then taking out the mixture, we carefully weighed it in the ſame ſcales, and found the ſuperficies a little altered (as if it were diſpoſed to calcination) and the weight to be increaſed about two grains, or ſomewhat better.]

EXPERIMENT XVIII.

[THE other experiment, I tried in glaſſes, was with mercury, hoping, that, if I could make a precipitate *per ſe*, in a hermetically ſealed

sealed glass, I should, by comparing the weight of the precipitate, and the quick-silver, that afforded, have a clear experiment to my purpose; and I should have no bad one, if I could but make it succeed with a glass, though not sealed, yet well stopped; instead of those infernal glasses (as they call them) which are commonly used, and wont to be left open (though some slightly stop them with a little paper, or cotton:) but though, partly, that I might a little diversify the experiment, and make it the more likely to succeed in one or other of the glasses, I divided the mercury, and distributed it amongst several of them, and but a little to each, the success did not answer expectation, the hermetically sealed glasses being unluckily broken; and the precipitation in the others proceeding so slowly, that I was, by a remove, obliged to leave the trial imperfect: only I was encouraged (in case of a future opportunity) to renew it another time, by finding, that most of the glasses, though tall, and stopped with fit corks, afforded some very fair precipitate, but not enough to answer my design.]

TRIALS of the Fourth Sort.

MOST of the experiments hitherto recited having been made, as it were, upon the by with others, whose exigencies it was fit these should comply with; very few of the exposed bodies were kept in the cupelling-fire above two hours, or thereabouts. Upon which account, I thought fit to try, how much some bodies, that had been already exposed to the fire, would gain in weight, by being again exposed to it; especially considering, that most calcinable bodies, (for I affirm it not of all,) which yield rather calces, than ashes, by being without additament reduced in the fire to fine powder, seemed to be by that operation opened, or (as a chemist would speak) unlocked, and therefore, probably, capable of being further wrought upon, and increased in weight, by such a menstruum, as I supposed flame, and igneous exhalations to be. And about this conjecture, I shall subjoin the ensuing trials.

EXPERIMENT XIX.

ONE ounce of calx of tin, that had been made *per se*, for an experiment in our own laboratory, being put in a new cupel, and kept under the muffler for about two hours, was taken out hot, and put into the scales, where the powder appeared to have gained in weight one drachm, and thirty-five grains, by the operation of the fire, which made it also look much whiter than it did before, as appeared by comparing it with some of the calx, that had not been exposed to the second fire: no part of the putty was, as we could perceive, melted by the vehemence of the fire, much less reduced into metal.]

EXPERIMENT XX.

[OUT of a parcel of filings of steel, that had been before exposed to the fire, and had its weight thereby increased some grains, not scruples; we took an ounce, and having exposed it at the same time with the calx of tin, and, for the same time, kept it in the fire, we took it out at the two hours end; and found the weight to be increased two drachms, and two and twenty grains. The filings were very hard baked together, and the lump being broken, looked almost like iron.]

EXPERIMENT XXI.

THE following experiment, though it may seem in one regard but a continuation of the fifteenth, yet it has in this something peculiar from all the foregoing, that not only it affords an instance of the increase of weight obtained by a metal at the second time of its being exposed to the fire, but shews also, that such an increment may be had, though this second ignition be made in close vessels.

[SOME of the copper mentioned in Experiment XV. being accidentally lost, one ounce and four drachms of what remained was included betwixt two crucibles and exposed to a strong fire for two hours, and suffered to continue there till the fire went out: when it was taken out, it appeared to have gained ten grains in weight, and to have upon the superficial parts of the plates (as we observed) divers dark-coloured flakes, some of which stuck to the metal, but more, upon handling it, fell off.]

AND here I shall conclude one of the two parts of our designed treatise: for, though, I remember, that these were not all the trials, that were made, and set down, upon the subject hitherto treated of; yet these are the chief, that having escaped the mischances, which befel some others, I can meet with among my promiscuous memorials; whose number, when I drew them together, I could scarce increase, having by all these, and other trials of differing kinds, wasted my cupels, and commodious glasses, where I could not well repair my loss. Whether I should have been able by reduction, specifick gravity, or any other of the ways, which I had in my thoughts, to make any discovery of the nature of the substance, that made the increment of weight in our ignited bodies; the want as well of leisure, as of accommodations requisite to go through with so difficult a task, keeps me from pretending to know. But these three things, I hope, I may have gained by what has been delivered: the first, that we shall henceforth see cause to proceed more warily in the experiments we make with metals in the fire, especially by cupellation. The next, that it will justify, and, perhaps, procure an easier assent to some passages in my other writings, that have relation to the substance, whatever it be, that we are speaking of.

of. And the third, (which is the principal,) that it will probably excite you, and your inquisitive friends, to exercise their sagacious curiosity, in discovering what kind of substance that is, which, though hitherto overseen by philosophers themselves, and, being a fluid far more subtil than visible liquors, and able to pierce into the compact and solid bodies of metals, can yet add something to them, that has no despicable weight upon the balance, and is able, for a considerable time, to continue fixed in the fire.

ADDITIONAL EXPERIMENTS,

About ARRESTING and WEIGHING of

IGNEOUS CORPUSCLES.

EXPERIMENTS to discover the increase in weight of bodies, though inclosed in glasses, being those, that I considered as likeliest to answer what I designed in the hitherto prosecuted attempt, and finding the seventeenth experiment, as well as the next (tried upon mercury) to be very slow, and its performance not to be very great, I began to call to mind, what, many years ago, experience had shewn me possible to be performed, as to the managing glass-vessels, even without coating them, in a naked fire, provided a wary person were constantly employed to watch them. And supposing hereupon, that in no longer time, than a laborant might, without being tired, hold out to attend a glass, a metal exposed in it to a naked fire might afford us a much more prosperous trial than that lately referred to, I afterwards resolved, when I should be able to procure some glasses conveniently shaped, to prosecute my design; in pursuance of which, though I had not any furnaces fitted for my purpose, I directed a laborant to make the following trials.

EXPERIMENT I.

[WE took eight ounces (*Troy* weight) of block-tin, which being cut into bits, was put into a good round vial with a long neck, and then warily held over quick coals, without touching them, till it was melted; after which, it was kept almost continually shaken, to promote the calcination, near an hour, the metal being all the while in fusion, and the glass kept at some distance from the thoroughly kindled coals. The most part of this time the orifice of the vial was covered with a cap of paper (which sometimes fell off by moving the glass) to keep the air, and steams of the coals, from getting into the neck. And at the end of this time, he, that held the glass, being tired, and having his hand almost scorched, the vial being removed from the fire, was broken, that we might take out the metalline lump, which had a little darkish calx here and there upon the upper surface, but much more beneath, where it had been contiguous to the bottom of the glass; then putting all this, carefully freed from little fragments of broken glass, into the same balance with the self-same counterpoise I had used before, I found, according to my expectation, an increase of weight, which amounted to eighteen grains, that the tin had acquired by this operation.]

EXPERIMENT II.

[THIS done, we separated the calx for fear of losing it, and having melted the metal in a crucible, that by pouring it out it might be reduced to thin plates, capable of being cut in pieces, and put into such another vial as the last; we weighed it again, together with the lately reserved calx, but found, that notwithstanding all our care, we had lost three grains of the eighteen we had gained. This done, we put the metal into another vial. But in regard the neck was shorter than that of the former, and could not, like it, be long held in ones hand; and because also I was willing to see what interest the shaking of melted tin has in the quickness of the calcination, the glass, which had a stopple of paper put to it to keep out smoke and air, was held at some distance from the coals, only whilst the tin was melting; and then was warily laid upon them, and kept there for two hours; at the end of which it was again taken off, and the metal weighed with the same counterpoise and balance, as formerly; and then it appeared to amount to eight ounces, twenty-four grains, and to have much more separable calx than at the first time. Nor did I much wonder, that the weight should be increased, in this last operation, but nine grains in two hours, and in the former, twice so many in half the time; since, during the two hours, the glass was kept in one posture, whereas, in the first operation, it was almost perpetually shaken all the while it was kept in fusion. And it it observed, that the agitation of melted minerals will much promote the effect of the fire upon them, and conduce to their calcination.]

EXPERIMENT III.

THOUGH these trials might well satisfy a person not very scrupulous, yet to convince even those, that are so, I undertook, in spite of the difficulties of the attempt, to make the experiment in glasses hermetically sealed, to prevent all suspicion of any accession of weight accruing to the metal, from any smoke or saline particles getting in at the mouth of the vessel. And in prosecution of this design, I thought upon a way of so hermetically sealing a retort, that it might be exposed to a naked fire, without being either cracked, or burst; an account of which trial was set down.

[EIGHT ounces of good tin, carefully weighed out, was hermetically sealed up in a new small retort, with a long neck, by which it was held in one's hand, and warily approached to a kindled charcoal fire, near which the metal was kept in fusion, being also ever now and then shaken for almost half an hour, in which time it seemed to have acquired on the surface such a dark colour, as argued a beginning of calcination, and it both emitted fumes, that played up and down, and also afforded two or three drops of liquor in the neck of the retort. The laborant being not able to hold the glass any longer, it was laid on quick coals, where the metal continued above a quarter of an hour longer in fusion; but before the time was come, that I intended to suffer it to cool, in order to the removing it, it suddenly broke in a great multitude of pieces, and with a noise like the report of a gun; but (thanks be to God) it did no harm neither to me, nor others, that were very near it. In the neck we found some drops of a yellowish liquor, which a virtuoso, that tasted it, affirmed to be of an odious, but peculiar sapor; and as for the smell, I found it to be very stinking, and not unlike that of the distilled oil of fish.]

BUT though our first attempt of this kind had thus miscarried, we were not thereby discouraged, but, in prosecution of the same design, made the ensuing trial.

EXPERIMENT IV.

[THE tin, which had been before (in the first, or some such experiment) partly calcined in a glass, being melted again in a crucible, that it might be reduced to pieces small enough to be put into another glass, was put again into the scales, and the surplusage being laid aside, that there might remain just eight ounces; these were put into a bolt-head of white glass, with a neck of about twenty inches long, which being hermetically sealed (after the glass had been a while kept over the fire, lest that should break by the rarefaction of the air) the metal was kept in fusion for an hour and a quarter, as (being hindered by a company of strangers from being there myself) the laborant affirmed. Being unwilling to venture the glass any longer, it was taken from the fire, and when it was grown cold, the sealed end was broken off: but before I would have the bottom cut out, I observed, that the upper surface of the metal was very darkly coloured, and not at all smooth, but much, and very oddly asperated; and the lower part had between the bottom, and the lower part of the lump, a pretty deal of loose, dark-coloured calx, though the neighbouring surface, and some places of the lump itself, looked by candle-light (it being then night) of a golden colour. The lump and calx together were weighed in the same scales carefully, and we found the weight to have increased twenty three grains, and better, though all the calx, we could easily separate, being weighed by itself, amounted not to four scruples, or eighty grains.

FOR confirmation of this experiment, I shall subjoin another, wherein but a quarter of so much metal was employed, with such success, as the annexed memorial declares.

EXPERIMENT V.

[TWO ounces of filings of tin were carefully weighed, and put into a little retort, whose neck was afterwards drawn slenderly out into a very small apex: then the glass was placed on kindled coals, which drove out fumes at the small orifice of the neck for a pretty while. Afterwards the glass, being sealed up at the apex, was kept in the fire above two hours; and then being taken off, was broken at the same apex; whereupon I heard the outward fire rush in, because, when the retort was sealed, the air within it was highly rarified. Then the body of the glass being broken, the tin was taken out, consisting of a lump, about which there appeared some grey calx, and some very small globules, which seemed to have been filings melted into that form. The whole weighed two ounces, twelve grains, the latter part of which weight appeared to have been gained by the operation of the fire on the metal. In the neck of the retort, where it was joined to the body, there appeared a yellowish and clammy substance thinly spread, which smelt almost like the fœtid oil of tartar.]

EXPERIMENT VI.

TO vary the foregoing experiments by making trials on a mineral, that is held to be of a very metalline nature, but is not a true metal, nor will be brought to fusion by so moderate a heat, as will suffice to melt tin, and yet has parts less fixed than tin, as being far more easily sublimable, we thought fit to make the following experiment.

[WE took an ounce of filings of zink, carefully weighed; and having as carefully put them into a round bolt-glass, we caused the neck to be drawn out very slender, and then ordered the laborant to keep it upon quick coals for the appointed time. Afterwards returning home, I called for the glass, which, he said, he had kept four hours upon the coals; answering me also, that there did, for a great

part

part of the time ſmoke appear to aſcend from the zink, and get out at the unſtopped apex. And in effect I obſerved, that the upper part of the glaſs was lined with flores or ſublimate of a darkiſh grey. The glaſs being dexterouſly cut aſunder, we took out, not only the filings of zink, ſome of which were melted into little globules, but the flores too; and yet weighing all theſe in the ſame ſcales we had uſed before, we found five grains and ſomewhat better wanting of an ounce. Which we the leſs wondered at, becauſe of the continuance of the lately mentioned exhalations emitted by the filed mineral.]

EXPERIMENT VII.

FOR more ample confirmation of the truth diſcovered by what I have been reciting about tin, I thought fit to try the like experiment upon another metal, which, though of ſomewhat more difficult fuſion than tin, I had reaſon to think might, if employed in a moderate quantity, and warily managed, be kept melted in glaſs without breaking it. And accordingly, having carefully weighed out four ounces of good lead, cut before-hand into pieces little enough for the orifice of the glaſs, I cauſed them to be put into a ſmall retort with a long neck, wherein was afterwards left but an orifice not much bigger than a pin's head: then leaving directions with the laborant what to do, becauſe I was my ſelf called abroad, at my return he brought me, together with the glaſs, this account; that he had kept it over and upon the coals two hours, or better, and then ſuppoſing the danger of breaking the glaſs was over, he had ſealed it up at the little orifice newly mentioned, and kept it on the coals two hours longer. Before the glaſs (which I found to be well ſealed) was broken, I perceived the pieces of lead to have been melted into a lump, whoſe ſurface was dark and rugged, and part of the metal to have been turned into a dark-coloured powder or calx: all this being taken out of the retort, was weighed in the ſame balance, whereon the lead appeared to have gained by the operation ſomewhat above thirteen grains.

EXPERIMENT VIII.

TO ſhew, that metals are not the only bodies, that are capable of receiving an increaſe of weight from the fire, I thought fit to make upon coral a trial, whereof my memorial gives me this account.

[LITTLE bits of good red coral, being hermetically ſealed up in a thin bubble of glaſs, after two drachms of them had been weighed out in a pair of nice ſcales, were warily kept at ſeveral times over and upon kindled coals, and at length being taken out for good and all, were found of a very dark colour, and to have gained in weight three grains and about a half.]

EXPERIMENT IX.

ONE experiment there is, which, though it might have come in more properly at another place, is not to be omitted in this, becauſe it may invite us to conſider, whether, in the foregoing experiments, excepting thoſe made on lead and tin in ſealed veſſels, there may not be more of the fire adherent to, or incorporated with the body expoſed to it, than one would conclude barely from the recited increments of their weight. For having taken very ſtrong freſh quick-lime, provided on purpoſe for choice experiments, and expoſed it, before the air had time to ſlake it, upon the cupel, to a ſtrong fire, where it was kept for two hours; I found, that it had increaſed in weight even ſomewhat beyond my expectation. For being ſeaſonably put into the balance, the lumps, that weighed, when expoſed, but two drachms, amounted to two drachms and twenty-nine grains; which makes this experiment a pregnant one to our purpoſe. For by this it appears, that notwithſtanding a body may for many hours, or even for ſome days, be expoſed to a very violent fire, yet it may be ſtill capable of admitting and retaining freſh corpuſcles; ſo that, though well made lime be uſually obſerved to be much lighter than the ſtones whereof it is made, yet this lightneſs does not neceſſarily prove, that, becauſe a burnt lime-ſtone has loſt much of its matter by the fire, it has therefore acquired no matter from the fire; but only infers, that it has loſt far more than it has got. And this may give ground to ſuſpect, that in moſt of the foregoing trials, the acceſſion of the fiery particles was greater (though in ſome more, in others leſs ſo,) than the balance diſcovered; ſince, for aught we know, divers of the leſs fixed particles of the expoſed body might be driven away by the vehemence of the heat; and conſequently the igneous corpuſcles, that faſtened themſelves to the remaining matter, might be numerous enough, not only to bring the acceſſion of weight, that was found by the ſcales, but to make amends for all the fugitive particles, that had been expelled by the violence of the fire. And ſince ſo fixed a body, as quick-lime, is capable of being wrought upon by the igneous effluvia, ſo as that they come to be, as it were, incorporated with it, it may, perchance, be worth conſidering, whether in other calcined, or incinerated bodies, the remaining calces, or aſhes, may not retain more than the bare impreſſion (unleſs that be ſtretched to mean ſome participation of a ſubſtance,) of the fire. Whether theſe particles, that adhere to, or are mingled with the ſtony ones of the lime, may have any thing to do in the heat and tumult, that it produced upon the ſlaking of lime, this is not a fit place to examine. And though by this experiment and thoſe made in ſealed retorts, which ſhew, that what is afforded by fire may in a corporeal way invade, adhere, and add weight to even fixed and ponderous bodies, there is a large field opened for the ſpeculative to apply this diſcovery to divers phænomena of nature and chemiſtry; yet I ſhall leave this ſubject unmeddled with in this place.

A DISCOVERY OF THE PERVIOUSNESS OF GLASS TO PONDERABLE PARTS of FLAME.

With ſome Reflections on it by way of COROLLARY.

THAT I might obviate ſome needleſs ſcruples, that may be entertained by ſuſpicious wits upon this circum ſtance of our additional experiments, "That the glaſſes employed about them were "not expoſed to the action of mere flame, "but were held upon charcoals," (which to ſome may ſeem to contain but a groſſer kind of fire:) and that alſo I might, by diverſifying the way of trial, render ſuch experiments both more fit to afford corollaries, and more ſerviceable to my other purpoſes, I attempted to make it ſucceed with a body ſo thin and diſengaged from groſs matter, as mere flame is allowed to be, knowing, that by going cautiouſly with it to work, one might handle a retort without breaking it, in ſpite of a violent agitation of kindled matter.

EXPERIMENT I.

SUPPOSING then, that good common ſulphur, by reaſon of its great inflammability, and the vehemency and penetrancy of its flame, would be a very fit fuel for my purpoſe, I provided a ſmall double veſſel ſo contrived, that the one ſhould contain as many coals, as was neceſſary to keep the ſulphur melted, and that the other, which was much ſmaller, and ſhaped like a pan, ſhould contain the brimſtone requiſite for our trial; and, laſtly, that theſe two ſhould be with a convenient lute ſo joined to one another, that all being cloſed at the top, ſave the orifice of the little pan, (the fire and ſmoke of the coals having their vent another way,) no fire ſhould come at the retort to be employed, but the flame of the burning brimſtone. Then two ounces of filings of tin being heedfully weighed out, and put into a glaſs-retort provided for ſuch trials, and made fit to be eaſily ſealed up at the neck, when the time ſhould be convenient, the ſulphur (which ought to be of the purer ſort) was kindled, and the glaſs by degrees expoſed to it; where it continued, as the laborant informed me, (the ſmell of brimſtone, peculiarly offenſive to me, forbidding me to be preſent,) near two hours before the metal melted; after which, he kept the retort near an hour and half more, with the metal melted in it. Then bringing it me to look upon, I perceived a pretty deal of darkiſh calx at the bottom, and partly too upon the ſurface of the far greater part of the metal, which now lay in one lump. The part of the retort, that had been ſealed, being broken off, we firſt took out the calx, and then the lump, and putting them into the ſcales, they had been formerly weighed in, found them to have made a very manifeſt acquiſt of weight, which, if both the laborant and I be not miſtaken, (for the paper, which ſhould inform us, is now miſſing) amounted to four grains and a half, gained by the recited operation. Afterwards, we being grown more expert in making ſuch trials, the experiment was repeated with the ſame quantity of filings of the ſame metal: at the end of the operation, (which in all laſted ſomewhat above three hours) having broken off the ſealed neck of the retort, we found, that a good proportion of dark coloured calx had been produced. This being weighed with the uncalcined part of the metal, the two ounces, we firſt put in, appeared to have acquired no leſs than eleven grains and a half (and ſomewhat better.)

SUCH ſuperſtructures, both for number and weight, may poſſibly in time be built on this

this and the like experiments, that I shall venture to obviate, even such a scruple, as is like to be judged too sceptical. But I remember, that, considering upon occasion of some of the experiments formerly recited, that though it were very improbable, yet it did not appear impossible, that the increment of weight, acquired by bodies exposed in glass-vessels to the fire, might proceed, not from the corpuscles of fire, but from the particles of the glass it self, loosened by the power of so intense a heat, and forcibly driven into the inclosed body; I was content to take a couple of glasses, whereof one was shaped into a little retort, and having weighed them, and then having kept them for a considerable time upon kindled coals, and then weighed them again, I could gather little of certainty from the experiment, (the retort at one time seeming to have acquired above half a grain in the fire,) save that there was no likelihood at all, that so considerable an encrease of weight, as we divers times obtained in close vessels, should proceed from the glass it self, and not from the fire.

EXPERIMENT II.

BECAUSE it seems evident enough, that, whatever chemists tell us of the hypostatical sulphur, common brimstone is a body heterogeneous enough, having in it some parts of an oily or inflammable nature, and others acid, and very near of kin to the spirits of vitriol; I thought fit to vary our experiment, by making it with a liquor, that is generally reputed to be as homogeneous as chemists themselves are wont to render any, I mean with a spirit of wine, or some such liquor as will totally flame away without affording soot, or leaving any drop of phlegm behind it. In prosecution of this design, we carefully weighed out an ounce of filings of block-tin, and put them into a glass retort, fit for the purpose, whose neck was afterwards drawn out to a great slenderness; and we also provided a conveniently shaped metalline lamp, such as that the flame of this ardent spirit might commodiously burn in it, and yet not melt or crack it; which lamp, though furnished with a cotton wick, afforded no soot, because, as long as it was supplied with liquor enough, it remained unburnt. These things being in readiness, the retort was warily approached to the flame, and the metal was thereby in a short time melted. After which, the glass being kept exposed to the same flame for near two hours in all, the sealed apex of the retort was broken off, and there appeared to have been produced a not inconsiderable quantity of calx, that lay loose about the remaining part of the tin, which, upon its growing cold, was hardened into a lump. This, and the calx, being taken out of the retort with care, that no little fragment of glass should at all impose upon us, was weighed in the same scales as formerly, and found to have gained four grains and a half, besides the dust, that stuck in the inside of the retort, of which we reckoned enough to make about half a grain more; so that of so fine and pure a flame, as of this totally ardent spirit, enough to amount to five grains was arrested, and in good measure fixed by its operation on the tin it had wrought upon.

EXPERIMENT III.

FOR confirmation of the former trial, wherein we had employed the *spiritus ardens* of sugar, we made the like experiment with highly rectified spirit of wine, only substituting an ounce of lead instead of one of tin. The event, in short, was this; that after the metal had been for two hours or better, kept in the flame, the sealed neck of the retort being broken off, the external air rushed in with a noise, (which shewed the vessel to have been very tight,) and we found pretty store of the lead; for it was above seven scruples, turned into a greyish calx, which together with the rest of the metal being weighed again, there was very near, if not full six grains of increase of weight acquired by the operation.

1. N. B. THE lump of lead, that remained after the newly recited operation, being separated from the calx, was weighed and cut in pieces, that it might be put into a fresh retort, wherein it was again exposed to the flame of spirit of wine, that I might satisfy my self, whether probably the whole body of the lead might not, by repeated operations, or (perhaps by one continued long enough) be reduced to calx. And though, after the retort (whose neck had been drawn out) had been kept in the flame for about two hours, it was, by the negligence of a foot-boy, unluckily broken, and some of the calx lost; yet we made a shift to save about five grains of it, (whose colour was yellowish;) which was enough to make it likely, that, if we had had conveniency to pursue the operation to the utmost, the whole metal might have been calcined by the action of the flaming spirit.

2. N.B. AND lest you should be induced by some chemical conceits to imagine, that the particles, that once belonged to flame, did make more than a coalition with those of the lead, and by a perfect union were really transmuted into the metal whose weight they encreased; I shall add, that (according to a method elsewhere delivered) I examined the seven scruples of calx, mentioned to have been made in the third experiment, by weighing them in air and water, and thereby found, as I expected, that though the absolute gravity of the metal had been encreased by the particles of flame, that stuck fast to it, yet this aggregate of lead and extinguished flame had lost much of its specifick gravity. For whereas lead is wont to be, to water of the same bulk, as about eleven and an half to one; the subtil calx of lead was to water of the same bulk, little, if at all, more than as nine to one.

THESE are not the only experiments I made of the operation of mere flame upon bodies inclosed in glasses; but these, I suppose, are sufficient to allow me to comply with my present haste, and yet make good the title prefixed to this paper. For whence can this encrease of absolute weight (for I speak not of specifick

specifick gravity,) observed by us in the metals exposed to the mere flame, be deduced, but from some ponderable parts of that flame? And how could those parts invade those of the metal inclosed in a glass, otherwise than by passing through the pores of that glass? But because I judge it unphilosophical, either to be more careful, that what one writes should appear strange, than be true; or to be forward to advance the repute of strangeness, to the prejudice of the interest of truth, though it be perhaps but a remote one, or a collateral one; I shall deal so impartially, as to subjoin on this occasion two or three short intimations, that may prove both seasonable for caution, in reference to the porousness of glass, and give a hint or two in relation to other things.

I do not then, by the foregoing experiments, pretend to make out the porosity of glass any farther, than is expressed in the title of this paper; namely, in reference to some of the ponderable parts of flame. For otherwise I am not at all of their mind, that think glass is easily penetrable, either, as many do, by chemical liquors; or, as some, by quick-silver; or, as others, at least by our air; those opinions not agreeing with the experiments I made purposely to examine them, as you may find in another paper.

AGAIN, if we compare the increase we observe to be made in the weight of the bodies, that we expose to the naked fire, and those of the same or the like kinds, that we included in glasses, or so much as in crucibles; it may be worth considering, whether this difference in acquired weight may not give cause to suspect, that the corpuscles, whereof fire and flame consists, are not all of the same size, and equally agitated, but that the interposed vessel keeps out the grosser particles like a kind of strainer, though it gives passage to the minutest and most active.

I offer it also to consideration, whether this perviousness of glass, even to the minute particles, that pervade it, and their adhesion to the metal they work on, does necessarily imply pores constantly great enough to transmit such corpuscles: or, whether it may not be said, that glass is generally of a closer texture, than when in our experiments the pores are opened by the vehement heat of the flame, that beats upon it, and in that state, may let pass corpuscles too big to permeate glass in its ordinary state; and, that this penetration is much assisted by the vehement agitation of the igneous parts, which, by the rapidness of their motion, both force themselves a passage through the narrow pores of the glass, and pierce deep enough into those of the included body, to stick fast there; (as hail-shot thrown with one's hand against a board will pass off from it, but being shot out of a gun will pierce it, and lodge themselves in it:) and I know a menstruum, that does not work upon a certain metal, whilst the liquor is cold, or but faintly heated; and yet by intending the heat would be made to turn it into a powder or calx, (for it does not not properly dissolve it.)

PERHAPS it may not be amiss to add on this occasion, that though glass be generally acknowledged to have far smaller pores, than any other matter wont to be implied to make vessels, that are to be exposed to the fire; yet, till I be farther satisfied, I shall forbear both to determine, whether the rectitude, that some philosophers suppose in the pores of glass, as it is a transparent body, or rather in their ranks or rows, may facilitate the previousness we above observed in glass, and to conclude from the foregoing experiments, that ponderable parts of flame will be able, as well to pass through the pores of metalline vessels, as those of glass. For though, with a silver vessel, made merely of plate, without soder, I made two or three trials, (of which you may command an account) in order to the resolving of these doubts; yet by an accident, which, though it were not a surprizing one, was unlucky enough to defeat my endeavours, I was kept, for want of fit accommodations, from bringing my intended trials to an issue.

AND now having endeavoured by the foregoing advertisements, to prevent the having unsafe consequences drawn from our experiments; it remains, that I briefly point at three or four corollaries, that may more warily be deduced from them. To which, if I get time, I may subjoin a hint or two about further enquiries.

COROLLARY I.

Confirming this paradox, that flame may act as a menstruum, and make coalitions with the bodies it works on.

THE experiments we have made and recited, of the permeating of flame (as to some of its parts) through glass-vessels, and of its working on included metals, may much confirm the paradox I have elsewhere proposed, that flame may be a menstruum, and work on some bodies at the rate of being so; I mean, not only by making a notable comminution and dissipation of the parts, but by a coalition of its own particles, with those of the fretted body, and thereby permanently adding substance and weight to them. Nor is it repugnant to flame's being a menstruum, that in our experiment, the lead and tin, exposed to it, were but reduced to powder, and not dissolved in the form of a liquor, and kept in that state. For, besides that the interposed glass hindered the igneous particles from getting through in plenty enough; I consider, that it is not necessary, that all menstruums should be such solvents, as the objection supposes. For whether it be (as I have sometimes suspected,) that menstruums, that we think simple, may be compounded of very differing parts, whereof one may precipitate what is dissolved by the other; or for some other cause, I have not now time to discuss. Certain it is, that some menstruums corrode metals and other bodies, without keeping dissolved all, or perhaps, any considerable part; as may be seen, if you put tin in a certain quantity of aqua fortis, which will in a very short time reduce it almost totally to a very white substance, which, when dry, is a kind of calx. And so by a due proportion of oil of vitriol, abstracted from quicksilver by a strong fire, we have divers times reduced the main body of the mercury into a white powder, whereof but an inconsiderable part would be dissoluble in water. And such a white calx I have had by the action of another fretting liquor on a body not metalline.

And having thus cleared our paradox of the opposed difficulty, my hast would immediately carry me on to the next corollary, were it not, that there is one phænomenon, belonging to this place, that deserves to be taken notice of. For, whether it be, as seems probable, from the vehement agitation of the permeating particles of flame, that violently tear asunder the metalline corpuscles, or from the nature of the igneous menstruum; (which being, as it were, percolated through glass itself, must be strangely minute,) it is worth observing, how small a proportion, in point of weight, of the additional adhering body, may serve to corrode a metal, in comparison of the quantity of vulgar menstruums, that is requisite for that purpose. For, whereas we are obliged to employ, to the making the solution of crude lead, several times its weight of spirit of vinegar, and (though not so many times) even of aqua fortis, it was observed in our experiment, that, though the lead was encreased but six grains in weight, yet above six score of it were fretted into powder, so that the corrosive body appeared to be about the twentieth part of the corroded.

COROLLARY II.

Proposing a paradox about calcination, and calces.

ANOTHER consequence, deducible from our discovery of the perviousness of glass to flame, may be this; that there is cause to question the truth of what is generally taken for granted about calcination, and particularly of the notion, that not only others, but chemists themselves, have entertained about the calces of metals, and minerals. For, whereas it is commonly supposed, that in calcination the greater part of the body is driven away, and only the earth, to which chemists add the fixed salt, remains behind; and whereas even mechanical philosophers, (for two or three of them have taken notice of calcination,) are of opinion, that much is driven away by the violence of the fire, and the remaining parts, by being deprived of their more radical and fixed moisture, are turned into dry and brittle particles: Whereas these notions, I say, are entertained about calcination, it seems, that they are not well framed, and do not universally hold; since, at least, they are not applicable to the metals, our experiments were made on. For, it does not appear by our trials, that any proportion, worth regarding, of moist and fugitive parts, was expelled in the calcination; but it does appear very plainly, that by this operation the metals gained more weight than they lost; so that the main body of the metal remained entire, and was far from being, either as a peripatetick would think, elementary earth, or a compound of earth and fixed salt, as chemists commonly suppose the calx of lead to be. From which very erroneous hypothesis they are wont to infer the sweet vitriol of lead, which they call *saccharum Saturni*, to be but the sweet salt of it extracted only by the spirit of vinegar, which does indeed plentifully enough concur to compose it. Whence I conclude, that the calx of a metal even made as they speak, *per se*, that is, by fire without additament, may be, at least in some cases, not the *caput mortuum*, or *terra damnata*, but a magistery of it. For, in the sense of the most intelligible of the chemical writers, that is properly a magistery, wherein the principles are not separated, but the bulk of the body being preserved, it acquires a new and convenient form by the addition of the menstruum, or solvent, employed about the preparation. And, not here to borrow any argument from my notes about particular qualities, you may guess, how true it is, that the greatest part of the body, or all the radical moisture, is expelled in calcination, which therefore turns the metal into an arid, unfusible powder; by this, that I have several times, from calx of lead, reduced corporal lead. And I remember, that having taken, what I guessed to be but about a third, or fourth part, of the calx of lead, produced by the third experiment, I found by a trial purposely devised, that without any flux-powder, or any additament, but meerly by the application of the flame of highly rectified spirit of wine, there could, in a short time be obtained a considerable proportion of malleable lead; whereof the part I had the curiosity to examine, was true malleable lead; so little was the arid powder, whence this was reduced, deprived by the foregoing calcination of the supposed radical moisture requisite to a metal. The consideration of what may be drawn from this reduction, in reference to the doctrine of qualities, belongs not to this place.

COROLLARY III.

ONE use, among the rest, we may make, by way of corollary, of the foregoing discovery, which is in reference to a controversy warmly agitated among the corpuscular philosophers themselves. For some of them, that follow the Epicurean or atomical hypothesis, think, that when bodies are exposed in close vessels to the fire, though the igneous corpuscles do not stay with the bodies they invade, yet they really get through the pores of the interposed vessels, and permeate the included bodies in their passage upwards; whereas others, especially favourers of the cartesian doctrine, will not allow the atomists igneous corpuscles, which they take to be but vehemently agitated particles of terrestrial matter, to penetrate such minute pores as those of glass; but do suppose the operation of the fire to be performed by the vehement agitation made of the small parts of the glass, and by by them propagated to the included bodies, whose particles, by this violent commotion, are notably altered, and receive new textures, or other modifications.

But our experiments inform us, that, though neither of the two opinions seems fit to be despised, yet neither seems to have hit the very mark; though the Epicurean hypothesis comprise somewhat more of the truth, than the other. For, though it be not improbable, that the brisk agitation, communicated by the small parts of the glass to those of the body contained in it, may contribute much to the effect of the fire; and though, by the small increment of weight, we found in our exposed metal, it is very likely, that far the greater part of the flame was excluded by the close texture of the glass; yet, on the other side, it is plain, that igneous particles were trajected through the glass, which agrees with the Epicureans; and they, on the other side, mistook, in thinking, that they did but pass through, and divide, and agitate the included bodies; to which, nevertheless, our experiments shew, that enough of them, to be manifestly ponderable, did permanently adhere.

Whether these igneous corpuscles do stick, after the like manner, to the parts of meat, dressed by the help of the fire, and especially roast-meat, which is more immediately exposed to the action of the fire, may be a question, which I shall now leave undiscussed, because I think it difficult to be determined, though, otherwise, it seems worthy to be considered, in regard it may concern men's health to know, whether the coction of meat be made by the fire, only as it is a very hot body, or whether it permanently communicates any thing of its substance to the meat exposed to it: in which last case, it may be suspected, that not only the degree, and manner of application of a fire, but the nature of its fuel, may be fit to be considered.

COROLLARY IV.

The experiments above recited give us this further information, that bodies very spirituous, fugitive, and minute, may, by being associated with congruous particles, though of quite another nature, so change their former qualities, as to be arrested, by a solid and ponderous body, to that degree, as not to be driven away from it by a fire intense enough to melt and calcine metals.

For the foregoing trials (taking in what I lately delivered of the lessened specifick gravity of calcined lead) seems plainly enough to discover, that even the agitated parts of flame, minute enough to pass through the pores of glass itself, were, as it were, entangled among the metalline particles of tin and lead, and thereby brought to be fixed enough to endure the heat, that kept those metals in fusion, and little by little reduced them into calces: which is a phænomenon, that one would not easily look for, especially considering how simple a texture, that of lead or tin may be supposed to be, in comparison of the more elaborate structures of very many other bodies. And this phænomenon, which shews us, what light and fugitive particles of matter may permanently concur to the composition of bodies ponderous and fixed enough, may perchance afford useful hints to the speculative; especially if this strict combination of spirituous and fugitive substance with such, as being gross or unwieldy, are less fit, than organized matter, to entangle or detain them, be applied, (as it may be with advantage) to those aggregates of spirituous corpuscles, and organical parts, that make up the bodies of plants and animals. And this hint may suggest a main inference to be drawn from the operations of the sunbeams on appropriated subjects, supposing it to prove like that of flame on tin and lead.

Exp. III. N. B. 2.

And now having dispatched our corollaries, we might here enquire, whether all the particles of fire and flame, that are subtil, and agitated enough to penetrate glass, and fasten themselves to included bodies, be reduced by ignition to the same nature, or else retain somewhat of their proper qualities? which enquiry I have some cause not to think so undeterminable, as at first blush it may appear. For one of the ways, that may be proposed for this examen, is already intimated at the close of the third experiment; which shews, that we may compare the specifick gravity of the calces of the same metal, made in glasses by the operation of flames, whose fuels are of very differing natures. And I said, one of the ways, because it is not the only way I could name, and have partly tried. But though I might say more concerning expedients of this kind, and could perhaps propound other enquiries, that may reasonably enough be grounded upon the hitherto recited phænomena, (and those of some other like trials) yet I must not unseasonably forget, that the pursuit of such disquisitions would lead me much farther, than I have now the leisure to follow it.

A

LETTER

CONCERNING

AMBERGREASE,

And its being

A VEGETABLE PRODUCTION.

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SIR,

SOME occaſions calling me this afternoon up to *London*, I met there with a very intelligent gentleman, who was ready to go out of it; but before he did ſo, he willingly ſpared me ſome time to diſcourſe with him about ſome of the affairs of our Eaſt-Indian Company, of which he was very lately deputy-governour; and, his year being expired, is ſtill one of the chief of the court of committees, which a foreigner would call directors, that manage all the affairs of that conſiderable ſociety. And, among other things, talking with him about ſome contents of a journal lately taken in a Dutch Eaſt-Indian prize, I learned from him, that he, who underſtands that language very well, is now peruſing that manuſcript, and, among many other things recorded there, that concern the oeconomical, and political affairs of the ſaid Dutch Company, he met with one phyſical obſervation, which he thought ſo rare, that remembring the curioſity I had expreſſed for ſuch things, he put it into Engliſh, and tranſcribed it for me, and immediately drawing it out of his pocket, he preſented me the ſhort paper, whereof I now ſhew you the copy: upon peruſal of which, you will very eaſily believe, that not only his civility obliged me, but the information it brought me, ſurprized me too. For the ſeveral trials, and obſervations of my own, about ambergreaſe, have long kept me from acquieſcing either in the vulgar opinions, or thoſe of ſome learned men concerning it; yet I confeſs, my experiments did much leſs diſcover what it is, than this paper has done, in caſe we may ſafely and entirely give credit to its information, and that it reach to all kinds of ambergreaſe. And, probably, you will be invited to look on this account, though not as complete, yet, as very ſincere, and, on that ſcore, credible, if you conſider, that this was not written by a philoſopher, to broach a paradox, or ſerve an hypotheſis, but by a merchant, or factor, for his ſuperiors, to give them an account of a matter of fact; and that this paſſage is extant in an authentick journal, wherein the affairs of the company were, by publick order, from time to time regiſtered, at their chief colony, *Batavia*. And it appears by the paper itſelf, that the relation was not looked upon as a doubtful thing, but as a thing, from which a practical way may be deduced to make this diſcovery eaſily lucriferous to the Dutch Company. And I could heartily wiſh, that in thoſe countries, that are addicted to long navigations, more notice, than is uſual, were taken and given of the natural rarities, that occur to merchants and ſeamen. On which occaſion, I remember, when I had, in compliance with my curioſity, put myſelf into our Eaſt-Indian Company, and had, by their civility to me, been choſen of their committee, as long as my health allowed me to continue ſo, I had the opportunity, in ſome regiſter-books of merchants, Engliſh and Dutch, to obſerve ſome things, which would eaſily juſtify this wiſh of mine, if my haſte, and their intereſt,

interest, would permit me to acquaint others with them. But to return to our account of ambergrease, I think you will easily believe, that, if I had received it not by a paper, but immediately from the writer, I should, by proposing divers questions, have been enabled to give you a much more satisfactory account, than this short one contains. But the obliging person, that gave it me, being just going out of town, I could not civilly stay him to receive my queries about it; which though (God permitting) I may propose, ere long, if I can light on him again, yet I fear he has given me, in these few lines, all that he found about this matter. However, this relation, as short as it is, being about the nature of a drug so precious, and so little known, will not, I hope, be unwelcome to the curious; to whom none is so like to convey it so soon, and so well, as Mr. *O.*; whose forwardness to oblige others by his various communications, challenges returns of the like nature from others, and particularly from his affectionate humble servant.

Follows the Extract itself out of a Dutch journal, belonging to the Dutch East-Indian Company.

" AMBERGREASE is not the scum, " or excrement of the whale, &c. but " issues out of the root of a tree, which tree, " how far soever it stands on the land, always " shoots forth its roots towards the sea, seek-" ing the warmth of it, thereby to deliver the " fattest gum, that comes out of it: which tree, " otherwise, by its copious fatness, might be " burned and destroyed. Wherever that fat " gum is shot into the sea, it is so tough, that " it is not easily broken from the root, unless " its own weight, and the working of the " warm sea doth it, and so it floats on the sea.

" THERE was found, by a soldier, $\frac{7}{8}$ of a " pound, and by the chief, two pieces weigh-" ing five pounds. If you plant the trees, " where the stream sets to the shore, then the " stream will cast it up to great advantage. " *March* 1, 1672, in *Batavia*: journal advice " from———"

TRACTS,

CONSISTING OF

OBSERVATIONS about the SALTNESS of the SEA.

An Account of a STATICAL HYGROSCOPE, and its USES.

TOGETHER WITH

An APPENDIX about the FORCE of the AIR'S MOISTURE.

A FRAGMENT about the NATURAL and PRETER-NATURAL STATE of BODIES.

To all which is premiſed,

A SCEPTICAL DIALOGUE about the POSITIVE or PRIVATIVE NATURE of COLD.

WITH

Some EXPERIMENTS of Mr. *BOYLE*'s referred to in that Diſcourſe.

OF THE

POSITIVE OR PRIVATIVE NATURE OF COLD.

A SCEPTICAL DIALOGUE between CARNEADES, THEMISTIUS, ELEUTHERIUS, PHILOPONUS.

SECTION I.

Eleuth. MAY one be allowed to aſk *Carneades*, what book it is he is reading with ſo much attention?

Carn. The queſtion, *Eleutherius*, is very allowable, and as eaſily anſwered, by ſaying, that what I was reading, is our friend Mr. *Boyle*'s newly publiſhed Hiſtory of Cold.

Them. YOUR readineſs, *Carneades*, to anſwer, encourages me alſo to aſk you a queſtion; which ſhall not be, as probably you expect it ſhould, how you like this new piece? for I know you would be too kind to the author, not to tell me; that he has detected ſome old errors, and made diſcovery of ſome new truths: but my queſtion ſhall be about what is my wonder, as well as that of divers others, who think it ſtrange, that a writer, that has de-

delivered so many effects and other phænomena of cold, should omit to tell us so much, as whether he asserts it to be a positive quality, or a bare privation of heat; as, since *Cardan* (in his treatise *De Subtilitate*) some other learned men, and especially *Cartesius*, have maintained.

Carn. You will not wonder, if a person, that you look upon, and I confess not injuriously, as a friend to Mr. *Boyle*, tell you, that this author, by the many histories he has presented us, and by his not seeming to dare to determine the controversy you have mentioned, shews, that he was more solicitous to lessen his ignorance, than to pretend to knowledge: and upon the observation I have made of his humour in general, I presume one principal reason of his silence may be, that he has not yet compleated the trials he had designed about cold; and thinks, that in abstruse subjects, such as this is, it is not so convenient to deliver a positive opinion of the nature of it at the beginning, as to reserve it for the latter end, after the history of the phænomena; when the nature of the thing enquired into may, as it were, spontaneously result from the considerations suggested by the precedent matters of fact surveyed together.

Eleuth. If such a wariness were indeed the motive of your friend's silence, I shall easily excuse it; and perhaps think too, that the like would not mis-become naturalists on many other occasions. And yet I do not dislike *Themistius*'s question; for it is one thing to venture upon declaring the adæquate nature of cold, and another to determine, whether it be a positive, or a privative quality? the latter attempt importing a much less venture than the former.

Carn. I will not pretend to know the very reasons, that induced the author silently to pass by this controversy; but having been once present, when he had occasion to discourse of it, I then conjectured, that among his experiments of cold, that are not yet published, there may be some uncommon ones, that may have suggested to him scruples, which obliged him to forbear declaring himself, till he had cleared them, which those, that are unacquainted with such trials, may probably have never thought of.

Them. If what you call a controversy, were indeed worthy of that name, I should not unwillingly allow of your friend's silence; but the opinion broached by *Cardan*, and adopted by Mr. *Des Cartes* and others, seems to me so devoid, not only of reason, but of all appearance of it, that methinks one, that has delivered such considerable effects of cold, as Mr. *Boyle* has done, may well ascribe to their cause, at least, a positive nature; and, without at all being guilty of boldness, reject an opinion, that is not only barely an error, but an extravagance, and perhaps a plain absurdity.

Carn. Possibly the gentleman we are speaking of, may be wary and sceptical enough to reckon among difficult things, not only the declaring the adæquate nature of cold, and the manner of its operations; but the demonstrating, whether it be a positive quality or not. And though I will not take upon me to know his thoughts about that subject, which, perhaps, are grounded upon some of his peculiar experiments and notions; yet, for discourse sake, I am content to debate with *Themistius*, Whether or no the opinion, he so severely censures, be not only erroneous, as, for aught appears, Mr. *Boyle* himself may be found to have thought it; but also, as *Themistius* would have it, absurd.

Them. I readily accept of your offer; for it cannot be an unpleasant entertainment to observe the arts, whereby one, that I know will not speak impertinently, will endeavour to make reason elude the clearest testimonies of sense. And though I might press you with the concurrent authority of *Aristotle*, and all the philosophers, that have lived between his time and those of that extravagant fellow *Cardan*; yet I shall rather employ, to convince you, the authority and reasons of a grand leader among your new philosophers, who being a great broacher of paradoxes, and having upon that score written books expresly against *Aristotle*, was not like to have sided with him, unless the evidence of truth had, as it were, necessitated him to do so.

Carn. I presume, you mean the learned and subtle *Gassendus*, whom I am glad you have pitched upon for your cause's champion, not only because, in defending the common opinion, he waves the common practice of troubling his readers with a multitude of authorities, which to me, in such a case as this, would signify very little, and betakes himself to arguments; but because, being so modern and judicious a writer, we may well suppose him to have summed up and improved what can be said in behalf of the cause he maintains. Upon which account, I shall be excused from answering impertinent objections against the opinion I defend, and from the trouble of ranging about, among other authors, for more weighty arguments than those, which the disproving of his will shew to be unsatisfactory.

Them. I am glad you named the author I meant, *Carneades*, for I apprehended you had not met with what he says upon this subject; because I could scarce imagine, that an intelligent person, after having read his arguments, will doubt of a truth he hath so clearly evinced by them. But since I perceive you have seen what he has written, I shall, without farther preamble, propose his reasons to you, though not in the very same order, wherein he has couched them.

Eleuth. But before you begin them, give me leave to ask *Carneades* a short question, whose answer will, I suppose, conduce, if not be necessary, to the clearing of the state of the controversy betwixt you. For it is one thing to deny belief to the received opinion, that cold is a positive quality, and another thing to assert, that it is but a privation of heat; since, if *Carneades* does undertake the latter of these two, he must bring positive arguments to prove cold to be but a negative thing. Whereas, if he content himself to play a doubting part, it may suffice him, being in effect but a defendant,

dant, to shew, that the proofs brought to conclude cold to be a positive quality are not cogent.

Carn. I acknowledge your question, *Eleutherius*, to be pertinent, and not unseasonable. And, I presume, you will not be surprized, that a person accused of scepticism answers it by declaring, that he undertakes not to demonstrate, that cold must be a privative or negative quality, and thinks it sufficient for his turn, to shew, that the arguments brought to evince it to be a positive one, are not concluding. And, since you have already diverted *Themistius* from beginning so soon as he intended, it will not be amiss, that I continue that suspension a little longer, to prevent, what I know we both hate, verbal controversies; which yet may very easily spring from undetermined acceptions of words, as ambiguous as I have observed heat (of which I now make cold but a privation) to be.

We may therefore consider, that the word heat being made use of to signify, as well the operations of that quality upon other bodies (as when the heat of the fire makes water boil, or that of the sun melts wax, and hardens clay) as its operations upon the sense of man, (as when a moderate degree of heat is said to cause pleasure, and an excessive one to produce pain;) this term, I say, as Mr. *Boyle* also has somewhere noted, may be employed sometimes in a more absolute and indefinite sense, and sometimes in a more confined and respective sense: in the latter of which, it is estimated by its relation to the organs of feeling of those men, that judge of it. Upon which account, men are wont to esteem no body hot, but such an one, the agitation of whose small parts is brisk enough to encrease, or surpass, that of the particles of the organ, that touches it: for, if that motion be more languid in the object, than in the sentient, the body is reputed cold; as may appear by this, that if the same person put one of his hands, when it is hot, and the other when it is cold, into luke-warm water, that liquor will feel cold to the warm hand, and warm to the cold.

Eleuth. So, that according to this doctrine, methinks, one may, for brevity sake, conveniently enough apply to your two-fold notion of heat, those expressions, which some schoolman employ about certain qualities, of any of which they say, that it may be either materially or formally considered. And by analogy to their doctrine, since heat is a tactile quality, and, as such, imports primarily a relation to the organ of touching, that relation, with what depends upon it, may pass for that, which is the *formale*, in the quality, called heat; and its effects and operations upon other bodies, may supply us with a notion of heat, materially taken.

Carn. I do not always quarrel, *Elutherius*, with terms borrowed from the schools, if they be as much more short and expressive than others, as they are more unusual, or even barbarous. But there is another distinction of heat, partly grounded upon that already proposed, which, because it may be of use in our future discourse, will not be unfit to be here intimated. For we may consider, that though, for the most part, a hot body is taken in the vulgar sense, for that, wherein the degree of heat is sensible to our organs of feeling; yet, in a looser sense, and which, for distinction sake, we may call philosophical, because concluded by reason, though not perceived by sense, a body may be conceived not to be destitute of heat, even when the degree of that quality is not great enough to be felt by the touch; provided it can produce, in some degree, those other operations, which, when more intense, are acknowledged to proceed from manifest heat. For elucidation of which, we may alledge, that in very frosty, and yet clear weather, the sun may be judged to warm the air, when it melts snow, and thaws ice; though, perhaps, many men, especially of tender constitutions, feel in their fingers and toes much stiffness, and more pain, upon the account of cold. To this I may add the common observation, if you grant the truth of it, that snow melts much sooner upon land newly turned up by the plow, than, *cæteris paribus*, in the neighbouring ground; which argues a warmth in that newly exposed earth; though, according to the touch, it would questionless appear cold. But we may be furnished with a clearer, and more pregnant instance, by but recalling to mind, what was just now mentioned of the warmth of tepid water, which was not to be felt by a hot hand, but produced there, a contrary sensation of cold. Which instance I therefore scruple not to repeat, because it affords an experiment in favour of that premised distinction, which, I think, may also have this ground in reason, that a considerable heat is often requisite to be sensible to our hands, &c. which are continually irrigated with the circulating blood, that comes very warm out of the heart, and enlivened by animal spirits, plentifully supplied from the brain.

If *Eleutherius* thinks fit to accommodate this distinction in the vulgar, and in the philosophical sense to his heat, formally and materially taken, I leave him to his liberty. And I shall also leave it to you both, Gentlemen, to accommodate to cold, *mutatis mutandis*, as they speak, what has been said about the distinctions of heat; because, I fear, *Themistius* thinks himself to have been too long detained already from proposing his arguments, which he may now begin to do as soon as he pleases.

SECTION II.

Them. I WILL then, with your permission, begin with that argument of *Gassendus*, which I am able to give you in his own words; because, upon the occasion of Mr. *Boyle*'s book, I made a transcript of what he says, to evince the positive nature of cold; and having the transcript yet about me, it is easy for me to tell you, that it is this: * *Ii sunt frigoris*

* Vid. Gassend. Physicam. Sect. I. Lib. VI. cap. 6.

frigoris effectus, quales habere privatio, quæ actionis est incapax, non potest.

THIS argument, though he begins not with it, I chuse to make the first, because I think it of such weight, that, though it were the only one he could alledge, it would serve his turn and mine, since it is drawn from the effects of cold, which, though he mentions them but in few and general words, experience shews to be both so manifold and so considerable, that if *Carneades* employ an hundred times as much time to answer the argument they afford, as I have done to recite it, he will, I think, do no more than would be necessary, and perhaps not enough to be sufficient. For cold affects the organs of feeling, and sometimes causes great pain in them, condenses air and water, and breaks bottles, that are too well stopped, congregates both homogeneous and heterogeneous things, increases hunger, checks fermentation in liquors, produces heat by antiperistasis, in deep cellars, mines, &c. and yet freezes men and beasts to death, dismantles whole woods and forests of their leaves, and does (I know not how many) other feats; among which it is not the least admirable, though one of the most common, that it turns the fluid and yielding waters of rivers and lakes, and sometimes of part of the sea itself, not too far from the shore, into firm and solid ice, which is often in northern climates strong enough, not only to be travelled upon by merchants with their carriages, but to be fought upon by whole armies with their trains of artillery. From which, and other instances, it is manifest, that effects so numerous and great cannot proceed from a mere privation, or any negative thing, but require a considerable, and therefore sure a positive quality to produce them.

Carn. THIS objection, *Themistius*, is, I confess, a considerable one, and of more weight than any of the rest, if not than all of them put together: but, as I think it very worthy to be answered, so I think it very possible to be well answered; and to give you my reasons for my so thinking, I shall distinctly consider in the argument the two particulars, which it seems to consist of.

AND first we are told, that if cold be but a privation, it cannot be the object of sense. To clear this difficulty, which, I know, you will think it very hard, if at all possible to do, I must beg your leave to observe something about sensation in general; not as designing an intire and solemn discourse of that subject, but because the particular remark I am about to make, is necessary to the solution of our present difficulty. I observe then, that that, which, at least in such cases as we are speaking of, produces in the mind those perceptions, which we call sensations of outward objects, is the local motion, caused by means of their action upon the outward organs in some internal part of the brain, to which the nerves belonging to those organs correspond; and the diversity of sensations may be referred to the differing modifications of those internal motions of the brain, either according to their greater or lesser celerity, or other circumstances, as our friend Mr. *Boyle* has somewhere exemplified in the variety of sounds; whereof some are grave, some sharp, some harmonious and pleasant, some jarring and offensive; and yet all this strange variety proceeds from the variations of those strokes or impulses, which the air, put into motion by sonorous bodies, gives to the ear.

To this it will be consonant, that as the air, or rather the mind by the intervention of the air, is differingly affected by a very grave sound, and a very acute one; though the former proceed from the want of that celerity of motion in the undulating air, which is to be found in the latter; which slowness, or imminution of motion, does, as such, participate of, or approach to, the nature of rest: so in the sensory of feeling there may, upon the contact of a cold body, be produced a very differing perception from that, which is caused by the contact of a hot body; and this, though the thing perceived, and by us called coldness, consists but in a lesser agitation of the parts of the cold body, than of those of the hot body, in respect of our hands or other organs of feeling.

AND this leads me, for the farther clearing of this matter, to represent to you, that since it is manifest, that bodies in motion are wont to communicate of their motion to those more slow bodies they happen to act upon, and to lose of their own motion by this communicating of it: since this, I say, is so, if, for instance, a man take a piece of ice in his hand, the agitation of the particles of the sensory will, in good part, be communicated to the corpuscles of the ice, which, upon that account, will quickly begin to thaw; and the contiguous parts of the hand losing of the motion they thus part with to the ice, there needs nothing else to lessen the agitation they had before. And there needs no more than this slackening, or decrement of agitation, to occasion in the mind such a new and differing perception, as men have tacitly agreed to refer to coldness.

Eleuth. IT seems by this discourse, *Carneades*, that you think, that sensation is properly and ultimately made in, or by the mind, or discerning faculty; which, from the differing motions of the internal parts of the brain, is excited and determined to differing perceptions; to some of which men have given the names of heat, cold, or other qualities. So that, according to you, if a considerable change or variation be made in the most ordinary, or in the former motion or modification of motion of the parts of a sensory, and consequently of the parts, that answer them in the brain, new sensations will be produced, whatever the cause of this alteration be, whether privative or positive.

Carn. YOU do not misapprehend my thoughts, *Eleutherius*, and what you say gives me a rise to illustrate this matter yet a little farther, by observing, that the sensories may be so accustomed to be affected after a certain manner by those external objects, whose operation on them is very familiar, or perhaps almost constant, that the privation, or the bare imminution of the wonted operation leaves the parts of the sensory, for want of it, in a different disposition from what they formerly were in; which change in the sensory, if it be not too small, will be attended by a perception of it in the mind. To declare and confirm this by an example, we may consider, that though darkness be confessedly a privation of light, and the degrees of it gradual imminutions of light; yet the eye, that is, the perceptive faculty, by the intervention of the eye, may well enough be said to perceive both light and darkness, that is, both a positive thing, and the privation of it. And it is obvious, that the motion of a shadow, which is a gradual privation of light, is plainly, and without difficulty, discoverable by the eye; of which the reason may be easily deduced from what I have been lately saying. And to shew you, that there is on these occasions such a change made in the organs of seeing, as is visible even to by-standers, I shall need but to appeal to the experiment of making in the day time a boy or girl look towards an enlightened window, and then towards an obscure part of the room; for when the latter comes to be done, you will plainly perceive, that for want of such a degree of light, as was wont to come in at the pupil, and straiten a little that perforation of the uvea; that round circular hole, or, as you know they call it, apple of the eye, will grow very manifestly larger than it was before, and than it will appear again, if the eye be exposed to a less shaded light.

This observation may be seconded by what happens to a man, when coming out of the sun-shine, where the sun-beams much contract his pupil to shut out an excessive light, that would be offensive to the organ, he comes presently into a dark room, where he must continue some time before he can see others, as well as he is seen by them, whose pupils have had time to be so enlarged, as in that darker place to let in light enough to make objects visible to their eyes, which are not so to his, whose pupils are yet contracted by the light they were but just before exposed to. To this I might add divers other phænomena, explicable upon the same grounds; but I shall rather chuse to relate to you an uncommon accident, which happening to eyes somewhat unusually disposed, does more remarkably discover, what alteration darkness, or a privation of light, may have upon those organs. I know a very learned man, who is no less studious of mathematicks, and other real parts of knowledge, than skilled in those, which are taught of the schools: this virtuoso, who seemed to me to have something peculiar in his eyes, confessed, and complained to me, that if he come, though but out of a moderate light of the open air, into a room, that is any thing dark, he does not only feel such an alteration, as other men are wont to do on the like occasion; but is so powerfully affected by it, that he thinks, he sees flashes of fire before his eyes, and feels a troublesome discomposure in those parts, that sometimes lasts an hour or two together, if he so long continue there.

Eleuth. I know not, *Carneades*, whether after this you will think it any great confirmation of your opinion, that *Aristotle* has somewhere this saying, that, *Oculus cognoscit lucem & tenebras.*

Carn. I thank you, *Eleutherius*, for so pertinent an allegation; though not for my own sake, yet for theirs, that will more easily receive a truth upon the testimony of *Aristotle*, than that of nature. And now, I hope, that *Themistius* will consent, that, dismissing the argument hitherto examined, we proceed to the next.

SECTION III.

Them. SINCE you will have it so, I shall comply at present, and the rather, because, not only I foresee there will be occasion to speak of it again, but because you experimental philosophers, that are wont so much to cry up the informations you think you receive from sense, sometimes, in spite of contrary dictates of reason, will, I hope, be prevailed with by the argument I am about to propose, which is so manifestly grounded upon sense, that without denying, that we do feel what we feel, we cannot deny cold to be a positive quality. For thus *Gassendus* most convincingly argues; *Cùm per hyemem immittimus manum in labentis fluminis aquam, quod frigus in ea sentitur, non potest dici mera privatio, ailúdque prorsus esse apparet sentiri aquam frigidam, & sentiri non calidam. Et fac eandem aquam gelari, sentietur haud dubiè frigidior: an dices hoc esse nihil aliud quàm minùs calidam sentiri? Atqui calida jam antea non erat: quomodo ergo potuit minùs calida effici?*

Carn. I will not say, *Themistius*, his argument is not specious, but you, perhaps, or at least *Eleutherius*, will not affirm it to be more than specious, if you please to consider, with me, two or three things, that I have to suggest about it.

And first, to shew *Themistius*, that, whatever he was just now intimating, experimental philosophers do not prefer the immediate impressions made on the senses to the dictates of reason, though they think the testimony of the senses, however sometimes fallacious, much more informing than the dictates of *Aristotle*, which are oftentimes, and that groundlesly, repugnant to them; I will represent to you, that the organs of sense, considered precisely as such, do only receive impressions from outward objects, but not perceive, what is the cause and manner of these impressions, the perception, properly so called, of causes belonging to a superior faculty, whose property it is to judge, whence the alterations made in the sensories do proceed, as may easily be proved, if I had time and need to do so, by many instances, wherein the senses do, to speak in the usual phrase, mis-inform, and, as far as in them lies, delude us, and therefore must be rectified

rectified by reason. As when the eye represents a strait stick, that has part of it under water, as if it were crooked; and two fingers, laid cross over one another, represent us a single bullet, or a button, rolled between them, as if there were a couple: so that it is very possible (for I forbear saying it is true, having not yet proved it) that though the sensory be very manifestly, and vehemently, affected upon the contact of cold water, or other cold bodies, yet the cause of that impression, or affection, is, and may be judged, and determined by reason to be, other than that, which the sense may to an inconsiderate person suggest. As when a child, or one, that never heard of the thing before, first sees a stick, whereof one part is in the air, and the other under water, he will presently, but erroneously, conclude that phænomenon to be caused by the sticks being crooked, or broken.

NEXT we may consider, that sensations may, in divers cases, be made, as well from alterations, that may happen in the internal parts of the body, as from those, that are manifestly produced in the external organ, by external objects and agents; as may appear by hunger, thirst, the titillation of some parts of the body, barely upon venereal thoughts, and (which belongs directly to our present argument) the great coldness, that we have known hysterical women complain of in their heads and backs, and the great, and troublesome degree of cold, which we every day observe, upon the first invasion of the fits of agues, especially quartans; which troublesome symptoms, that sometimes last for several hours, are therefore commonly called the cold fits.

AND now it would be seasonable for me to call upon you to remember (and add to what I have now said) that, which, at the beginning of our conference, I took notice to you of, about sensation in general; if I did not presume, that those things are yet fresh enough in your memory, to allow me to proceed directly to answer the objection, which I shall do, though not like a school-man, yet like a naturalist, by giving an account of the proposed phænomenon, without having recourse to that hypothesis, which it is urged to evince.

I observe then, that though, in the respective sense above-mentioned, water, wherein the objection supposes the hand to be plunged, be cold, in regard its parts are less agitated, than the spirits and blood harboured in the hand; yet, in a philosophical sense, it is not quite destitute of heat, since it is yet water, not ice, and would not be a liquor, but by reason of that various agitation of its minute parts, wherein fluidity, a quality essential to liquors, consists. Upon the score of this respective coldness of the water, the hand is refrigerated; for the spirits and juices of that organ meeting in the water, with particles much less agitated than they are, communicate to them some part of their own agitation, and thereby lose it themselves, upon which decrement of wonted agitation, such a change is made in the sensory, and, though not so manifestly in some other parts of the body, as is perceived by the animadversive faculty under the notion of coldness; sensation (whatever obscure definitions are wont to be given of it) being indeed an internal perception of the changes, that happen in the sensories.

AND if now, as the objection supposes, the water, wherein the hand is plunged, comes to be more refrigerated than before, the spirits, blood, and other parts of the hand, finding the aqueous corpuscles more slowly moved than formerly, must, according to the laws of motion, (according to which a body, that meets another much more slowly moved than itself, communicates to it more of its motion, than if it were less slowly moved) transfer to them a greater measure of their own motion, and consequently themselves come to be deprived of it: and upon this increase of the slowness of motion in the parts of the hand, there follows a new and proportionable perception of the mind, and so a more vehement sensation of cold. But though it be not to be admired, that the bare slowness of motion in the object should be discernable by sense, albeit it seems to participate of rest, which, with you, passes for a privation, since the ear perceives, when a voice grows faint, and when a sharp sound degenerates into a flat one; and we can perceive by the hand (abstracting from heat and cold) the celerity or slowness of bodies, that in their passage strike upon it, as for instance, of winds, or streams; yet this is not the only thing I think fit to be taken notice of on this occasion. For I consider farther, that besides the most consistent and stable parts of the hand, there are, from the heart and the brain, fresh blood and spirits continually transmitted to the hand; and the former of these, the blood, is, according to the laws of its circulation, and after it has received a great change in the much refrigerated hand, carried back through other parts to the heart; whence it is, in the same circulation, distributed to the whole body. To which may be added, that when the great refrigeration of the hand happens, external agents may contribute to the effects of it, as I shall by and by have occasion to shew.

IF then you please to remember, that upon the turning one's eye to the dark part of a room less enlighted than the window, though darkness be but a privation, and though the obscurity of that part be not absolute, but consist only in a less degree of light; yet the action of the spirits, and other parts of the body, is so changed, upon occasion of the light's acting more faintly than was usual upon the organ, that the pupil is immediately and manifestly dilated, and in some cases, as in that, which I mentioned to you of a learned man, much considerabler effects ensue; you will not wonder, that, where not only the spirits, but the blood, (whence those spirits are generated) that circulates through the whole body, and upon whose disposition all the other parts so much depend, is very much disaffected, there should be felt a great alteration in the hand, which is the most immediately exposed to the action of the

the cold water. And for the reasons newly given, it ought to be as little strange, that in other parts of the body, the disordered, and not circulating blood, should have its wonted action on them considerably altered; since the more stable parts, and especially those external ones, that are most exposed to the cold, have their pores straitened, and consequently their texture somewhat altered; on the same occasion, on which the wonted agitation of the spirits, with the particles, that compose the blood, is notably lessened. And that such causes may produce great effects in a human body, you will be more prone to admit, if you consider the disorders, that happen in the cold fit of an ague, and oftentimes, upon the shutting up of those excrementitious steams, that are wont to be discharged by insensible transpiration; to whose being stopped in the body, by the constriction of the pores, which chiefly happens through cold, some learned physicians, especially the famous *Sennertus*, impute the cause of most fevers, as indeed experience itself does but too frequently shew it to be guilty of many.

Phil. I confess, *Carneades*, you have said some things, that I thought not on before; but yet *Gassendus*'s argument seems to be such, that I fear it will be hard to hinder many from saying, That if cold be but a privation of heat, it is a privation of a strange nature: for it may be introduced into bodies, that were not hot before, nay, in some cases, into such as are naturally cold, and also by consequence must have been put into a preternatural state to be at any time hot.

Carn. This objection, *Philoponus*, being in effect so much the same with that of *Gassendus*, that it differs from it but in the dress you give it, it will scarce require a peculiar and distinct answer; and therefore, as soon as I have reminded you of the distinction, that we have formerly made of the vulgar and philosophical sense of the word cold, I shall need to alter but a little what I said before, by telling you, that since fluidity consists in the various agitation of the insensible corpuscles of a liquor, and that heat consists in a tumultuary, but a more vehement agitation of the insensible parts of a body, and so, that hot water scarce differs otherwise than gradually from that, which is cold to sense; if cold be taken in the larger and philosophical sense, it may well be said, that as long as water retains the form of water, and so continues to be a fluid body, though it may be very cold to the touch, yet it is not absolutely or perfectly cold, and therefore is capable of a farther degree of coldness, which it receives when brought to congelation: for till then it was not destitute of those agile corpuscles, that were requisite to keep it fluid; and till then, *Gassendus* himself must acknowledge, that it was not absolutely, or perfectly cold; because he, as you may remember, did in his former (but lately-mentioned) argument ascribe the glaciation of water to the invasion of those, that he calls corpuscles of cold.

Eleuth. Give me leave to add, *Carneades*, that it is not every glaciation it self, that brings liquors to be perfectly cold in the philosophical sense of that expression, and quite expells, or subdues all the agile particles, that were in the water before it was turned into ice. For I think, that to effect this change, it is sufficient, that so many of these restless particles be destroyed or disabled, that there remains not enough of them to keep the water in a state of fluidity, so that the surplusage may yet continue in the frozen liquor, and whilst they are there, perform several things, as the making it evaporable in the air, and even odorous, and by their recess or destruction the ice may grow yet more cold. And as this notion suits very well with the differing degrees of hardness, that we find in differing portions of ice, sometimes upon the account of the matter, (as frozen water is harder than frozen oil,) and sometimes upon that of the different degrees of cold in the same water, or other matter, (as our friend somewhere observes;) so it may be highly confirmed by an experiment I saw him make, but that is not yet published.

The sum of the experiment was this; that he first put an hermetically sealed thermoscope into a glass, broader at the top than at the bottom, and greased the inside with tallow, that ice might not strongly stick to it. In this glass was put water, more than enough to cover the ball of the instrument; and that water being warily frozen, notice was taken, whereabouts the tincted spirit of wine rested in the stem; after which, the instrument and the ice being removed into the open air, upon an exceeding frosty morning, the ice was taken off from the ball, and presently after, the tincted liquor, as the maker of the trial expected, subsided a pretty way (the length of the instrument considered) below the former mark; which argued that he rightly guessed, that such a degree of cold, as is sufficient to turn water into ice, may not produce a body perfectly cold; this ice it self keeping the enclosed ball, in a sense, warm, by fencing off the air, which, at that time, (even in our temperate clime) by the effect, appeared to be colder than the very ice. And, methinks, it may strengthen *Carneades*'s discourse, to represent, that there is no sufficient cause, why many things, that are reckoned among privations, or negations, by the Peripateticks themselves, as well as cold is by *Carneades*, may not admit of degrees; as may be exemplified by deafness, ignorance, and divers other things. And to bring a case, not very unlike that under consideration, we may take notice of a total eclipse of the moon, which you know always happens when she is at the full. For darkness in the air being acknowledged to be a privation or negation of light, when the earth, interposed between the moon and the sun, has eclipsed her, for instance, nine digits, (as astronomers speak,) men generally complain of darkness in the air, though there remain a considerable part of the discus, or the hemisphere of the moon, obverted to us, yet enlightened by the sun; but when the interposed earth proceeds to cover the remaining three digits, and so makes the eclipse total, the darkness also is said and esteemed to be much encreased: nor would men otherwise be persuaded, though *Themistius* should tell them, that the air cannot have grown darker, though it were dark before; and indeed though the air was more and more darkened in proportion to the increase of the eclipse, yet it was never completely darkened till it became total. But I fear I dwell too long upon one argument.

SECTION IV.

Eleuth. LET me therefore, *Carneades*, sum up what I take to be your doctrine, and tell these gentlemen, that I think you do not look upon the sensation of cold as a thing effected by an intire privation, properly so called, and considered as such; but that, according to you, that slowness of motion in the particles of cold water, which the hand finds, when it is thrust into that liquor, does occasion the spirits, and the corpuscles of the blood, to part with to those of the water a considerable share of their own surplusage of agitation, whereby they lose it themselves; upon which is consequent a perception of this change made in the hand, which, if it be very great, is also frequently accompanied with some sensible change in other parts of the body, occasioned chiefly by the frequent returns of the circulating and highly refrigerated blood to the heart, whence it is dispersed to the whole body. According to which doctrine, the sensation of cold is but a perception of the lessened agitation of the parts of the hand, either stable or fluid, especially of the blood; which alterations are in great part produced, not by the coldness of the water, as cold is a privation, but from the new modification of the action of the blood and spirits upon the nervous and membranous parts, the constriction of whose pores concurs to that modification. And if I do not misunderstand your opinion, *Carneades*, methinks it may be confirmed by this, which I have known observed by experienced surgeons, that by too strict ligatures unskilfully made, an arm, for instance, may be gangrenated; in which case, all the proper and immediate effect of the ligature is but the constriction of the part, though that constriction being unusual and excessive, it proves the occasion of the mortifying of the hand and arm, by hindering the free and usual access of the blood and spirits to that limb; upon which, by the depraved action of the parts of the body one upon another, and the concurrence of external agents, there ensues a mortification, or gangrene of the part, which, if due remedies be not timely employed, is communicated to other parts, and kills the man.

Carn. Whatever become of your instance, *Eleutherius*, I thank you for your readiness to propose it in favour of my hypothesis, which you will easily judge not to be much concerned in the close of the excellent *Gassendus* his argument, for the positive nature of cold. For though these words of his ——

Them. You may save your self the trouble of naming of them now, since, whatever they may seem to you, I profess I look upon them, as containing a distinct argument, which I shall therefore propose in its due place hereafter; but in the mean time, and before we leave the argument you would have us dismiss, give me leave to remind you, *Carneades*, of some part of your former discourse, and to take thence a rise to tell you, that you, who told us, that we ought not to consider the operations, that qualities have upon our own sensories only, but also what they do to other bodies, will, I hope, allow me to demand, how a privation, or, if you will, how an imminution of motion can produce the hundredth part of those effects, which we daily see produced by cold in the bodies, that are about us.

Carn. I thought, *Themistius*, I had intimated to you already, what might have prevented your question; but since I see it is otherwise, you shall not find me backward to explain my self a little more fully. I do not pretend, that either an absolute privation of motion in a body, or a slowness of motion in the parts of it, is, as such, the proper efficient cause of the effects, vulgarly but unduly ascribed to cold alone; for, in my opinion, cold is rather the occasion, that the true efficient cause of such effects, which, I think, are properly to be ascribed to those physical agents, whose actions, or operations, happen to be otherwise modified, than else they would have been upon the occasion of that imminution or slackness of agitation, which they meet with in cold bodies, by occasion of which, they are both deprived themselves of the agitation they communicate to such slow bodies, and thereby act no longer, as, were it not for that loss, they would, and by a natural consequence of this change, which is made in themselves, they do also, though less notably, modify the action of other bodies upon them: From which unusual alterations happening in a world so framed as this of ours is, and governed by such laws, respecting motion and rest, as are observed among bodies, there must, in all probability, result many new, and some of them considerable phænomena. For though quiescent bodies seem not to have any action, which among corporeal substances seems to be performed only by local motion; yet bodies quiescent themselves may concur to great effects, both by determining the motions of other bodies, this or that way, or by receiving their motion totally, or in part, and so depriving the formerly moving bodies of it. Thus the arches of a bridge, though immoveable themselves, by guiding the water of the river, that beats against them, may occasion a rapid and boisterous stream, capable to drive the greatest mills, and perform more considerable effects, though the river, before it met with them, ran calmly enough, as is evident at *London* bridge, especially when the water is near a low ebb. And now I have mentioned water, I will add, that though water it self be not a quiescent body, but, being a liquor, has its parts in perpetual motion among themselves; yet since that agitation is exceeding slow, flow, in comparison of the swiftness of a cannon-bullet, in respect whereof the calm surface of the water participates of the nature of a quiescent body, bullets themselves shot from out of guns elevated but little above the level of the water, (upon which score they make but a very sharp angle with it;) these bullets, I say, do not unfrequently rebound from the surface of the water, and consequently, even these so wonderfully swift bodies receive a new determination from it.

Eleuth. One may add, *Carneades*, to your instances, that in a tennis-court the wall, against which balls are strongly impelled by a racket, contributes much to the mischief, that those balls do often to by-standers in the gallery, as the wall, though itself unmoved, gives a new determination to the moving ball, and by its resistance makes it rebound or reflect at an angle equal to that of the ball's incidence. And this concurrence of the wall to such effects is the more evident, because of this other circumstance, which also befriends your opinion, that, if the impelled ball, instead of hitting against the wall, hits against the net, this, by yielding, deprives the ball of its impetus, and hinders the reflection, that would else ensue.

Carn. You have, I confess, somewhat prevented me, *Eleutherius*; but yet not altogether: for though I was going to propose the example of a ball, yet it was in somewhat a differing way; for I was about to propose to *Themistius* the example of a ball, which, if it be forcibly and perpendicularly thrown against the hard ground, has its determination so altered, that whereas it moved before towards the centre of the earth, it immediately, with almost the like swiftness of motion, tends directly upwards. And if on the other side you throw the ball, not against a hard, but against a muddy piece of ground, it will not rebound, losing its own motion, by communicating it to the parts of the yielding mudd; as may be in some measure illustrated by the great commotion made in a small pond of water, when a ball (or a round stone) being but gently let fall upon the surface of it, has its motion thereby deaded, and transferred to the parts of the liquor, which, perhaps, will be visibly agitated at the remotest brink of the pond.

Eleuth. These examples may conduce much to explicate your doctrine, *Carneades*; but since *Themistius* himself was so equitable a while ago, as to allow you much time to defend such a paradox as yours against *Gassendus*'s argument, I shall with your leave, of which I doubt not, to the examples already mentioned, add this one more. Suppose upon a stream, that runs through some town, which is not very rare, there were built a number of differing mills, some for the grinding of corn, others for the fulling of cloth, others for the moving of bellows to melt oars and metals, others for forging of sword-blades, others for making of paper, and others for other uses: and suppose, that an enemy coming to besiege this town, should successfully imitate *Cyrus*'s stratagem, when by suddenly diverting the course of *Euphrates* he took *Babylon*; would it not be consequent to this dirivation of the water into some lower place, and this ceasing of the stream to run in its former channel, that the action of all these mills, by which so many differing operations were performed, must of necessity cease too? though the besiegers do not produce this change by any positive and direct violence, that they offer to the mills, but only by hindering them from receiving the wonted impulses, which were requisite to keep them in motion.

Carn. I dislike not your instance, *Eleutherius*, which yet will not altogether render useless what I was going to say about a wind-mill, which will illustrate one part of my doctrine, for which your water-mill does not seem to have been intended. And, that this example may the better do so, I will suppose a wind-mill to be built in some low place near the bank of your stream, which stream we will suppose to be liable, as some others are, upon the falling of great and sudden rains upon the neighbouring hills, to overflow its banks, in case the increase of the water be not then hindered by the wind-mill's lifting up constantly some parts of it, and conveying it away by pipes or otherwise: and then let us suppose, what really sometimes happens, that the wind should so cease, that there should not blow any wind strong enough to move the sails for a great while together; will it not hence manifestly follow, that by reason of this absence of the wind, which absence has the nature of a privation, or negation of a stream-like motion in the air, not only there will be a ceasing of those effects and operations, whatever they were, that were wont to be performed within the mill itself, but also there will be a durable intermission of that main work of the mill, whereby it carried off such a quantity of water; which work ceasing with the wind, whilst the flowing in of the water does not cease too, but continues as formerly, the still-increasing water must bear down or overflow its wonted banks, or other boundaries, and by its unruly effusions drown the neighbouring parts, and produce the disorders, that is, the new phænomena naturally consequent to an inundation made by such a quantity of water. And if the water conveyed away, by means of the mill, through pipes or channels, were employed to water grounds, or other particular uses, the growth or fertility, at least of the vegetables, that water was requisite to nourish, or the other uses, to which it was necessary, must consequently be much, if not totally, hindered.

Phil. I know not, whether we may not refer to the subject of your discourse, what may be observed in paralytick affections, where a little viscous or narcotick humour, obstructing, or otherwise disaffecting one part of a nerve, though its proper and immediate action be only to hinder, or weaken the spirits, that were wont, in competent plenty, to pass freely along the nerve to the muscles whereto it leads; yet the action of the other parts of the body, and the relaxation of the fibres do oftentimes produce a tremulous motion in the limbs, and particularly the hands; and sometimes also the mouth,

mouth, neck and other parts, are drawn awry in an odd and frightful manner.

Carn. THOUGH I approve of *Philoponus*'s fancy, yet I think a more quick and notable instance to the same purpose may be taken, from what happens to birds, and rats, and cats, and such kind of warm animals, in Mr. *Boyle*'s engine. For, as the air by the agitation of its parts, or that of some ethereal substance, that pervades it, entertains the fluidity of water, and other aqueous liquors; and when that agitation is hindered, or too much lessened, water ceases to be fluid, and upon that divers violent effects ensue, wont to be ascribed to glaciation: so the bodies of warmer animals, having been borne in the air, and perpetually exposed to the action of it (though that be seldom heeded) when being placed in the receiver of the air-pump, and by the operation of that instrument, which withdraws the former air, and keeps out the new, the air, that was wont continually to act upon them, is kept from doing so any longer, though this absence, or not touching of the air, be but a privative or negative thing, yet by reason of the structure of the animal, his spirits and humours, assisted by the concourse of more general causes, are brought to act so differingly from what they were wont to do, that the blood and juices swell, the stomach vomits, the animal grows faint and staggers, the limbs, and at length the whole body are convulsed, the circulation is stopped, and at last the whole animal killed; and all this done in a very few minutes of an hour, without the visible intervention of any positive agent.

Eleuth. WHAT you say, *Carneades*, concerning the quick and violent death of warm animals in Mr. *Boyle*'s engine, puts me in mind of an experiment I saw made in that instrument upon cold animals, which, methinks, may well illustrate the comparison we lately employed of a wind-mill. For as those great artificial engines lose their motion, and the operations depending on it, if that stream of air, we call the wind, be held from keeping them going; so insects, and some other cold animals, have their differing motions so dependent upon the contact of the air, that, as soon as ever they are deprived of it (by the engine we are speaking of) divers sorts of them will lie moveless, as if they were dead; and I have known several of them, that were put in together, continue in that state for many hours, as long as it pleased our friend to with-hold the air; but when once he thought fit to let a stream of air enter the receiver, these seemingly dead animals, as worms, bees, flies, &c. like so many little wind-mills of nature's (or rather, her great author's,) making, were set a moving in various manners (as creeping, flying, &c.) suitable to their differing species.

Carn. So that, to sum up, in a few words, the result of these instances, and the rest of the past discourse on the same subject, it appears by what has been said, that the effects undeservedly ascribed to cold need not, in our hypothesis, be referred to a privation, but to those positive agents, or active causes, which, by their own nature, are determined to act otherwise, or suffer otherwise from one another, in cases, where there is a great hinderance, or ceasing of wonted agitation, than where there is not.

SECTION V.

Them. IT may, perhaps, now be time to put *Carneades* in mind, that, in what he has been discoursing all this while, he has proposed answers but to a couple of *Gassendus*'s arguments, and left the rest untouched.

Carn. I should readily grant, *Themistius*, that I have dwelt too long upon so few arguments, if I did not hope, that by fully answering them, and giving the company a particular account of my notions concerning cold, I might very much shorten and facilitate the remaining part of my task, which engages me to return answers to the other arguments you speak of, the grounds of solving which, I think, I have already laid in the past discourse. And therefore you may go on to propose the next argument of *Gassendus*, as soon as you please.

Them. And I shall do it, *Carneades*, in that learned man's own words, which I well remember to be these: *Fac manum immitti in aquam nunc calidam, nunc frigidam; quamobrem manus intra istam, non intra illam refrigeratur? An quia calor manûs intra frigidam retrahitur, manûsque proinde relinquitur calida minùs? At, quidnam calor refugit, quod intra frigidam reperiatur? nonne frigus? at si frigus est tantùm privatio, quidnam calor ab illa metuit? privatio sanè nihil est, atque adeò nihil agere, unde ejus motus incutiatur, potest.* Gassend. Lib. VI. cap. 6.

Carn. This objection, *Themistius*, may indeed puzzle many school-philosophers, but will easily admit an answer in my hypothesis. For that does not oblige, or so much as tempt me to ascribe (as a Peripatetick would do,) to a meer quality, (for such is heat) both a knowledge of its danger, and a care, and skill, to preserve itself from its enemy, the cold, by a retreat inwards. For, agreeably to what I lately delivered, it is obvious for me to explicate the phænomenon thus: when a man puts his hand into warm water, the agitation of the corpuscles of that liquor surpassing that of the spirits, blood, and other parts of his hand, cannot but excite in him a sense of heat; but when he puts the same hand into cold water, the case ought to be much altered, not by any imaginary retreat of the spirits, but the communication of motion, by other parts, to the surrounding water, by which means, there must be in the hand a great lessening of the former agitation of its parts, the perception or sense of which decrement of motion is that, which we call the feeling of cold.

Eleuth. I think indeed, *Carneades*, that though this argument may be considerable against those, that the learned framer of it might have in his eye, it is but invalid against you. But can you as well decline the force of that other objection, which *Gassendus* more insists on, and which seems as directly to oppose you, as any other adversaries of his hypothesis?

Them.

Them. I presume, *Eleutherius*, you mean that cogent argument, with *Gassendus* proposes, and prosecutes more fully, than the rest, deducing it from the way of artificially freezing water by a mixture of snow and salt; placed about the outside of the glass, that contains the liquor. For, from this practice, he rationally concludes, that since this frigorifick mixture is, through the glass, able to freeze the water into ice, it may as justly be affirmed to act by corpuscles of cold, as fire can be to act by calorifick corpuscles, when kindled coals, placed on the outside of the glass, make the contained water boil. And this cogent argument will, I hope, prove the more satisfactory to *Carneades*, since it is not drawn from what he would call a disputable peripatetick notion, but from the same quiver, whence he affects to take his shafts, experience itself.

Carn. I freely acknowledge, gentlemen, this argument to be very plausible; but that it is clear and cogent, I must not grant, till I be better satisfied, that it is so.

AND I shall scarce think it as evident, that ice, and salt, act by a positive quality, as that burning coals do so, though cold seems as well to be produced by the former, as heat by the latter. For innumerable experiments shew, that heat, in the fire especially, is a positive quality, consisting in a tumultuary and vehement agitation of the minute parts of the body, that is said to be hot, and producing also in the bodies, that it is communicated to, a local motion, which is manifestly a positive thing. This is so evident, in the heating of bodies by mere attrition, the smoking and melting of divers bodies in the sun-beams (especially at fit times of the day, and year,) the sudden boiling and dissipation of water, oil, &c. dropped, on a red-hot iron, and many other obvious instances, that it were a needless work to go about to prove it, especially, since both *Themistius*'s Peripateticks, and *Gassendus* himself, who so often disagree about other things, agree in confessing, that heat is a positive quality.

Them. BUT remember, *Carneades*, that the grounds, on which they do so, are the same, on which *Gassendus* justly builds the proposition, that cold also is a positive quality.

Carn. I did not forget that, *Themistius*; for I was about to subjoin to what I last said, that it is evident, not only by the confession of my adversaries, but by that (which to me is much more considerable) of nature herself, proclaiming it in the instances I just now mentioned, that heat is a positive quality; whereas, that cold likewise is so, does not appear to me by the experiment of artificial congelations. For, in this, all that is clear in matter of fact, is, that snow, or beaten ice, and salt, are put about a vessel full of water, or other aqueous liquor, and that, within a while after, this water begins to be turned into ice; but that this glaciation is performed by swarms of atoms of cold, that permeating the glass, invade and harden the liquor, is not perceived by sense but concluded by a ratiocination, the cogency of which I am allowed to examine, without affronting the certainty of sense, that not being concerned in the case. If then an intelligible way can be proposed of fairly explicating the phænomenon, besides that insisted on by *Gassendus*, the objection drawn from this experiment against my hypothesis will be invalid. And such an explication, monsieur *Des Cartes* ingeniously gives in his meteors: *Quia materia subtilis* (says he) *partibus hujus aquæ circumfusa crassior aut minus subtilis, & consequenter plus virium habens, quàm illa quæ circa nivis partes hærebat, locum illius occupat, dum partes nivis liquescendo partibus salis circumvolvuntur. Facilius enim per salsæ aquæ quàm per dulcis poros movetur, & perpetuò ex corpore uno in aliud transire nititur, ut ad ea loca perveniat, in quibus motui suo minùs resistitur: quo ipso materia subtilior ex nive in aquam penetrat, ut egredienti succedat, & quum non satis valida sit ad continuandam agitationem hujus aquæ, illam concrescere sinit.* Lib. Meteor. Cap. III.

Phil. I leave *Themistius* to consider, whether this explication be without exception; but I confess it is not without analogy, and that even amongst the four first qualities themselves. For when we chemists have a mind to dry (for instance) the calces, or precipitates, or other powders, from which we have filtrated the liquors we employ to wash or dulcify them, it is usual either to put the filters, wherein these powders remain almost in the form of mudd, or to spread the stuff itself upon brown paper, or pieces of brick, or chalk, which much hasten the exsiccation of the things laid upon them, not by any drying particles which they emit into the soft substances, but by imbibing the superfluous parts of the liquor, and thereby freeing from them the substances to be dryed. And I remember, I have seen our friend Mr. *Boyle*, by immersing a piece of soft crumb of bread into an actually cold liquor, that would hastily imbibe its aqueous corpuscles, and dry it in a minute, or two, of an hour, so as to make it feel hard.

Eleuth. These instances bring into my mind another chemical experiment, that I have seen made by the same gentleman, which was; that by putting into weak spirit of wine a sufficient quantity of salt of tartar, he quickly defleghmed the spirit without distillation, or so much as heat. And this will the better illustrate the *Cartesian* explication, because it is manifest, by the change, that will be made of the most part of the salt of tartar into a liquor, that will not mix with the now defleamed spirit of wine, that the reason of the operation is, that the aqueous particles of the phlegmatick spirit, finding, it seems, more convenience, or facility, to continue their motion among the fixed corpuscles of the salt, than the vinous ones of the spirit, pass into the alkaly, and dissolve it; and thereby desert the liquor, through which they were diffused before. And I know another saline body, that so unites with water, as not to be, by the eye, distinguishable from it, and yet is of such a texture, that water is so much less disposed to mingle with it, than with spirit of wine itself, that it will forsake the body it kept in agitation

tion, to paſs into this ſpirit; and ſo leave that, which it kept in the form of a liquor before, to appear in the form of a conſiſtent body; which inſtance comes ſomewhat nearer, than the former, to the experiment of glaciation.

Carn. THOUGH what you have recited, gentlemen, be not unwelcome to me, yet, I think, I can propoſe you an experiment fitter to delucidate the Carteſian explication. For, I remember, that our common friend, having a mind to ſhew, that a ſmall proportion of agile matter, inviſibly diffuſed through a body, that would be otherwiſe conſiſtent, may bring it to, and keep it in the ſtate of fluidity; deviſed and ſhewed me the following experiment. He took camphire broken into ſmall bits, and caſting a convenient quantity of it upon aqua fortis, ſuffered it to float there, till, without heat, the camphire was diſſolved into a liquor, and it looked and felt like an oil, which, though ſhaken with the aqua fortis, would emerge to the top again. If this oil were kept well ſtopped, that the ſpirits of the menſtruum might not evaporate, it would (as he affirmed trial had taught him) continue long fluid, he having ſometimes kept it a year, or two, or more. And that it is the agile ſpirits of the aqua fortis, that keep the camphire fluid, he has made probable by divers things, that I muſt not now ſtay to recite. And that the quantity of theſe agile particles is but ſmall, I am induced to think by this, among other things, that when I have made a ſmall parcel of but moderate aqua fortis turn a pretty proportion of camphire into oil, and ſeparated that oil from it, I could, by caſting freſh camphire on the ſame menſtruum, reduce that alſo into the form of oil. Now, that theſe fluidifick ſpirits (if I may ſo call them) are not ſenſibly warm (no more than the Carteſian *materia cœleſtis*) in water, is manifeſt to the touch: and whereas I at firſt ſuſpected, that the reaſon, why the pouring of this oil into water doth preſently reduce it into camphire again, might by the coldneſs of the water; I after thought, upon a farther information, that the reaſon rather was, that the nitrous ſpirits being diſpoſed to paſs out of the oil into the water, this liquor readily imbibed and diluted them, and conſequently, diſabled ſo many of them, that thoſe, that remained, could not do their former work any longer: ſince he had tried purpoſely, that the reduction of the oil into camphire would preſently be made, though that liquor were not poured into cold water, but hot; ſo that the agitation, that it received from the particles of the menſtruum, though not to our touch ſenſibly warm, was much more efficacious, than that, which it received from the heat of the water.

Eleuth. I know not, whether beſides the inſtances, that have been now propoſed, one may not alledge ſuch an argument alſo in favour of the Carteſian opinion about cold, as would not be inſignificant, though it ſhould be made appear, that cold may ſometimes be produced by, or upon the emiſſion of corpuſcles, that in ſome ſenſe may be called frigorifick. For there may be corpuſcles of ſuch a nature, as to ſize, ſhape, and other attributes, as to be fit to enter the pores, and pierce even into the inward parts of water, and ſome other bodies, ſo as to expel the calorifick corpuſcles they chance to meet with, or to clog, or hinder their activity, or on ſome other account, conſiderably to leſſen that agitation of the minute parts, by which the fluidity of liquors, and the warmth of other bodies, is maintained. But even in ſuch caſes, though the agent, and the actions, that produce coldneſs, be poſitive things; yet the nature of coldneſs itſelf may conſiſt in a privation. As when a man is killed by a bullet, his death is effected by a poſitive, and even impetuous action, and yet death itſelf is but a privation of life. If alſo, in a dark room, a man caſt cold water upon a burning coal, though the water act by its poſitive quality of moiſture, and, by virtue of that, extinguiſh the fire, and, by that means, deſtroy the light, yet the darkneſs, that is conſequent upon this action, is not a poſitive thing, but a privation.

SECTION VI.

Phil. THE pauſe you here made, Gentlemen, makes me think it ſeaſonable to put the company in mind, that it begins to grow late, and therefore to call upon *Themiſtius* to produce what he has yet to alledge out of *Gaſſendus.*

Them. The philoſopher, you have named, has indeed another weapon to deſtroy the error about cold, which he confutes. And this argument, like a two-edged ſword, that cuts on both ſides, does not only confirm what he maintains, but deſtroy the chief objection, that can be made by his adverſaries. The argument I ſpeak of, he propoſes in theſe terms: *Tametſi multa videantur ex ſola caloris abſentia frigeſcere, nihilominus niſi frigus extrinſecus introducatur, non tam profectò frigeſcere quàm decaleſcere ſunt cenſenda. Eſto enim lapis, lignum, aut aliquid aliud, quod nec calidum, nec frigidum ſit, id ubi fuerit admotum igni calefiet ſanè; at cùm deinceps calor excedet, neque frigidum ullum circumſtabit, non erit cur dicas ipſum frigefieri potius quàm minùs calidum fieri, redirève in ſuum ſtatum.*

Carn. WHETHER this contain not a diſpute *de modo loquendi*, I ſhall leave the company to judge, by what I ſhall return in anſwer to it. I ſay then, that it ſeems to me, that there is in the diſcourſe an obſcurity, if not an ambiguity, though, I am confident, not affected by the candid *Gaſſendus.* But to anſwer as directly as I can; if we ſpeak only of a coldneſs, as to ſenſe, I ſee not, why water, or wood, or any ſuch body, that is heated by the fire, may not, upon its removal thence, be ſaid to grow cold, and not barely to *decaleſcere*, in our philoſopher's ſenſe of that word. For the heat and coldneſs of water, in reference to ſenſe, conſiſting, as I lately ſhewed in this, that the particles of it are more or leſs agitated, than the hand that is immerſed in it, they need nothing elſe to make the liquor grow cold, than ſuch an imminution of the briſk motion

motion of its corpuſcles, that they ceaſe to be as much agitated, as thoſe of our organs of feeling: and if this already impaired agitation be ſtill more and more leſſened, the liquor will ſtill grow colder and colder, without the help of any poſitive cauſe, until at length the agile parts, that kept it fluid, being quite expelled, or diſabled, the form of the liquor comes to be exchanged for that of ice.

Phil. But what ſay you to that part of *Gaſſendus*'s argument, where he propoſes an adiaphorous body, which, when affected with an adventitious heat, would not grow cold by the bare removal, or ceſſation of that heat, unleſs it were refrigerated by an agent, that were poſitively and actively cold?

Eleuth. I ſay, *Philoponus*, this ſuppoſition ſhould not be made, and that I know of no ſuch adiaphorous body. For ſince, as I have been obliged to inculcate, thoſe bodies muſt be cold, as to ſenſe, whoſe parts are leſs agitated than thoſe of our hands, and conſequently metals, ſtone, wood, and other ſolid bodies, and alſo water, wine, and all other unmingled liquors, we know, being heated by the fire, will grow cold again of themſelves, becauſe the adventitious motion ceaſing by degrees, either upon the receſs of the igneous corpuſcles, or the imparting of the extraneous agitation to the air, or other contiguous bodies, the ſtone, or water, &c. will again have ſo much fainter an agitation, than that of a man's ſenſory, as to be by him judged cold: and becauſe almoſt all the ſpecies of permanent bodies here below, that are known, have, in what is called their natural ſtate, a leſs degree of agitation in their inſenſible parts, than men's organs of feeling are wont to have, thoſe bodies may be ſaid to be naturally cold, and therefore ought not to be ſuppoſed to be indifferent to cold or heat.

Phil. But whether or no nature do really afford us an adiaphorous body; yet ſurely the mind is able to conceive one, and therefore *Gaſſendus* may be allowed to ſuppoſe ſuch bodies, and *Carneades* may be obliged to anſwer what he argues upon that ſuppoſition.

Carn. It is one thing to propoſe an adiaphorous body, as barely an intelligible, or a poſſible thing; and another, to give inſtances, of it, as *Gaſſendus* has done in particular bodies, in which that indifference is not to be found. And it is this laſt kind of ſuppoſition, that I diſallowed in *Gaſſendus*'s argument. But if a body ſhould be propoſed, as adiaphorous in reference to heat or cold, I might ſay, without prejudice to my cauſe, that if ſuch a body ſhould be carried into a hot place, it might there grow warm; and if it ſhould be removed back again, and kept, till it loſt that new adventitious heat, it might rather *decaleſcere*, than grow cold as to ſenſe. But the reaſon is, becauſe, it is not every degree of imminution of heat, that is able to denominate a body cold, but ſuch a degree as reduces the parts of it to a fainter motion, than is at that time, in thoſe of our organs of feeling; and till this be done, or at leaſt very near done, the propoſed body is ſtill (if I may ſo ſpeak) in the ſtate of heat, as to ſenſe: which laſt words I add, becauſe, that in reference to other bodies, it may then be notably refrigerated. As lead, that has but heat enough to keep it in fuſion, may, by the pouring on of ſuch water, as to a man's hand, would feel hot, be brought to grow hard, which loſs of fluidity is alſo the natural effect of cold, though perhaps, both the metal, and the liquor, be yet as to ſenſe conſiderably hot.

Eleuth. So that, according to you, none of the kinds of bodies, that are actually known in nature, are adiaphorous as to ſenſe. On which occaſion let me note by the by, that the frequent variations of ſenſe muſt render it but an uncertain ſtandard of heat and cold: and upon ſuppoſition, that there were an adiaphorous body in reference to our ſenſe; yet it would not be ſo in reference to all other bodies, or, in the phraſe of our *Verulam*, ſpeaking of heat, *in ordine ad univerſum.* And for what remains, the controverſy grounded on *Gaſſendus*'s argument ſeems to be rather verbal, than real, and may be determined, or compoſed, by ſettling the diſtinct acceptions of the words cold and heat.

SECTION VII.

Phil. WHEREFORE I wiſh, that we may not waſte the little time, that is left us, upon niceties of no greater concernment; and I think this ſhort time would be better employed, if *Carneades* would be pleaſed to tell us a little more particularly, what he ſuppoſes to be the thing, that withheld Mr. *Boyle* from delivering an opinion about the nature of cold.

Eleuth. Yet, methinks, it is but fair, that *Carneades*, who has all this while been confined to the anſwering another's arguments, ſhould now take his turn to propoſe his own.

Carn. I find, in each of your motions, Gentlemen, ſomething ſo equitable, and ſo expedient, that I ſhal in part comply with both. And that I may haſten to do what *Philoponus* deſires, I ſhall do no more than briefly point at two things, that may be alledged in favour of the hypotheſis I defend. For if you reflect upon what we have already diſcourſed, we may take notice of things there, that will ſcarce be well accounted for by being aſcribed to poſitive cold, but may be far better explained agreeably to our hypotheſis. And muſt add, in the next place, that I, who ſuſtained the perſon of a reſpondent, may pretend to have ſufficiently diſcharged my office, if I have ſhewn the invalidity of all the opponents arguments; and it is his part, who aſſerts a poſitive thing in nature, to make it good, whereas he, that denies it, needs not alledge any other reaſon why he does ſo, than the authority of that juſtly received axiom in philoſophizing, *Entia non ſunt multiplicanda abſque neceſſitate.* And, I hope, there will need no other engine to demoliſh an ill-formed and proofleſs opinion about cold, than an axiom ſo ſolid and efficacious, that in the opinion of almoſt all the modern naturaliſts it has been able to aboliſh ſuch potent and immenſe bodies as the *primum mobile* itſelf, and a ſuperior orb or two, the leaſt of which contained that firmament, in compariſon whereof the whole earth is but a point. And not only ſo, but the ſame axiom has baniſhed the angels and intelligences from the cœleſtial orbs, that *Ariſtotle* and his followers, had aſſigned them to turn about; or rather hath releaſed thoſe noble and happy ſpirits from the drudgery, to which the philoſophers of ſo many ages, had needleſsly doomed them.

Eleuth. I the leſs diſtruſt the validity of the axiom you alledge, becauſe I obſerve it to be the ground, on which is built a great part of the reformation of philoſophy, that is introduced by the moderns. For one of the main things, that firſt moved conſidering men to ſeek for more ſatisfactory opinions, than thoſe of the peripatetick ſchools, was, that theſe obtruded a great many tenets in philoſophy, that were not only unproved, but unneceſſary to the explication of the phænomena of nature; as it were not difficult to ſhew.

BUT I ſee *Philoponus* preparing to renew the motion he lately made, in which the ſhortneſs of time makes me now think it ſeaſonable to join with him, I being no leſs deſirous than he to know, what may be the motives of your friend's declining to declare himſelf fully about the nature and cauſe of cold.

Carn. I have already intimated to you, at the beginning of our conference, that he is himſelf the fitteſt perſon to be addreſſed to for ſatisfying this enquiry. But not to be altogether ſilent on this occaſion, I ſhall tell you, that, as far as I can gueſs, he waits till farther trials and ſpeculations have reſolved him in ſome points, wherein he is not yet ſatisfied: for, being of a temper backward enough to acquieſce without ſufficient evidence, when the enquiry is difficult, and the ſubject important; he ſeems to me to be kept in ſuſpence, both by ſome ſpeculative doubts, and the phænomena of divers experiments, ſome of which are not delivered in his book. It would be now improper to mention the ſcruples and heſitancies they have occaſioned in him; though of thoſe, I have heard him ſpeak of, I ſhall name ſome inſtances, that occur the moſt readily. As I remember I heard him make enquiry, as to thoſe, that would have cold produced by corpuſcles of cold; whether, and on what account, thoſe little fragments of matter are cold? whether thoſe frigorifick particles, that muſt in multitudes crowd into water to turn it into ice, have gravity or levity, or are indifferent to both? And how any of the three anſwers, that may be made to this enquiry, will agree to ſome phænomena, that may be produced? what ſtructure the corpuſcles of cold can be of, that ſhould make them frigorifick to that innumerable variety of bodies they are ſaid to pervade? And, whether the frigorifick faculty of theſe corpuſcles be looſable, or not? As alſo, whether or no they be primitive bodies; and if it be ſaid, they are not, whether there was not cold in the world before they were produced, and whence that cold could proceed? And if it were ſaid, they are primitive bodies, he demanded, how it came to paſs, that by putting a certain factitious body actually warm, into water, that was alſo warm, (both which appeared by a good ſealed weather-glaſs) there ſhould preſently be produced an actual coldneſs (diſcernible by the ſame thermoſcope?) Theſe, and I know not what other ſcruples and difficulties, ſuggeſted to him by his thoughts, or his experiments, were the things, that, I ſuppoſe, prevailed with a man of his temper to forbear for a while the declaring of his ſentiments about cold, leſt the event of ſome farther trial ſhould ſhew him cauſe to retract them.

Phil. What you have freſhly intimated, *Carneades*, of Mr. *Boyle*'s having other heſitations, than thoſe you have named and ſuggeſted by experiments, not publiſhed in his hiſtory, does, I confeſs, the more excite my curioſity to have, at leaſt, a taſte of thoſe preplexing phænomena.

Carn. YOU may eaſily gueſs, *Philoponus*, by what I have told you already, that you are not to exſpect a full ſatisfaction from me on this occaſion. But yet, that your curioſity may not be fruſtrated, I ſhall venture to acquaint you with two phænomena, which were, I ſuppoſe, none of the leaſt motives of his backwardneſs to declare himſelf. But though ſome body perhaps thinks, that the grounds of ſolving theſe phænomena, and moſt of the newly recited ſcruples, may be picked out of ſome things, that may already have paſſed among us in this conference; yet, becauſe we have not now time to enter upon a diſcuſſion of this matter, I am willing you ſhould ſuſpend the debate, till we have occaſion to meet another time; and therefore I ſhall now only acquaint you with a couple of experiments, that he ſet down for a virtuſo, who was to ſolve the two main problems ſuggeſted by them. The firſt whereof was, whence water ſhould, upon congelation, acquire ſo vaſt a force, as he found it had, to lift up great weights, and burſt containing bodies; though it ſeemed by ſeveral circumſtances, that the motion of the water is very much diminiſhed, when it is changed into ice. And the ſecond problem is thus conceived; if, as a briſk agitation of a body's inſenſible parts produces heat, ſo the privation of that motion is, as *Cardan*, and the Carteſians would have it, the cauſe of cold; whence is it, that, if certain bodies be put together, there will be a manifeſt and furious agitation of the ſmall parts, and yet, upon this conflict, the mixture will not grow hot, but ſenſibly and even conſiderably cold? The narratives themſelves, of the experiments, are too long to be now read over to you. And therefore, I ſhall leave the paper, that contains them, among you, to be peruſed at your leiſure, between this and our next meeting, till when I muſt bid you farewell; only deſiring you in the mean while, to remember, that, as I have but acted a part impoſed upon me in our paſt conference, ſo notwithſtanding any thing, that I have ſaid in my aſſumed capacity, I reſerve to myſelf the right of appearing as little pre-engaged, as any of you at our next meeting.

TWO

TWO PROBLEMS ABOUT COLD,

Grounded on NEW EXPERIMENTS,

And proposed in a LETTER to a FRIEND.

To my very Learned Friend Mr. *J. B.*

SIR,

I PRESUME, that you will not be surprized to be told, that I send you the inclosed papers, not only, that I might gratify your curiosity, but, that you may by them be enabled to help me to satisfy my own; and therefore I shall accompany the historical transcripts I made of the following experiments, as I found them registered for my own remembrance, with some of the doubts suggested to me by some of the phænomena, that occurred. But yet I shall not trouble you with all the difficulties, that at first troubled me, but reduce the exercise, I desire to give your sagacity, to the solution of two problems. And I will begin with propounding that first, which is grounded upon the last of the two following papers, because, though the historical part of that be much the longest, yet the grounds of my quære concerning it, will be much more briefly proposed, the experiment
Prob. II. itself naturally suggesting this problem; "How, "upon the mixture of two or three bodies, "such as those mentioned in the paper, there "should manifestly ensue a great and tumultuary agitation of small parts, and yet, even "during this conflict, not any sensible heat, "but a considerable degree of cold, be produced," and that even in the internal parts of the mixture?

THE inducements to make this problem need not be far fetched, it being obvious enough, that, according to the corpuscularian philosophy, which you and I agree in, a brisk, and various agitation of the minute parts of a body is that, which makes it hot, both in reference to our sensories, and to its operations on other bodies. But I doubt, the rise of the problem is much more easy to be understood, than the cause of the phænomenon, about which I will not ask you, whether one may not assert, that local motion is, in its own nature, a generical thing, which may be so diversified by circumstances, that one kind of modification of it, as it is made in corpuscles of several sizes, and shapes, may be the cause of heat, and another, that of cold? or else, whether we may suppose, that cold is a positive thing, and operates by real corpuscles of cold, which happening to abound, and yet to be locked up in the bodies, whose mixture I employed, they are, by the great conflict, that dissolves the texture of the clashing salts, separately put into motion, and that in such numbers, that though really there would be a heat produced by the brisk and confused agitation of some of the parts, yet that heat is not only concealed, and checked, but mastered by the over-powering operation of the frigorifick corpuscles. But to ask you about this, or any other particular way of solving our phænomenon, were to forget, that my aim is to learn not your opinion of this, or that particular conjecture, or fancy, about our problem, but in general, how it may be best resolved, and what you think to be the true cause of so odd an effect.

HAVING thus dispatched the little I had to say about the paper, that suggested the second problem, I will now suppose, that you have read the phænomena, that contain the rise of the first, to which I shall proceed, without farther preamble, since the question, or problem, that these naturally call for, is, "Whence this "vast force of freezing water proceeds?" Prob. I.

FOR the breaking of resisting bodies being to be made by a violent local motion, and cold, according to the judgment even of the moderns, either consisting in, or, at least, being accompanied with a privation, or a great imminution of motion, it seems very difficult to conceive, how cold should make water to exert so wonderful a force. I know the learned *Gassendus*, and divers other philosophers, teach us, that glaciation is performed by the entering of swarms of corpuscles of cold, as they call them, into the liquor. But I much doubt, whether, from this hypothesis, a good solution of our phænomenon will be derived, since these atoms of cold seem not barely, as such, to make that expansion of the water, which is required in the experiment by me recited. For I see, that though water will be more and more refrigerated, according as the air grows colder and colder, yet, till it be brought to an actual glaciation, all the swarms of the frigorifick atoms in it are so far from expanding it, that they more and more condense it. And even that degree of cold, which destroys fluidity, though it expands water, does not do it merely by the multitudes of the frigorifick corpuscles, that invade the pores of the lately fluid body, since pure spirit of wine, and almost all chemical oiles, though exposed to the same degree of cold, that turns water into ice, or, as I have tried, unto a far greater than is necessary to do so, will be but the more condensed by those swarms of particles. But, which is more considerable, I have carefully observed, that, besides common or expressed oils, chemical oil of aniseeds itself, being frozen, or concreted by an intense degree of cold, will not be expanded, but notably condensed, and accordingly grow specifically heavier than before. And this was one thing, that kept me from expecting the removal of our difficulty from the ingenious explication given of freezing by the Cartesians, when they teach, that the eel-like particles, whereof they suppose water to consist, are very remissly agitated, and their want of pliantness makes their contexture less close; which seems not to agree with the lately mentioned trials. And though these eel-like particles should lose all their flexibleness, though, in that case, it may probably be said, that they would take up less room than before, if nothing oppose their expansion, yet it does not thence appear, how they should acquire so vast a power to expand themselves in spite of opposition, as we have shewn water, by freezing, does acquire.

I did not hope to resolve our problem by the help of a vulgar supposition, that well stopped vessels are broken in frosty weather *ob fugam vacui*, since I found that supposition to be erroneous by divers experiments, some of which are mentioned in the history of cold.

IT seemed less improbable, that some assistance to the solving of our difficulty might be given by two other things. Whereof the first is, that, for aught I have yet observed, no liquor but water, or that which participates of water, by having aqueous particles separable from it, will be made to swell by cold; nor will water itself do so upon every degree of cold, but only upon so great an one as actually turns it into ice. And the second is, that upon the glaciation of water, and aqueous liquors, we may observe in the ice many bubbles, greater or smaller, intercepted between the solid parts, and supposed to be full of air, (I say, supposed, because, upon trial, I found them to have yielded but a small proportion of common air;) which supposition, if true, would perhaps invite one to suspect, that the air contained in these bubbles might have an interest in our phænomenon; since I have found, by trials purposely made, that air, congregated into visible, though not great portions, may exercise a considerable elasticity, which appeared not whilst it was invisibly dispersed through the water.

AND if I did not suppose, both that you had taken notice, that there are wont to be numerous particles of springy air dispersed through the pores of water; and that you had considered, whether the want of pliantness, occasioned by cold in the aqueous corpuscles, whilst they are yet agitated and brandished by some permeating matter; and whether, upon the change of the pores, that we may conceive to be made in freezing water, either by the recess of one sort of subtil corpuscles, or the admission of another, or the closer constipation of the grosser parts, there may not be produced in corpuscles, that compose water, to say nothing of the intermixed air, or the concretions, or the coalitions, occasioned by the cold) a springiness capable to make many little bodies, endowed with it, exert a great force against the sides of the vessel, that oppose their joint endeavour to expand themselves: if, I say, I did not believe, that these, and the like suspicions, had occurred to you, as well as to me, together with the difficulties, wherewith each of them seems to be incumbered, I would acquaint you with what thoughts, and trials, occurred to me about these, and the like conceits. But I not daring to think this could prove other than a needless work, I must remember, that my business, in this paper, is to propose difficulties, not the ways of solving them; it being, from your kindness and sagacity, that these are as well expected, as desired, by,

SIR,

Your, &c.

AN

AN ATTEMPT

To Manifest and Measure the

GREAT EXPANSIVE FORCE

OF

FREEZING WATER.

CONSIDERING, when I writ the hiſtory of cold, that though divers phænomena might induce an attentive obſerver to think, that freezing water had an expanſive force, yet I had not met with any, that endeavoured, or even propoſed, to meaſure it, whether, becauſe they reflected not on it at all, or judged not the force conſiderable; I, who looked with other eyes upon it, thought fit to repair that omiſſion, but was then ſo ill furniſhed with requiſites for doing it fully, that, I remember, I complained of it in my hiſtory of cold. And though, even afterwards, when the time of the year was favourable, I could not procure ſuch accommodations, as my deſign exacted; yet, thinking an imperfect way of meaſuring to be better than none, I preferred, to the making no attempt at all, the endeavouring to do what the leaſt defective inſtruments, I could procure, would permit me; towards the making an eſtimate by known meaſures, of the expanſive power of freezing water. For though I did not expect I ſhould be able accurately to define it, yet I hoped I ſhould make ſuch an eſtimate, as to know, that force not to be, as one would think it, faint and contemptible, but very great and conſiderable.

I remember on this occaſion, that to manifeſt the force of freezing water, I cauſed the barrel of a ſhort gun to have a ſcrew fitted to the noſe of it, by which we might exactly ſtop it, as we did the touch-hole another way; then filling the barrel with common water, and cloſing it accurately by the help of the ſcrew, we laid it in a conveniently-ſhaped veſſel, wherein we encompaſſed it with a frigorifick mixture (of ſnow, or ice, and ſalt,) and, in a ſhort time, we found, as we expected, the barrel to be burſt, part of the ice appearing along the gaping ſlit, that had been made in the body of the iron by the freezing water, which, by this effect, ſeemed to emulate the juſtly-admired force of kindled gun-powder. But the deſign of this ſhort paper tending not ſo much to prove, as (in ſome ſort) to meaſure the expanſive force of water, I ſhall ſubjoin the tranſcripts of two or three experiments, made chiefly for that purpoſe.

EXPERIMENT I.

[THERE was taken a ſtrong cylinder of braſs, whoſe cavity was two inches in diameter; into this was put a bladder of a convenient ſize, with a quantity of water in it, that the neck of the bladder (which I had taken care to have oiled) being ſtrongly tied, the water might not get out into the cavity of the cylinder, nor be capable of expanding itſelf ſome other way, than upwards. Then into this cylinder was fitted a plug of wood, turned on purpoſe, which was ſomewhat leſs in diameter than the cylindrical cavity, that it might riſe and fall eaſily in it. Upon the upper part of this plug was laid a conveniently ſhaped flat body, upon which were placed divers weights to depreſs the plug, and hinder its being lifted up by the expanſion wont to be made in water, that is made to freeze: then a frigorifick mixture being afterwards applied to the cylinder, it appeared, within half an hour, or ſomewhat more, by a circle, that had been purpoſely traced on the ſide of the plug, where it was almoſt contiguous to the orifice of the cylinder, that the water in the bladder began to expand itſelf, and about two hours after, having occaſion to ſhew the experiment to ſome inquiſitive perſons, the circle appeared to have been heaved up, in my eſtimate, about $\frac{3}{8}$, if not half, of an inch, notwithſtanding all the weights, that endeavoured to hinder the aſcenſion, though theſe weights amounted to 115 pound, which were all the determinate weights we could then procure, beſides a brick, and ſome other things, that were eſtimated at five pound more; nor did I doubt, that a far greater load would not have hindred its expanſion.]

EXPERIMENT II.

[WE took a braſs cylinder, whoſe dimenſions were three inches eight tenths in diameter, and in depth four inches. Into this we put a fine bladder of a convenient ſize, almoſt filled with water, and ſtrongly tied about the neck; upon this bladder we put the wooden

wooden plug to ſtop up the orifice, as much as was convenient, and upon the plug we put a piece of a flat board for the weights to ſtand upon. Theſe things being prepared, we conveyed the cylinder, with all, that belonged to it, ſave the board, into a large wooden bowl, where we applied to the cylinder a good quantity of the frigorifick mixture, made with beaten ice and bay ſalt; and having firſt marked with a circular line the edge or contact, where the orifice, or lip of the cylinder, touched the plug, we laid on the weights upon the board; and when by their weight they had depreſſed the plug till the cover of it leaned upon the cylinder, we diſpoſed ourſelves to attend the iſſue of the trial. The event whereof was this, that when the action of the frigorifick mixture had produced ſome ice in the water included in the bladder, that liquor appeared to have dilated itſelf ſtrongly enough to begin to raiſe the plug with the ſuper-incumbent weights, and by degrees they were, by the growing ice, raiſed, till the mark, diligently made on the plug, where the edge of the cylinder touched it, was about a tenth part of an inch above the ſtation it had before the plug had been depreſſed. Then we took out the bladder, and found the cylinder of water within the bladder not to be wholly turned into ice, but to contain ſome quantity of unfrozen water in the parts about the centre, which liquor, if we had not ſo ſoon deſiſted from the experiment, (as for certain reaſons we did) might probably have raiſed the weights ſomewhat higher. But as it was, the ice in length was but three inches and about $\frac{1}{8}$. and yet ſo ſmall a quantity of ice ſufficed to raiſe, beſides the board they leaned on, as many weights of lead, as amounted to an hundred pound averdupois.]

EXPERIMENT III.

[THE day after the above-mentioned experiment was made, to try yet farther the expanſive force of freezing water, the ſame was reiterated after the manner above delivered, but with this difference, that having procured more weight, when the plug was lifted up $\frac{3}{8}$, or ſomewhat better, (which plug began ſenſibly to riſe within half, or three quarters of an hour, after the frigorifick mixture was applied) it was loaded with a weight of two hundred pounds, and a fifteen pound piece of lead, and other bodies, as boards, &c. to lay the weights upon, which being alſo weighed by themſelves, came to fifteen pound more, ſo that the whole amounted to two hundred and thirty pound; and if the hundred pounds were both of them, as their bulk and ſhape invited us to gueſs, of that ſort of weights, which are called the great hundred, containing an hundred and twelve pound a-piece, twenty-four pound muſt be added to the ſum, which would thereby be made up two hundred and fifty-four pound.]

A

NEW EXPERIMENT

ABOUT THE

PRODUCTION of COLD

By the CONFLICT of BODIES, appearing to make an EBULLITION.

AND now, that we are ſearching after the nature of cold, I am put in mind, that I have ſometimes wondered at a certain experiment, that is ſo anomalous, and ſeems ſo little of kin to the uſual phænomena of cold, that though I do not particularly teach the way of making it, becauſe I could not do it without diſcovering ſomething in chemiſtry, that cogent conſiderations forbid me at preſent to publiſh; yet I cannot forbear to relate, on this occaſion, the matter of fact, both becauſe it may afford conſiderable hints to ſagacious enquirers, and becauſe it ſeems ſo little congruous to moſt theories of the cauſes of cold, that it may make the framers of theories more wary, and help alſo to excuſe my backwardneſs to propoſe hypotheſes about cold in a reſolute and confident way.

THE experiment is this: We took three ſaline bodies, each of them purified by the fire; and whereas there are divers bodies, that being mingled together acquired a heat, which neither of them had apart; and whereas it is ſaid by ſome, that there are a few, which being blended

blended together, make a mixture somewhat colder than either of themselves, these salts of ours being put together in due proportion, do upon their mixture produce that, which the eye judges to be a great effervescence; but though the hissing noise be loud, and though the numerous bubbles suddenly generated will make the matter apt to overflow the glass, if the one be not capacious, and the other be not put in by little and little; yet even whilst this seeming ebullition lasts, the glass, which one would expect to find very hot, (as usually happens upon the mixture of the salt of tartar, and spirit of nitre, and upon the confusion of the like saline bodies disposed to produce together such efflorescencies) instead of growing hot, does, if it be held in one's hand, feel much cooler than before, and that in a wonderful degree; insomuch, that even in winter the outside of the glass would quickly be covered with great drops of dew, which after a while would unite, and trickle down by their own weight. And this we could make to last for a great while, by casting in by degrees more and more of one of the ingredients on the other. And besides that, this copious dew on the outside of the glass, reached as high as the mixture within, which argued whence it proceeded; besides that, purposely looking on the bottom of the glass, whose outside was concave, we found no such drops of dew there, because the vapours of the external air could not, in any quantity, have access to it; which shewed the dew, conspicuous elsewhere, not to come from the transudation of the finer parts of the mixture through the pores of the glass: besides these things, I say, I remember, that having sometimes purposely wiped off the dew here and there with my handkerchief, the dry parts of the glass would in no long time regain fresh drops of dew. And this odd experiment we did for the main repeat, not only in the presence of an industrious chemist, (whose trials unexpectedly gave us the rise of the experiment,) but also alone, and at differing seasons of the year.

I shall add, that having afterwards, about the middle of *November*, thought fit to vary a little, and repeat the experiment, because I could then make use of a sealed weather-glass, which I had not at hand when I made the former trials; I took two deep glasses, into the one of which I put a good quantity of fair water, and in the other I made such a mixture, as I was lately mentioning; and having by a string, (to prevent the altering of the temper of the included air by the warmth of my fingers) let down the weather-glass into the water, that the liquor shut up in the instrument might be cooled by the ambient water; after it had staid there a reasonable time, I took it out, by the string, that was fastened to the upper part of it, and letting it down into the mixture, that was then hissing, and filling the vessel, that contained it with multitudes of successively emerging and hastily vanishing bubbles; I perceived nevertheless, that the coldness of the seemingly effervescent mixture made the imprisoned tincted liquor to subside so low, that from four inches and three quarters (or thereabout) at which height it stood in the carefully divided stem, when the weather-glass was taken out of the water, it fell in a short time lower than to one inch and an half. And because I foresaw, that this might seem scarce credible, especially if I should relate, how swiftly the imprisoned liquor subsided at the beginning; I shall annex, that, for farther satisfaction of others, I removed the thermometer out of the mixture into the common water again, where it soon reached to somewhat above four inches and a half; and not content with that, I put it a second time into some of the frigefactive mixture before it had done foaming, in which it fell, as before, somewhat below an inch and a half, and, presently after, almost as low as to an inch. And having once more put it back into the glass, that contained the water, the included liquor re-ascended to above four inches and a half, and this in an excellent sealed weather-glass, whose stem was not in all above ten inches long, with a ball proportionably big. And for farther confirmation, I took notice, that, whilst the mixture, by its hissing noise, and its strangely numerous bubbles, seemed to be in a state of ebullition, the outsides of the glass, that contained it, were, as far as the mixture reached, so plentifully bedewed with the condensed vapours of the ambient air, that their weight carried them down in little streams, which left round about the bottom of the vessel a pretty quantity of liquor, that appeared by its taste not to have been made by the transudation of any of the sharp and saline liquors, that were agitated within the glass. There remained only one scruple, which was suggested to me by the remembering of a circumstance, which however, at the making of the fore-mentioned trials, I had not minded, and which possibly most observers would have neglected; but calling to mind, that the water, I had made use of to immerse the weather-glass in, was brought out of a room, wherein a fire was wont (though not constantly) to be kept, whereas the ingredients of the mixture were kept, and put together in a chamber, which, though contiguous to the former, had no chimney in it; I thought fit, for greater circumspection sake, to let the water stand all night in this last-mentioned chamber, that the ambient air might have the same operation upon it, as upon those bodies, that were to be ingredients of the mixture: and then repeating the formerly recited experiment, though I thought it needless to spend time to watch, as before I had done, the greatest difference in cold betwixt the water and the bubbling mixture; yet by making removes of the weather-glass to and fro, from one liquor to another, it sufficiently appeared, that the greater coldness, remarkable in the mixture, did not before proceed in any considerable degree (if in any degree at all) from the water's not having been kept in the same room with it.

So that by these different trials it seems manifest, that the coldness of the mixture was not a deception of the ſenſory, ſince it would be diſcovered by the operation it had, not only upon the vapours of the air on the outſide of the glaſs, but upon the thermometer itſelf, placed in the midſt of the mixture, which this laſt named circumſtance argues to have been cold throughout, and even in its innermoſt parts.

AND to ſhew, how much this ſtrange coldneſs depended upon the peculiar texture of the mixture, or the ſtructure of its component corpuſcles, and the peculiar kind of motion, that was excited in the tumultuating particles; I ſhall here ſubjoin a relation, which probably will not appear deſpicable; namely, that in the firſt place I took ſome of the acid liquor, the reſt of which I had made uſe of to make the mixture, whereof I have been ſpeaking; and put a convenient quantity of fair water, which had been kept a night or two, in the ſame room (wherein was no chimney) with it, that there might be no cauſe of ſuſpicion, that the one had been expoſed to a more or leſs cold air than the other; and yet theſe two liquors did ſcarce ſenſibly differ in coldneſs; though to diſcover whether they did or no, I removed from one to another of them a good ſealed weather-glaſs, with a very ſlender ſtem.

AND in the next place, I took a convenient quantity of the pure ſalt, I had ſo often employed, and caſt it into a glaſs full of water, which I had kept many hours in the ſame room with it, and wherein I had a little before placed a ſealed weather-glaſs, that the included liquor might be brought to the temper of the ambient liquor; but upon this injection, the tincted liquor of the thermoſcope ſubſided ſo little, as not to make me look upon this ſalt as being itſelf extraordinarily cold, ſince other obvious ſalts (that I have at other times caſt into water to cool it a little) and even ſea-ſalt would (according to my eſtimate) have refrigerated it as much, if not more. Nor did I obſerve the glaſs, wherein I was wont to keep ſtore of our ſalt, (though I had often occaſion to handle it) diſcloſe to the touch any remarkable degree of coldneſs; ſo that the coldneſs of our hiſſing mixture could not be attributed to that of either of the ingredients apart, but was a quality emerging upon their being blended. Now, when I thus made theſe preparatory trials, having afterwards placed in the ſame window (of the chamber laſt mentioned) a couple of glaſſes, with common water in one, and in the other ſome of that mixture, of whoſe frigefactive power I had very recently made trial; I left them to ſtand there together all night, and left alſo ſtanding by them ſuch a ſealed weather-glaſs, as I have been mentioning; and the next morning, when all the viſible commotion or agitation of the minute parts of the contrary ſalts of the mixture was quieted, I put the weather-glaſs, firſt, into one of thoſe two liquors, and then into the other, and after removed it back into the former again, without perceiving any difference worth minding, betwixt the coldneſs of the mixture, and that of common water: and with much the like ſucceſs I repeated the trial, after the water, and the other liquor had ſtood in the ſame room (unfurniſhed with a chimney) for near two days and nights.

AND for farther confirmation, I ſhall add, that having inſtead of the ſalt, which I hitherto made uſe of, taken ſome of the ſpirit, that was wont to come over together with that ſalt, and did ſo abound with it, that a good deal of it lay undiſſolved at the bottom of the liquor; having, I ſay, employed this ſaline ſpirit, inſtead of the ſalt itſelf, and having for trial's ſake mixed with it another ſpirit, drawn in my own laboratory for the purpoſe, which to me ſeemed as like, as could be made, to that, which I had all this while made uſe of; I found, that the mixture of theſe two liquors, though it produced far fewer bubbles than I was wont to have, inſtead of growing cold, grew lukewarm, and quickly impelled the liquor in the weather-glaſs, from a little above three inches, to as much above eight; and yet, beſides that this laſt ſpirit was, as far as I could perceive, and that after the ſame manner, drawn from the ſame materials with that I had uſed all this while; the ſmell and taſte, (which are both of them peculiar and odd enough) concurred to manifeſt the two ſpirits to be of the ſame kind.

AND, for farther proof, I ſhall add, that to ſatisfy myſelf the more fully, I took a parcel of the ſame liquor, I had lately employed with ſucceſs in making the frigorifick mixture; and yet even this liquor, which with the dry ſalt would queſtionleſs have produced a frigefactive mixture, as well as the reſt had done, which I had a little before taken out of the ſame viol; this liquor, I ſay, put to a new portion of the ſaline ſpirit above-mentioned, though they did not produce minute bubbles numerous enough to make a foam; yet the mixture, inſtead of growing very cold, grew manifeſtly lukewarm, not only in the judgment of the touch, but by its operation on a good ſealed weather-glaſs, carefully, and for a competent while employed to examine the temper of it. Whereas on the contrary, having purpoſely kept ſome of the frigorifick ſpirit by the fire-ſide, till its temper was ſo altered, that it nimbly enough rarified and impelled up the ſpirit of wine contained in a ſealed weather-glaſs, immerſed in it, and having into this liquor caſt ſome of the frigorifick ſalt, even whilſt the ſpirit of wine was riſing, and would probably have riſen a pretty while longer; this injected ſalt, when it began to be diſſolved, did not only give a check to the riſing liquor, and quickly put a ſtop to its aſcent; but, as I expected, ſoon made it ſubſide again, till it fell about three inches or more (which was very much in a ſhort weather-glaſs) beneath the ſtation where the ſpirit of wine had reſted, before the liquor was ſet by the fire ſide; nay, afterwards, I tried, that a frigorifick ſalt, being well warmed by the fire ſide, did, with an appropriated liquor, that was alſo warmed, produce a coldneſs manifeſtly

nifestly perceivable by the weather-glass. So that in these cases a body but moderately cold, nay, actually warm, hastily reduced one, actually warm, or at least tepid, to a far greater degree of actual coldness than itself had.

THESE are some of the experiments I tried with the liquors and salts, of which, upon allowable considerations, I must now forbear to set down the way of preparing: but, that even at present I may not be altogether wanting to the curious, I devised a way of making a succedaneum to this experiment, which I shall here willingly annex, as that, which though it be much inferior to what I may one day be at liberty to acquaint the reader with; yet it will shew the main thing intended, by manifesting, that cold may, by the mingling of bodies, be produced, or increased to a degree exceeding that of either of the bodies, that composed the mixture; and this, though at the same time a seeming effervescence be made by the bodies, that thus refrigerate each other.

I took then very good salt of tartar, and putting to it a convenient quantity of spirit of vinegar, I did, whilst the mixture was hissing, (but seemed to the touch to have refrigerated the glass, that contained it,) immerse into it the ball of a good sealed thermoscope, furnished with spirit of wine. And, though the weather-glass were not much above a foot long, yet the coldness of this mixture made the tincted liquor descend, hastily enough, two inches and almost a half. And to shew farther, that this mixture was actually colder than cold water, removing the weather-glass out of the mixture into that liquor, the tincted spirit began to reascend, and that so nimbly, that in about three minutes (that the ball of the thermoscope stayed under water) the spirit of wine had reascended about an inch and a half, if not more. And to try whether this coldness of the mixture did proceed from, or depend upon, some texture of the parts, that was not very permanent, and yet did not quite degenerate, immediately after the ingredients had ceased to work upon one another; I remember, that near an hour after the ebullition of the spirit and salt of tartar was over, the thermoscope being removed out of the common water, where it had stood immersed, into the mixture, descended about half an inch or more. For want of salt of tartar I could not begin the experiment anew, and so am not sure it will always succeed uniformly*. But yet to give my self what further satisfaction I could, by trying the same experiment in such a way, as might discover, whether or no the phænomenon did not depend upon, or require some peculiar texture in the fixed salt, that had been employed; I took some alcali (made by dissolving pot-ashes in fair water, and reducing them by coagulation to a white salt,) and pouring spirit of vinegar to it, I found, that this mixture did not, whilst it hissed, grow at all colder, but rather somewhat warmer. And, for farther satisfaction, immersing into it the ball of the newly mentioned weather-glass, I found, that it ascended in a short time about an inch, and, being removed into the water, descended about half an inch; and by making removes of it from one of these liquors into the other, two or three times more, I found, that the spirit of wine did rise and fall, according to what has been newly observed, but its motions upwards and downwards were both less than before, and more slow.

* The Author's wariness was not here amiss, he having afterwards found, that this experiment did not always succeed.

OBSERVATIONS AND EXPERIMENTS ABOUT THE SALTNESS OF THE SEA.

The FIRST SECTION.

CHAP. I.

THE cause of the ſaltneſs of the ſea appears, by *Ariſtotle's* writings, to have buſied the curioſity of naturaliſts before his time; ſince which, his authority, perhaps, much more than his reaſons, did, for divers ages, make the ſchools, and the generality of naturaliſts, of his opinion, till towards the end of the laſt century, and the beginning of ours, ſome learned men took the boldneſs to queſtion the common opinion; ſince when the controverſy has been kept on foot, and, for aught I know, will be ſo, as long as it is argued on both ſides but by dialectical arguments, which may be probable on both ſides, but are not convincing on either. Wherefore, I ſhall here briefly deliver ſome particulars about the ſaltneſs of the ſea, obtained by my own trials, where I was able; and where I was not, by the beſt relations I could procure, eſpecially from navigators.

FIRST then, whereas the Peripateticks do, after their maſter *Ariſtotle*, derive the ſaltneſs of the ſea from the aduſtion of the water by the ſun-beams, it has not been found, that I know of, that where no ſalt, or ſaline body, has been diſſolved in, or extracted by water expoſed to the ſun or other heat, there has been any ſuch ſaltneſs produced in it, as to juſtify the *Ariſtotelian* opinion. This may be gathered, as to the operation of the ſun, from the many lakes and ponds of freſh water to be met with, even in hot countries, where they lie expoſed to the action of the ſun. And as for other heats, having out of curioſity diſtilled off common water in large glaſs bodies and heads, till all the liquor was abſtracted, without finding, at the bottom, the two or three thouſandth part, by my gueſs, of ſalt, among a little white earthy ſubſtance, that uſually remained. And though I had found a leſs inconſiderable quantity of ſalt, which, I doubt not, may be met with in ſome waters, I ſhould not have been apt to conclude it to have been generated out of the water, by the action of the fire, becauſe, I have, by ſeveral trials purpoſely made, and elſewhere mentioned, found, that in many places, (and I doubt not, but if I had farther tried, I ſhould have found the ſame in more) common water, before ever it be expoſed to the heat of the ſun or other fire, has in it an eaſily diſcoverable ſaltneſs of the nature of common ſalt, or ſea-ſalt; which two I am not here ſollicitous to diſtinguiſh, becauſe of the affinity of their natures, and that in moſt places the ſalt, eaten at table, is but ſea-ſalt freed from its earthy and other heterogeneities, the abſence of which makes it more white than ſea-ſalt is wont to be with us. Theſe laſt words I add, becauſe credible navigators have informed me, that in ſome countries, ſea-ſalt, without any preparation, coagulates very white; of which ſalt I have had, (from divers parts) and uſed ſome parcels.

BUT ſome of the champions of *Ariſtotle's* opinion are ſo bold, as to alledge experience for it, vouching the teſtimony of *Scaliger* to prove, that the ſea taſtes ſalter at the top, than at the bottom, where the water is affirmed to be freſh. But as for the authority of *Scaliger*, though I take him to be an acute writer, yet I confeſs, that, for reaſons elſewhere given, I do not allow it that veneration, which I find given it by very learned men; nor am I over prone, even as to matters of fact, to acquieſce in what he tells us, when he neither ſignifies, that he delivers things upon his own experience, or declares from what credible information from others he received them.

IT is true, that having often obſerved, that ſea-ſalt diſſolved in water is, upon the receſs of the ſuperfluous liquor, wont to begin its concretion, not as moſt other ſalts do, at either the lateral or lower parts of the veſſel, but at the top of the water, I will not think it impoſſible, that ſometimes in very hot climates, or weather, the ſea may taſte more ſalt at the top, than at ſome diſtance beneath it. But conſidering, how great a proportion of the ſalt common water is wont to be impregnated with, before it suffers saline concretions to begin, and how far short of that proportion the salt contained in the sea-water is wont to be, in so much, that about *Holland*, a Dutch geographer or two have not found it to amount to the proportion of one to forty; and I in *England* found it to be no more than I shall hereafter specify; it seems not unlikely, that *Scaliger*'s * observation was well made, and it must be very unlikely, that it should generally hold, if the saltness of the superficial parts of the sea be compared with that of the lower parts of it.

AND yet I do not build my opinion wholly upon this argument of some modern philosophers, that salt being a heavier body than water, must necessarily communicate most saltness to the lowest parts.

FOR though this argument be a probable one, yet water being a fluid body, the restless agitation of whose corpuscles makes them, and the corpuscles they carry with them, perpetually shift places, whereby the same parts come to be sometimes at the top, and sometimes at the bottom; this consideration, together with what was lately noted of the peculiar disposition of dissolved sea-salt, to begin its coagulation upon the surface of the water, may make the argument, we are considering, suspected not to be so cogent, as at first sight, one may think it. Which suspicion I might somewhat countenance by subjoining, that in divers metals, and other tincted solutions, I have not usually observed the upper part of the liquor, to be manifestly deeper coloured than the lower; though, between metalline bodies, and their menstruums, the disproportion of specifick gravity, does usually much exceed that, which I have met with, between sea-salt and common water.

CHAP. II.

IT is urged out of *Linschotten* by a learned modern writer, that wanting fresh water near *Goa* (the Metropolis of the Portugals in the *East-Indies*) they make their slaves fetch it, by diving, from the bottom of the sea; which seems a clear evincement of the peripatetick opinion. But in this observation, I cannot acquiesce, for two reasons: the one, because, that though what is alledged, as matter of fact, were strictly true, yet so general a conclusion could not be safely drawn from that particular instance, since in other parts of the sea, the contrary has been found by experience, as I shall shew ere long. And other reasons than those given by the Peripateticks may be rendered of what happens at *Goa*, which reasons may extend to the like cases, if elsewhere they shall happen to be met with. For it may very well be, that springs of fresh water may arise in some parts of the surface of the earth, that are covered with the sea, as they do in innumerable vallies and other places of the terrestrial surface, that is not so covered, Not to mention those springs, that appear in divers places upon a low ebb, covered with the sea during the flood. The curious Hungarian † governor, that gives us an account of the wonderful waters, that ennoble his country, relates, that in the river *Vagus*, that runs by the fortress *Galgotium*, the veins of hot water spring up in the bottom of the river itself. *Neque in ripa tantùm*, says he, *eruuntur calidæ, sed etiam intra amnem, si fundum ejus pedibus suffodias; calent autem immodicè*, &c. Nay, I have been assured by more than our learned eye-witness, that there is a place upon the Neapolitan coast, where they (and, I think, a writer or two of those parts) observed the water to spring up hot beneath the surface of the sea, in so much, that one of my relators thrusting in his hand and arm somewhat deeper than was convenient, found there an offensive degree of heat. Pag. 65.

BESIDES, (which is my second conjecture) as to the particular case of *Goa*, I had the curiosity to enquire of a great traveller, and a man of letters, that lived in that city and the neighbouring places, and gave me a pertinent account of them, especially of that place, whence the fresh water is fetched by the divers, which his curiosity led him to visit, and take special notice of; but I found by him, that the divers do not now think it needful to fetch their fresh water so low as from the bottom of the sea, and that, by the little depth, whence his and other men's curiosity caused it to be taken up, he judged, it did not so much come from any fresh water springs, rising at the bottom of the sea, as from a small river (whose name I do not remember) that nor far from thence runs into the sea, with such a juncture of circumstances, that at the mentioned places, the fresh water does yet keep itself tolerably distinct, and is not yet so far made brackish, as not to continue potable, though not very good. Which conjecture of his I could make probable, by what I have had from eminent and observing men among our own navigators, touching the sliding of waters one over another, in some parts of the sea, especially near the mouths of rivers. But the discussion of this matter, and the particulars of the account given me of the situation of the place, where water is dived for near *Goa*, would require more words, than they would in this place deserve, unless the point under debate were more important to our present purpose.

I might here pretend to a clear demonstration, by experience, of the contrary of what *Scaliger* delivers, by vouching the testimony of the learned *Patricius*, who affirms, that being upon the sea, which takes its denomination from the island of *Crete* (now *Candia*,) he did, in the company of a Venetian magistrate, *Mocinigo*, let down a vessel (furnished with a weight to sink it) to the bottom of the sea, where, by the help of a contrivance, it was unstopped, and filled with water there, which being drawn up, was found to be not fresh, but salt. This experiment, I say, I could oppose as a demonstration against *Scaliger*; but though it be a very probable argument, and more considerable than any I have seen

* See the third Section, towards the latter end.

† De admirandis Hungariæ Aquis.

ſeen brought by the Peripateticks for their opinion, yet I confeſs, it would be more ſatiſfactory to me, if it would not permit me to ſuſpect, that in the drawing up of the veſſel through the ſalt water, though there had been freſh water taken in at the bottom, the taſte may have been altered by the ſubingreſſion of ſalt water, which being bulk for bulk heavier than freſh, would by its ponderouſneſs endeavour to ſink into the aſcending veſſel, and thereby more eaſily expel part of the freſh water, and mingle with the reſt. Wherefore, I ſhall confirm the ſaltneſs of the ſea at the bottom by ſome obſervations, that are not liable to the ſame objections as that of *Patricius*.

THE firſt is that of the perſon, whom I elſewhere mention, to be able, by help of an engine, to ſtay a conſiderable time at the bottom of the ſea; for of him I learned, among other things, that I deſired to be informed of touching that place, that he found the water to have as ſalt a taſte there, as at the top.

THE next obſervation I obtained by means of a great traveller into the *Eaſt* and *Weſt-Indies*, who having had the curioſity to viſit the famous pearl-fiſhing at *Manar*, near the great *Cape* of *Comeri*, anſwered me, that he had the ſame curioſity, that I expreſſed to learn of the divers, whether they found the water ſalt at the bottom of the ſea, whence they fetch their pearl-fiſhes? and that he was aſſured by them, that it was ſo: and the ſame perſon being aſked by me about the ſaltneſs of the ſea in a certain place under the Torrid Zone, which the relation of a traveller inclined me to think to abound extraordinarily with ſalt, affirmed to me, that not only the divers aſſured him, that the ſea was there exceeding ſalt at the bottom, but brought up ſeveral hard lumps of ſalt from thence, whereof the fiſhermen, and others, were wont to make uſe to ſeaſon their meat, as he himſelf alſo did; which yet I may aſcribe not only to the plenty of ſalt already diſſolved in the water, but to the greater indiſpoſition, that ſome ſorts of ſalts, whereof this may be one, have to be diſſolved in that liquor.

To theſe I ſhall add this third obſervation: meeting with an inquiſitive engineer, that had frequented the ſea, and had ſeveral opportunities to make obſervations of other kinds in deep waters, I deſired him, that he would take along with him a certain copper veſſel of mine, furniſhed with two valves opening upwards, and let it down for me the next time he went to ſea; on which occaſion he told me, that, if I pleaſed, I might ſave myſelf the trouble of the intended trial, for, with a tin veſſel, very little differing from that I deſcribed unto him, he had had the curioſity, near the ſtraight of *Gibraltar*'s mouth, (where he had occaſion to ſtay a good while) to fetch up ſea-water from the depth of about forty fathom, and found it to be as ſalt in taſte as the water near the ſurface.

THESE obſervations may ſuffice to ſhew, that the ſea is ſalt at the bottom in thoſe places where they were made; but yet I thought it was not fit for me to acquieſce in them, but rather endeavour to ſatisfy myſelf, by the beſt trial I could procure to be made with my copper veſſel, (as more ſtrong and fit than a tin one,) what ſaltneſs is to be found in the water at the bottom of our ſeas, not only, becauſe it may more concern us to know that, but chiefly, becauſe, though I deny not, that in the fore-going obſervations the taſte may ſufficiently prove, that the ſea is ſalt at the bottom as well as the top, yet I thought the taſte, by reaſon of the prediſpoſitions, and other unheeded affections, it is liable unto, no certain way to judge, whether the top and the bottom be as ſalt one as the other. Wherefore, I thought it would be more ſatisfactory to examine the ſea-water by weight, than by taſte; and in order thereunto, having delivered the above mentioned inſtrument to the engineer I lately ſpake of, when he was going to ſea, he ſent me, together with it, a couple of bottles of ſea-water, taken up, the one at the top, and the other at the bottom, at fifteen fathoms deep. The colour, and ſmell of theſe two waters were ſomewhat differing; but when I examined them hydroſtatically, by weighing a roll of brimſtone firſt in one, and then in the other, I ſcarce found any ſenſible difference at all in their ſpecifick gravities. So that if the degree of the ſaltneſs of ſea-water may be ſafely determined by its greater or leſſer weight, then ſo far forth as this ſingle experiment informed me, the ſaltneſs is equal at the top and bottom of the ſea: I ſaid, if the degree, &c. becauſe of what I ſhall hereafter take notice of about ſalts of leſs ſpecifick gravity than ſea-ſalt.

CHAP. III.

IT follows now, that I make out, what I formerly intimated, that though it were granted, that near *Goa*, and perhaps in ſome other places, the divers may have found the water freſh at the bottom of the ſea, it would not therefore neceſſarily follow, that the ſea-water, generally ſpeaking, is freſh at the bottom; for the obſervations lately mentioned ſufficiently manifeſt the contrary: and as to thoſe very few places (if really there have been any) where the ſea-water has been found freſh at the very bottom, I think one may aſcribe the taſte of the water to the bubbling up of ſprings of freſh water, at, or near enough to thoſe very places. I know this may appear a paradox, ſince it may ſeem altogether unlikely, that ſo ſmall a ſtream of water, as can be afforded by a ſpring, ſhould be able to force its way up in ſpite of the reſiſtance of ſo vaſt a weight, as that of the ſuper-incumbent ſea-water, eſpecially ſince this liquor, by reaſon of its ſaltneſs, is heavier in ſpecie than freſh water.

BUT this objection needs not oblige me to forſake my conjecture; for whatever moſt men believe, and even learned men have taught, to the contrary, it matters not how great the quantity of liquor be, which is laterally higher than the lower orifice of the pipe, or channel, that gives paſſage to the liquor, that is to be

impelled

impelled up into it; provided the upper surface of the liquor in the channel, or pipe, have a sufficient perpendicular height in reference to that of the stagnant water; for no more of all this fluid will hinder its ascent, than the weight of such a pillar of the said fluid, as is directly super-incumbent on it. * *Stevinus*, and I, have, by differing ways, particularly proved, that, according to the laws of the true hydrostaticks, the prevalency of the two liquors, that press against each other, is not to be determined according to the quantity of them, but to be adjudged to that, which exceeds the other in perpendicular height; so that, considering the channel, wherein a spring runs into the sea, as a long and inverted syphon, if that part of the either neighbouring, or more distant shore, whence the spring, or river, takes its course, be a neighbouring hill, or rock, or any other place considerably higher, than that part of the bottom of the sea, or of the shore covered with the surface of the sea, at which the channel, which conveys fresh water, terminates, that liquor will issue out in spite of the resistance of the ocean.

To illustrate at once, and prove this paradox, I thought upon the following experiment. I took a vessel of a convenient depth, and a syphon of a proportionable length, both of them of glass, that their transparency might permit us to see all that passed within them. Into the larger vessel we put a quantity of sea-water, and into the longer leg of the syphon, which had been for that purpose inverted, we poured a convenient quantity of fresh water, which we kept from running out at the shorter leg, by stopping the orifice of the longer with the thumb or finger: then this syphon being so placed in the greater vessel, that the orifice of the shorter leg was a great deal beneath the surface of the salt water, and the superficies of the fresh water in the longer leg was a pretty deal higher than that of the surrounding salt water, we unstopped the orifice of the upper leg, whereby the water in the syphon, tending to reduce itself to an æquilibrium, or equality of height, in both legs, the water in the upper leg being much higher, and heavier, than that in the other, did, by subsiding, drive away the water in the shorter leg, and make it spring out at the orifice of the shorter leg, in spite of the breadth, and specifick gravity of the salt water. And this impelling upwards of the fresh water lasted as long, as the surface of that water, in the longer leg, retained its due height above that of the surrounding sea-water; which circumstance I expressly mention, because there being a difference amounting to between a fortieth and fiftieth part, betwixt the specifick gravity of our sea water and common fresh water, by reason of the salt, which makes the former the heavier, the fresh water, in the longer leg of the syphon, ought to be between a fortieth and fiftieth part higher than the surface of the sea water, to maintain the æquilibrium betwixt these two liquors.

To make the fore-mentioned experiment the more visible, I thought fit to perform it with fresh water tinged with brasil, or logwood; but that it might not be objected, that thereby the specifick gravity of the liquor would be altered, or increased, I afterwards chose to make it with claret-wine, which being a liquor lighter than common water, and of a conspicuous colour, is very convenient for our purpose.

And when I made this trial, by placing the orifice of the shorter leg at a convenient distance below the surface of the sea-water, it was not unpleasant to observe, how, upon the removal of the finger, that stopped the orifice of the longer leg, the quick descent of the wine contained in that leg impelled the coloured liquor in the shorter leg, and made it spring up, at its orifice, into the incumbent sea-water, in the form of little red clouds, and sometimes of very slender streams. And as this shorter leg of the syphon was raised more and more towards the surface of the water, so there issued out more and more wine at the orifice of it; the liquor in the longer leg proportionably subsiding, but yet continuing manifestly higher than the surface of the salt water, than which it was in specie much lighter.

¶ But here I must give an advertisement to prevent a mistake; for if the syphon be not exceeding slender, after the wine in the longer leg is fallen down to its due station, a heedful observer may perceive, after a while, that though the syphon be kept in the same place, there will issue out of the shorter leg a little red stream, which proceeds not from the former impulse of the wine in the longer leg, but from the ingress of the sea-water, which being much heavier in specie than wine, sinks into the cavity of the syphon, and as it comes in on one side, thrusts up as much wine on the other side of the same cavity. But the red liquor, that ascends on this account, may be discerned to do so, by its rising more slowly, and after another manner, than that, which is impelled up by the sudden fall of the tall cylinder of wine in the longer leg.

The SECOND SECTION.

CHAP. I.

As to the cause of the saltness of the sea, I therein agree with the learned *Gassendus*, and some other modern writers, that the sea derives its saltness from the salt, that is dissolved in it: but I take that saltness to be supplied, not only from rocks, and other masses of salt, which at the beginning were, or in some places may yet be found, either at the

* Vid. *Stevinum*, Prop. 10. Lib. IV. Statices. And see the author's Hydrostatical Paradoxes.

bottom of the sea; or at the sides, where the water can reach them, but also (to say nothing here of what may, perhaps, be contributed by subterraneal steams) from the salt, which the rains, rivers, and other waters, dissolve in their passage through divers parts of the earth, and at length carry along with them into the sea. For not only it is manifest enough, that several countries afford divers salt springs, and other running waters, that at length terminate their course in the sea; but I have sometimes suspected, that very frequently the earth itself is impregnated with corpuscles, or, at least, rudiments of common salt, though no such thing be vulgarly taken notice of. Which suspicion may be confimed (to omit what I have elsewhere delivered on another occasion) partly by the observation of some eminent chemists, who affirm themselves to have found a not inconsiderable quantity of exceeding saline liquor upon the evaporation of large quantities of some waters, (for in some others I could not find it) and principally by the quantity of common salt, that is usually found in the refining of salt-petre; though that be a salt, which Sir *Francis Bacon*, and other experienced writers, teach, that almost every fat earth, kept from the sun and rain, and from spending itself in vegetation, will afford.

BUT having, on another occasion, sufficiently shewed, † that the earth does abound with common salt, in many more places than are wont to be taken notice of; and that it is probable, that by maturation, or otherwise, salt may daily grow in the earth, it will not be necessary to add, in this place, any thing to what I have said already to prove, that our common terrestrial salt, being dissolved, may suffice to make the sea-water brackish; and the rather, if we call to mind what has been formerly said about the possibility of springs rising beneath the surface of the sea, and of lumps of salt, that were taken up by divers, undissolved, at the bottom of the sea; the ocean may receive supplies of salt from rocks, and springs latent in its own bosom, and unseen even by philosophers. And this may be one reason, I conceive, (for I deny not but that there may be others, as the very unequal heat of the sun, &c.) why some seas are so much salter than others, or, at least, why in some places, the sea-water may be much salter than in others.

AND as we have seen, that our common terrestrial salt may be copiously enough communicated to the sea, to impregnate it with as much saltness as we observe it to have; so I do not see, that the difference between that salt and sea-salt is so great, but that it may well be supposed to be derived from those changes, that the terrestrial salt may be liable to, when it comes into the sea. For that the marine salt, and the terrestrial, do very well agree in the main things, may be argued from the resemblance both in shape, taste, &c. that may be observed between the grains, that will be produced, if we expose each of them in a distinct glass to such a heat, as may slowly carry off the superfluous moisture, and suffer them to coagulate into cubical, or almost cubical grains: and the lesser differences, that may be met with between these two salts, may well enough be supposed producible by the plenty of nitrous, urinous, and other saline, to which, in some places, may be added, bituminous bodies, that by land-floods, and otherwise, are from time to time carried into the sea, and by several things, that happen to it there, especially by the various agitation it is put into by tides, winds, currents, &c. and (which I would by no means omit) by its being in vast quantities exposed to the sun and air.

CHAP. II.

WE may justly be the more careful to determine, whether the saltness of the sea-water proceed from common salt dissolved in it, because if it appeared to be so, we might the more hopefully attempt to obtain by distillation sweet water from sea-water; since, if this liquor be made by the bare dissolution of common salt in the other, it is probable, that a separation may be made of them, by such a heat, as will easily raise the aqueous parts of sea-water, without raising the saline, whose distillation requires a vehement heat, as chemists well know to their cost. And such a method of separating fresh water from that, which was salt, would make our doctrine of use, and be very beneficial to navigation, and consequently to mankind. For in long voyages, it is but too common for the makers of them, to be liable to hazards and inconveniencies, for want of fresh and sweet-water, whereby they are sometimes forced to drink corrupt brackish water, which gives them divers diseases, as particularly the scurvy, and the usual effect of drinking salt-water, the dropsy. And seamen are wont to receive so many other incommodities by the want of fresh water, that, to prevent or supply it, they are oftentimes forced to change their course, and sail some hundreds of miles to a coast, not only out of their way, but unsafe in it self, and perhaps more dangerous, by being infested by pirates, or in the hands of enemies or savage people; by which means, they often lose the benefit of their Monsouns, and much more easily other winds, and frequently their voyage. And these are inconveniencies, which might be in good measure prevented, if potable, and at least tolerably wholesome water could be obtained by distillation, in the midst of the sea it self, to serve the sea-men, till they could be supplied with naturally fresh water. To make some trials of this, I remember I took some English sea-water, whence I was able to separate betwixt a thirtieth and fortieth part of dry salt; and having distilled it in a glass head and body, with a moderate fire, till a considerable portion of it was drawn over, we could not discern any saltness in it by the taste; and besides that I found it specifically lighter than such water, as is daily drank by persons of quality at *London*, I exposed it to a more chemical examen, and did

† In a Tract of Subterraneal Menstruums.

did not by that find any thing of sea-salt in it, though I have at several times, by the same way, manifestly discovered a saltness in in-land waters, that are drank obviously for sweet waters. If I would have employed a stronger heat, and vessels larger and lower, or otherwise better contrived for copious distillation, I might in a shorter time have obtained much more distilled water; but whether such liquors will be altogether so wholesome, experience must determine. Yet that sea-water distilled even in no very artificial way, may be so far wholesome, as not in haste to be sensibly noxious, but at a pinch useful, at least for a while, may be gathered from (what occurs to me since the writing of the last paper) the testimony of that famous navigatior, Sir *R. Hawkins*, who commanded a fleet in the *Indies* for Queen *Elizabeth*. For he, in the judicious account he gave the world of his voyage, wherein they were distressed, even in the admiral's ship, for want of fresh water, has this memorable passage, as I find it verbatim in our diligent *Purchas**.

"ALTHOUGH our fresh water had failed us " many days (before we saw the shore) by reason of our long navigation without touching " any land, and the excessive drinking of the " sick and diseased (which could not be excused;) yet with an invention I had in my ship, " I easily drew out of the water of the sea sufficient quantity of fresh water, to sustain my " people with little expence of fuel: for with " four billets I stilled a hogshead of water, and " therewith dressed meat for the sick and " whole. The water so distilled we found to " be wholesome and nourishing."

AND because the potableness of sea-water may concern the healths and lives of men, I shall here add to what I elsewhere deliver about my ways of examining, whether other waters participate of salt, two or three observations I made upon those few distilled liquors, I had occasion to draw from sea-water. Having then upon some of the distilled liquor dropped a little oil of tartar per deliquium, I perceived no clouds at all, or precipitation to be made; whereas a small proportion of that liquor being dropped into the undistilled sea-water itself, it would presently trouble and make it opacous, and, though but slowly, strike down a considerable deal of a whitish substance (which, of what nature it is, I need not here declare;) I found also, that a very small proportion of an urinous spirit, such as that of sal armoniac, would produce a whitish and curled substance (but not near so copious a one as the other liquor) in sea-water, not yet exposed to distillation, but not in the liquor drawn from it: which argued, that there were but few or no saline particles of sea-salt ascended with the water: for else these alkalizate and urinous salts would in all likelihood have found them out, and had a visible operation on them. And I farther remember, that when the distillation was made in glass vessels, with an easy fire, not only the first running, but the liquor, that came over afterwards, was not perceived to be brackish, but good and potable. To which agrees very well, that by a hydrostatical trial I found our distilled sea-water to be lighter in specie than common conduit water, though it exceeded that in specifick levity, less than it was surpassed in the same quality by distilled rain-water.

BUT to return to the subject, whence we have somewhat, but, I hope, not uselesly, digressed; I know it may be objected, that if the terrestrial salts carried by springs, rivers, and land-floods into the sea, were the cause of its saline taste, those waters themselves must be made salt by it, before they arrive at the sea. But besides that this objection will not reach the springs and rivers of salt water, that in several places, either immediately or mediately, discharge themselves into the sea; it might conclude against him, that should affirm this imported saltness to be the only cause of that of the sea; but it will not be of force against me, who take it to be only a partial cause, that by its accession contributes to the degree of saltness we observe in the sea, where this imported salt may join itself with the salt it finds there already, and being detained by it contribute to the brinyness of the water.

IF it be urged, that from hence it will follow, that the sea, from time to time, increases in saltness, I may suspend my answer, till it appear by competent observation, that it does not; which, I think, men have not yet made trials, that may warrant them to assert. And if the matter of fact were certain, I think it were possible to give a farther answer, and shew probable ways, how so small an accession of salt may be dispersed by nature, and kept from increasing too much.

CHAP III.

BUT now it is seasonable to consider, that the taste of sea-water is not such a simple saline taste, as spring-water would receive from sal gem, or some other pure terrestrial salt, dissolved in it, but a bitterish taste, that must be derived from some peculiar cause, that authors are not wont to take notice of. I am not assured by any observations of my own, that this recession, from a purely saline taste, is likely to be of the very same kind, and to be equally, or very near equally, met with in all seas; (not to add a doubt, whether it be at all sensible in some.) The cause both of the bitterness; and saltness too, of the sea-water, is said to be affirmed by learned Mr. *Lidyat*, to be adust, and bituminous exhalations ascending out of the earth into the sea. But that there is abundance of actual salt in the sea-water, to give it its saline taste, and ponderousness, the salt, that the sun does in many places copiously separate from the saltless waterish parts, sufficiently manifests. But as to the bitterish taste, I think it no easy matter to give a true account of it, but am prone to ascribe it, partly, to the operation of some catholick agents upon that vast body of the ocean, and partly, to the alteration, that the salt

* In lib. VII. p. 1378 of *Purchas*, out of Sir *R. Hawkins*'s voyage.

ſalt receives from the mixture of ſome other things, among which bitumen may be one of the principal.

BUT though I have, in another * paper ſhewn, that in ſome places of the ſea there are conſiderable quantities of bitumen, or bituminous matter to be met with; yet I dare not derive the bitterneſs of the ſea only from bituminous exhalations, but in good part, at leaſt, in ſome places, from the liquid, and other bitumen, that is imported by ſprings, and other waters, into the ſea; of which we have an eminent inſtance in that, which our Engliſh call *Barbadoes* tar, according to the relation I had of it from an inquiſitive gentleman, who is one of the chief planters of the iſland, and took pleaſure to obſerve this liquid bitumen to be carried in conſiderable quantities from the rocks into the ſea; and I think it poſſible enough, that ſome of the ſprings, that riſe under the ſurface of the ſea, may carry up with them bituminous matter, which may help to make the ſaltneſs of the ſea degenerate, (of which more perhaps elſewhere;) as I not long ſince made mention of ſprings, as well of hot, as cold water, riſing beneath the ſurface of the ſea. And this minds me to intimate here, that I have ſuſpected, that in ſome places the ſulphureous exhalations, and other emiſſions from the ſubmarine parts of the earth, may ſometimes contribute to change the ſaline taſte of the ſea-water: for I have elſewhere related, how, not only ſulphurous ſteams, but ſometimes actual flames, have broken through from the lower parts of the ſea to the uppermoſt; and have ſometimes taken pleaſure to make, by art, a rude imitation of that phænomenon. And partly ſome experiments of my own, and partly ſome other inducements, have perſuaded me, that divers times (for I do not ſay always) ſea-ſalt does not obſcurely participate of combuſtible ſulphur, of which I may ſpeak farther on another occaſion. But in regard, that the taſte of the ſea-water is not in all parts of the ocean uniform, it may here ſuffice to take notice in general, that this difference of taſte may partly be cauſed by adventitious bodies of ſeveral kinds, of which it is probable, that, in differing places, the ſea-water does variouſly partake. And, not to mention here the fragrant ſmell of violets, which has, by ſeveral, and particularly by an eminent perſon, of whom I enquired about it, been obſerved, in ſome hot countries, to proceed from ſea-ſalt; I have divers other inducements to think, that it is uſually no ſimple ſalt, nor free from mixture. For by more ways than one, and particularly by cohobating from it its own ſpirit, we have obtained a dry ſublimate, which ſeemed to be no pure, but a compounded body.

AND now to come to that, which I intimated might be one of the cauſes, why the taſte of ſea-water is not the ſame with that of common ſalt, diſſolved in freſh water; I ſhall add, that I have ſuſpected, that the various motion of the ſea, and its being expoſed to the action of the air and ſun, may contribute to give it a taſte other than ſaline; which ſuſpicion might be confirmed, by the obſervation I elſewhere mention of the ſea-ſalt, which, by barely being expoſed for many months to the air, and ſometimes perhaps put into a gentle agitation by a digeſtive heat, I found to have a very manifeſtly differing taſte from the ſimple ſolution of ſea-ſalt in common water.

I might here endeavour the farther confirmation of my diſcourſe, by what I have learned by enquiry from navigators, about the manifeſtly differing colours, and other qualities, of the differing parts of the ſea, which ſeem to argue, that it is not every where of ſuch a uniform ſubſtance as men vulgarly imagined, and that vaſt tracts of it are imbued with ſtupendous multitudes of adventitious corpuſcles, which, by ſeveral ways diverſifying its parts, keep it from being a ſimple ſolution of ſalt. But of this ſubject I have not leiſure to diſcourſe here; only becauſe it is generally thought, that the ſea-water is, by reaſon of the ſaltneſs it abounds with, uncapable of putrefaction, I will add, that having kept a pretty quantity of ſea-water, that I had cauſed to be purpoſely taken up between the Engliſh and French ſhores, in a good new rundlet, in a place, where the ſummer ſun beat freely upon it, it did, in a few weeks, acquire a ſtrongly ſtinking ſmell; though that the experiment had been more ſatisfactory, I wiſhed, that it had been made in a veſſel of glaſs, or earth, inſtead of wood. But a much better obſervation I procured from a much eſteemed navigator of my acquaintance, who having ſailed often in the Indian and African ſeas, I enquired of him, whether he had ever, in thoſe hot climates, where the ſea is ſuppoſed to be very ſalt, obſerved it to ſtink, for want of agitation, or otherwiſe: to which he anſwered, that once being, though it was but in *March*, becalmed, in a place he named to me, for twelve or fourteen days, the ſea, for want of motion, and by reaſon of the heat, began to ſtink, inſomuch that he thinks, if the calm had continued much longer, the ſtench would have poiſoned him: they were freed from it as ſoon as the wind began to agitate the water, and broke the ſuperficies, which alſo drove away ſtore of the ſea tortoiſes, and a ſort of fiſh, whoſe Engliſh name I know not, that before lay baſking themſelves on the top of the water.

AND to this agrees very well the notable obſervation, that I ſince met with, of the elſewhere commended Sir *R. Hawkins*, who, among other conſiderable things he takes notice of in his relations, has this paſſage, to our preſent purpoſe. † "Were it not for the "moving of the ſea by the force of winds, "tides, and currents, it would corrupt all the "world. The experience I ſaw, *anno* 1590, "lying with a fleet about the iſlands of *Azores*, "almoſt ſix months, the greateſt part of the "time we were becalmed; with which all the "ſea became ſo repleniſhed with ſeveral ſorts "of gellies, and forms of ſerpents, adders, "and ſnakes, as ſeemed wonderful, ſome "green, ſome black, ſome yellow, ſome white, "ſome

* In the Tract of Subterraneal Menſtruums.

† *Purchaſe*'s Pilgrims in Sir *R. Hawkin*'s Obſervations.

"some of divers colours, and many of them "had life; and some there were a yard and a "half, and two yards long; which, had I not "seen, I could hardly have believed. And "hereof are witnesses all the company of the "ships, which were then present, so that hard- "ly a man could draw a bucket of water clear "of some corruption. In which voyage, to- "wards the end thereof, many of every ship "fell sick of this disease, and began to die "apace, but that the speedy passage into our "country was a remedy to the crazed, and "a preservative for those, that were not "touched."

The THIRD SECTION.

CHAP. I.

AS for the various degrees of the saltness of the sea, authors are wont to be silent of it, save that some navigators tell us, that they observed some seas to have a more, and others a less saline taste; which, you will easily believe, has not afforded me much satisfaction. And, on the other side, my want of opportunity to make trials myself will confine me to acquaint you with no more than the few following observations.

1. To a learned man, that was to sail to places of differing latitudes in the Torrid Zone, I delivered a glass instrument, elsewhere described, fitted by the greater or lesser emersion of the upper part, to shew, accurately enough for use, the greater, or less specifick gravity of the salt water it was put to swim in. This he put, from time to time, into the sea-water, as he sailed towards the *Indies*, whence he wrote me word, "That he found, by the glass, "the sea-water to increase in weight, the "nearer he came to the line, till he arrived at "a certain degree of latitude, as he remembers, it was about the thirtieth; after which, "the water seemed to retain the same specifick gravity, till he came to the *Barbadoes*, "or *Jamaica*.

2. ANOTHER observation I obtained by enquiry of an ingenious person, and a scholar, at his return out of the *East-Indies*, who affirmed to me, that he, and a gentleman of my acquaintance, took up bottles full of sea-water, both under the Equinoctial, and also off the *Cape of good Hope*, which lies in about thirty-four degrees of southern latitude, and found the waters of these distant parts of the ocean to be of the same weight. And though it may well be doubted, whether this observation, being made with ordinary bottles, were so exact as could be wished; yet the persons being curious, and making it for their own satisfaction; and, my relator having, in both the recited places, filled with the sea-water he took up, and weighed; having, I say, filled the same bottles, since this vessel held two quarts (which must be above four pounds of salt-water) if the disparity of weight had been considerable, it would, in likelihood, have been found, at least manifestly sensible, in such a weight of liquor.

3. ENQUIRING of an observing person, that had been at *Mosambique*, which is thought to be one of the hottest places in the world, whether he did not there find the sea to be more than ordinarily salt? he answered me, that coming thither in a great carack, when he came back from the town to the ship, he observed near two hands breadth of the vessel to be above the ordinary part, to which it used to sink; insomuch, that he took notice of it to the captain, as fearing, that part of the lading had been by stealth carried to the shore: but the pilot, who had made thirteen or fourteen voyages to the *Indies*, assured him, what he had observed about the ship, was not unusual in that place, where the taste itself discovered the water to be exceeding salt.

NOR need we scruple to think, that some sea-waters may be very much more impregnated with salt than ours; for water will naturally dissolve, and retain a far greater proportion of salt, than that, which is commonly met with in the sea. For whereas a thirty-fifth, or thirtieth, or at most a twenty-fifth part of salt, will make water more saline than is found in many seas, I am, by a friend of mine, that is master of a salt work, informed, that the water of his springs afford him a twelfth part of good white salt, and that another spring, not far off, yields no less than an eighth part. To which (to avoid anticipation) I shall not here add, what I shall hereafter have occasion to say of the fullest impregnation of water with common salt.

[WHILST I was reviewing these papers, there came seasonably to my hands a letter written from *Musilapatan*, on the gulf of *Bengala* in the *East-Indies*, by an ingenious gentleman, Sir *William Langhorn*, that is entrusted with the care of the English factories in those parts; out of which letter the following passage is *verbatim* transcribed. "I did, in "order to your command, cause some water "to be saved under the line, at our first ac- "cess to it, intending, for want of good scales "and weights, (being none to be come at a- "board the ship) to have kept it, until it could "be weighed, but by the forgetfulness of a "servant, it was thrown away. Off the *Cape*, "in 37 d. 00 m. southern latitude, I saved "some again, and, through the same want of "weights, was fain to keep it, until I came to "the line again; and then made the best shift "I could for weights, and compared it with "the water there, filling the same bottle a- "gain to the same height by a mark, and "found it exactly the same weight. The "weight I have taken; but accounting this "a journey of business, left those notes, and "most of the like nature, behind me; in my "next it shall be inserted."]

CHAP. II.

IT remains now, that, according to my promiſe, I ſet down what I obſerved myſelf concerning the ſaltneſs of our ſea between *England* and *France*; not in compariſon with the ſaltneſs of other ſeas, whoſe waters I had not to compare with, but as to the proportion of ſalt contained in it to the water. And though one would think it very eaſy to make trials of this ſort, for a perſon not unacquainted with hydroſtatical practices, nor unfurniſhed with inſtruments, yet, I confeſs, that three or four trials that I made, not all of them the ſame way, made me find it more difficult, than was imagined, to arrive at any thing of certainty in this enquiry.

THIS you will eaſily believe, if I annex the ſubſtance of ſome experiments, that I remember I made about the gravity of ſea water, which I had ordered to be taken up, ſome at the depth of about fifteen fathom ſomewhat near our ſhore, and ſome in another place of the channel, between *England* and *France*.

THE ſum of the firſt experiment is this: we took a vial, fitted with a long and ſtrait neck, purpoſely made for ſuch trials, and having counterpoiſed it, filled it to a certain height with common conduit water: we noted the weight of that liquor, which being poured out, the vial was filled to the ſame height with ſea water, taken up at the ſurface, and by the difference between the two weights, the ſea water appeared to be about a forty-fifth part heavier than the other.

THE ſecond trial (which was for more accurately made hydroſtatically,) I find regiſtered to this effect: We carefully counterpoiſed in the ſcales, formerly made uſe of, a piece of ſulphur in the upper ſea-water, formerly mentioned; it weighed ℥ ß + *gr*. x. ß and being alſo weighed in the ſea water fetched from the bottom, gave us the ſame weight ℥ ß + *gr*. x. ß which ſhewed thoſe two waters to be of the ſame ſpecifick gravity: and then to compare this with the gravity of common water, we weighed the ſame ſulphur in common conduit-water, and found it ℥ ß + *gr*. xv. ß: by which it appeared, that the ſea-water was but about a fifty-third part heavier than this water: which is ſuch a difference from the proportion found out by the former way of trial, that I could not well imagine, what to attribute it to, unleſs the ſea-water by long ſtanding in a veſſel, which, though covered, was expoſed to the hot ſun, may both have been rarified, and have had ſome ſeparation made of its ſaline or other heavier parts, on which ſcore that portion we took up for our trial, might appear lighter than elſe it would have done; or unleſs the experiment having been made in *London*, where great and ſudden rains and other accidents will ſometimes viſibly vary the conſiſtence of common water, the liquor, I then employed without examining it, might be more ponderous at that time than at another. To which latter ſuſpicion I was the more inclined, becauſe, having afterwards weighed the ſame piece of ſulphur by help of the ſame balance in diſtilled rain water, I found the weight of the former liquor to exceed that of the latter by a good deal leſs than a thirty-fifth part; which ſeemed to make it probable, that if the water, we chanced to employ, had been free from all ſaline and other heavy particles, the difference formerly mentioned betwixt this obſervation and the foregoing would not have been near ſo great as it was.

THE laſt way I made uſe of to examine the proportion betwixt ſea-water and freſh, was chemical; whereof my regiſter affords me this account.

A pound avoirdupoiſe weight of the upper ſea-water was weighed out, and put into a head and body to be diſtilled in a digeſtive furnace *ad ſiccitatem*; and the diſtillation being leiſurely made, the bottom of the glaſs was almoſt covered with fair grains of ſalt, ſhot into cubical figures, and more white than was expected: in the reſt of the coagulated matter, we took not notice of any determinate ſhape. The ſalt being weighed, amounted to ℥ß, averdupois, and *gr*. x. At which rate, the proportion of the ſalt to the water will be that of 30 and $\frac{72}{100}$ to one, and ſo will amount to near the thirtieth part; which was ſo much greater than the former ways of trial made us expect, that I know not, whether it may not be worth while to try, whether ſuch a ſlow abſtraction, as we employ of the ſuperfluous water, and our doing it in cloſe veſſels, may not have afforded us more ſalt, than elſe we ſhould have obtained.

To this relation I find this note ſubjoined: ſuſpecting, that there may have ſomewhat elſe concurred to our finding ſo great a proportion of ſalt, I ſuffered that, which had been weighed, to continue a-while in the ſcale, and ſoon perceived, that, according to my conjecture, that ſcale began manifeſtly to preponderate, and that conſequently ſome of the unexpected weight of ſalt may be due to the moiſture of the air, imbibed after the ſalt was taken out of the glaſs, and laid by to be weighed: wherefore, cauſing it to be very well heated and dried in a crucible, we found it to weigh ʒiij. + ß. (that is 210 grains,) upon which account the proportion of ſalt contained in the water, was a thirty-ſixth part, and ſomewhat above half of thoſe parts, and to expreſs it in the neareſt whole number, a thirty-ſeventh part.

FROM whence this greater proportion of ſalt by diſtillation, than our other trials invited us to expect, proceeded, ſeems not ſo eaſy to be determined; unleſs it be ſuppoſed (as I have ſometimes ſuſpected) that the operation the ſea-water was expoſed to in diſtillation, made ſome kind of change in it, other and greater than before-hand one would have looked for; and that, though the grains of ſalt we gained out of the ſea-water, ſeemed to be dry before we weighed it, yet the ſaline corpuſcles, upon their concreting into cubes, did ſo intercept between them many ſmall particles of water, as not to ſuffer them to be driven away by a moderate warmth; and conſequently ſuch grains of ſalt may have upon this account been leſs pure

pure and more ponderous than elſe they would have been. And I might here add, that I ſometimes make a certain artificial ſalt, which, though being diſſolved in water, it will ſhoot into cryſtals finely ſhaped, and dry enough to be reducible into powder, yet coagulates water enough with it to make the water almoſt, if not quite, as heavy again as before. And I have been aſſured by a very learned eye-witneſs, that there is a ſort of ſea-ſalt, which they bring to ſome parts of *England*, from the coaſt of *Spain* or *Portugal*, which being here diſſolved, and reduced by purification and filtration to a much whiter ſalt, will yield by meaſure ſomewhat above two buſhels for one. But to ſatisfy the ſcruples and ſuſpicions I could ſuggeſt, would require more trials than I have now time or opportunity to make. What has been already delivered, may give at leaſt as ſcrupulous an account of the ſaltneſs of our Engliſh ſea-waters, as moſt other experimenters would have thought it needful to give. And to make a determination with any certainty about the degrees of the ſea's ſaltneſs in general, a great number of obſervations, made in different climates, and in diſtant parts of the ocean, would be neceſſary.

CHAP. III.

I KNOW not, whether I may be ſo indulgent to my ſuſpicions, as to wiſh, that obſervations were heedfully made, whether in the ſame ſea, and about the ſame part of it, the waters be always equally ſalt? For, though that be taken for granted, yet ſince we have no good obſervations long ſince made to ſilence the ſuſpicion, one may ſuſpect, that, at leaſt in many places, the ſaltneſs of the ſea may continually, though but very ſlowly, increaſe by the acceſſion of thoſe ſaline corpuſcles, that are imported by ſalt-ſprings, and thoſe, which rivers and land-floods do from time to time rob the earth of. And I ſuſpect it to be not impoſſible, that this, or that part of the ſea, may be ſometimes extraordinarily, and perhaps ſuddenly, impregnated with an additional ſaltneſs from ſaline ſteams plentifully aſcending into it, from thoſe ſubterraneal fires, about which I have made it elſewhere probable, that they may burn beneath the bottom of the ſea, and ſometimes ſend forth copious exhalations into it. But it may prove the more difficult to diſcern this adventitious ſaltneſs, unleſs the taſte, as well as balance, be employed about it; becauſe the ſalt, that produces it, may be of ſuch a nature, as to be much lighter in ſpecie than common ſea ſalt. And the mention of this leads me to give you here the advertiſement I promiſed you not long ago.

THAT though the weight of ſea-water be as good a way as is yet employed (and better than ſome others) to determine what ſea-water does moſt abound in ſalt; and though it be poſſible, that in our ſea, and perhaps, in almoſt all others, this way be not liable to any conſiderable uncertainty; yet I think it not impoſſible, that it may ſometimes deceive us, eſpecially in very hot regions; becauſe I have obſerved, that there may be volatile ſalts, which, though by reaſon of their activity, they make ſmart impreſſions on the tongue, and give the water imbued with them a ſtrong ſaline taſte, yet they add very little, and much leſs than one would think, to its ſpecifick gravity: as I have tried, by hydroſtatically examining diſtilled liquors, abounding in volatile and urinous ſalts, ſome of which I found very little heavier than common water, and conſequently nothing near ſo much heavier, as they would have been made, if they had been brought to ſo ſharp a taſte, by having nothing but common ſea ſalt diſſolved in them: ſo that, if in any particular place, by any other way, or from the ſteams of the earth beneath, ſome of which, I elſewhere ſhew, may be very analogous to thoſe afforded by ſal armoniack) the ſea ſhould be copiouſly impregnated with ſuch kind of light ſalts, the ſea-water may be much more ſalt to the taſte, and yet be very little heavier. For confirmation of which I find among my notes, that weighing a ſealed bubble of glaſs, made heavy by an included metal, firſt in ſpirit of ſal armoniack, that taſted much ſtronger than ſea-water, it weighed ʒiij + *gr.* 51 $\frac{1}{4}$, and weighing this ſame body in fair water, it weighed but ʒiij + *gr.* 45 $\frac{3}{4}$; ſo that notwithſtanding, its great ſaltneſs, the ſpirit was lighter than common water; though a good part of that comparative levity may probably be aſcribed to the liquor, wherein the ſaline particles ſwarm, which, by diſtillation, was grown more defecated and light, than common, though clean, water.

BUT for a farther proof, we took a hard lump of ſal armoniack; and though we could not weigh it in water, becauſe that would have diſſolved part of it, yet by a way (I elſewhere teach) I found, that weighing in the ſame liquor this lump of ſal armoniack, and a lump of good white ſea ſalt, (brought me as a curioſity out of the Torrid Zone) the proportion of the latter to a bulk of the liquor equal to it, was ſomething (though exceeding little) above that of two and a quarter to one, and the proportion of ſal armoniack to as much water, as was equal likewiſe to it, did not above a centeſm exceed that of one and $\frac{7}{10}$ to one; which falls ſo ſhort of the other proportion, as may juſtly ſeem ſtrange, eſpecially if it be conſidered, that the factitious ſal armoniack, the chemiſts generally uſe, and we employ, conſiſts in good part of ſea ſalt, which abates much of the comparative levity it might have, if it were made up only of urinous and fuliginous ſalts, which were its other ingredients.

IT were indiſcreet for me to propoſe any more ſuſpicions and trials fitted to clear them, unleſs I knew thoſe I have already mentioned would not paſs for extravagancies; and therefore, I ſhall here diſmiſs the ſubject of this tract of the ſaltneſs of the ſea, but that, ſince I have been diſcourſing of the degrees of it, it will not be impertinent to add, what is the greateſt meaſure of ſaltneſs, that I have brought

* In the Tracts of Subterraneal Fires and Steams.

brought water to, without the help of external heat. On this occasion, I employed two differing ways: the one was, by putting into a well-counterpoised vial two ounces of common-water, and then putting into it, well-dried and white, common salt, and shaking them together, till the liquor would, whilst cold, dissolve no more: this liquor, thus glutted with salt, weighed 1150 grains, from which two ounces being deducted, the overplus of weight, arising from the dissolved salt, amounted to 190 gr. so that a parcel of salt will, without heat, be dissolved in about five times its weight, or very little more, of common water. By which proportion we made so strong a brine, that divers pieces of amber being purposely let fall into, emerged, and floated on it. The other and better way, yet more tedious, that we made use of, was to let sea-salt run *per deliquium*, (as the chemists speak) that is, to set it in some moist place, till it was dissolved by the aqueous vapours, that swim in the air. In this liquor we weighed a piece of sulphur, which we also weighed in sea-water, wherein, finding it to weigh much more than in the former liquor, it appeared, that the sea-water was, in specie, much lighter than the other; though how much their gravities differed, I cannot find among my notes, nor be informed by my memory.

AND because I have not, in any author, met with the proportion of sea-salt to water of the same bulk, nor perceive, that hydrostaticians themselves have yet attemped any way to investigate it, (probably deterred by the easy dissolubleness of salt in water,) I shall here subjoin, that by the help of an expedient, I have elsewhere taught, I have examined a hard dry lump of sea-salt, and found its proportion in weight, to common water of the same bulk, to be almost as 2 to 1, (for it exceeded the *ratio* of 1 $\frac{9}{10}$ to 1.) And, I remember, I found the specifick weight of a hard and figured lump of *sal gem.* (which sort of salt I suppose to be somewhat more pure and ponderous than sea-salt) to be to that of water (very near) as 2 $\frac{1}{8}$ to 1.

THE FOURTH SECTION

Belonging to the TRACT, entitled,

Relations about the BOTTOM of the SEA.

THE presence of the air is not only so necessary to the life of many sorts of animals, but it hath likewise so great a stroke in the growth of vegetables, especially of the larger sorts, that, after what I had experimented about these matters, (of which this is not the proper place to give an account) I thought fit to make enquiry about the vegetation and growth of plants of considerable bulk, in those submarine regions, where, if there grow any, they must do it remote from the free contact of an ambient air. And having not now the leisure to repeat what botanists (of whose books I am not now provided) deliver about lesser plants growing under water, I shall now only present you with what information I could procure from navigators, about trees and fruits growing at the bottom of the sea.

TO what I have elsewhere had occasion to say to their opinion, that will not allow coral to be really a stony plant, but a liveless concrete, that is always hard and brittle under water; I shall now add, that, enquiring lately of an eminent and inquisitive person, that had spent some time upon the coast of *Africa*, where he had been present at the fishing of coral, and learning from his answer, that he had seen it not far from *Algiers*; I asked him, whether he had himself observed the coral to be soft, and not red, when it was newly brought from the bottom of the sea. To which he replied, that he had found it soft and flexible; and that, as for the colour, it was for the most part very pale, but with an eye of red, the bark being worse coloured than the substance it covered was; but when the bark was taken off, and the other part exposed to the air, the expected redness of the coral disclosed itself.

WHEN I demanded, whether he had observed, that any inky sap ascended to nourish the stony plant? and whether he had seen any thing like berries upon it? he ingenuously confessed to me, he had not been so curious, as purposely to make enquiry into those particulars; but that he remembered, that having broken some of the large pieces of coral, he took notice, that the more internal substance was much paler than the other, and very whitish; and that at the extream parts of some branches, or sprigs, he observed little blackish knobs, which he did not then know what to make of: and when I enquired, what depth the sea was of in that place? he answered, that it was nine or ten fathom. But as to the fruit of some kinds of coral, if I do not much mis-remember, I was, not long since, assured by a scholar, that navigated much in the east, that they divers times meet with in those seas a certain sort of coral, but not white, which bears a small fruit like a round berry, of a pleasant colour, and esteemed as rarities.

DISCOURSING with a person, that made diving his trade, whether he had not met with any

any trees or fruit in the depths of the sea? he told me, that in a great ship, whereinto he descended, to recover thence some shipwrecked goods, he was surprized to find in several places a certain sort of fruit, that he knew not what to make of, for he found them of a slimy and soft consistence, about the bigness of apples, but not so round in shape; and when he brought them up into the air, as he did many of them, they soon began to shrink up like old rotten apples, but were much harder, and more shrivelled. And it is remarkable, that this happened in a cold northern sea.

ONE, that made a considerable stay about *Manar*, a place I have often mentioned, answered me, that he learned from the divers, that in some places thereabouts there grows at the bottom pretty store of a certain sort of trees, bearing leaves almost like those of laurel, as also a certain fruit; but of what virtue, or other use, he had not the curiosity to enquire.

I was also informed by an eye-witness, that near the famous coast of *Mosambique* in *Africa*, there grows at the bottom of the sea store of trees, that bear a certain fruit, which he describes to be very like that, which, in *America*, they are wont to call *Acayu*, the leaves also resembling those of that tree.

BUT the best welcomest information I could procure about submarine plants, is that, which concerns the famous Maldivian nut, or Cocoa, which is so highly esteemed in the east, that some write, it is a great present from one king to another, and even much extolled in *Europe* by experienced physicians: for the origin of this dear drug is almost as much converted, as the alexiterial virtues are extolled. Having then once the good fortune to meet with a man of letters, that had resided in those unfrequented islands, I found he had been as inquisitive, as I could reasonably expect, about these admired productions of the sea, and that he had often learned from the divers, that they are real nuts, or fruits, borne by a sort of cocoa-trees, that grow at the bottom of the sea, and are thence, either torn off, by the agitation of the water, or gathered by the divers. These fruits are smaller than most other sorts of cocoa's, whose maturity they do not seem to arrive at. He thinks, the species may have been very differing from what it is, and may have come from nuts fallen into the sea, together with the ruin of some little islands undermined by the water, and so submerged; of which he told me, he saw, at least, three or four instances during his stay there. He told me, that whilst the fruit was under water, they observed no distinct shell and kernel, but the entire nut was so soft, that it may easily enough cut with a knife, and was eaten like their other fruits; but being kept about a week in the hot air, it grows solid, and so hard, as to require good steel tools to work upon it. He added, that though, even upon the place, the fairer sort be of very great esteem, yet not of any such prodigious price, as is given out. And he presented me one about the bigness of a large egg, and a fragment of another, which are both very hard; but as for their virtues, I can yet say nothing upon trial, for want of having had fitting opportunities.

OTHER observations made at the bottom of the sea may hereafter follow.

A PARADOX OF THE NATURAL AND PRETERNATURAL STATE OF BODIES, Especially of the AIR.

I KNOW, that not only in living, but even in inanimate bodies, of which alone I here difcourfe, men have univerfally admitted the famous diftinction between the natural and preternatural, or violent ftate of bodies, and do daily, without the leaft fcruple, found upon it hypothefes and ratiocinations, as if it were moft certain, that, what they call nature, had purpofely framed bodies in fuch a determinate ftate, and were always watchful, that they fhould not by any external violence be put out of it.

BUT notwithftanding fo general a confent of men in this point, I confefs, I cannot yet be fatisfied about it in the fenfe wherein it is wont to be taken. It is not, that I believe, that there is no fenfe, in which, or in the account upon which, a body may be faid to be in its natural ftate; but that I think the common diftinction of a natural and violent ftate of bodies has not been clearly explained, and confiderately fettled, and both is not well grounded, and is oftentimes ill applied. For, when I confider, that whatever ftate a body be put into, or kept in, it obtains or retains that ftate, according to the catholick laws of nature, I cannot think it fit to deny, that in this fenfe the body propofed is in a natural ftate; but then, upon the fame ground it will be hard to deny, but that thofe bodies, which are faid to be in a violent ftate, may alfo be in a natural one, fince the violence, they are prefumed to fuffer from outward agents, is likewife exercifed no otherwife than according to the eftablifhed laws of univerfal nature. It is true, that when men look upon a body as in a preternatural ftate, they have an idea of it differing from that, which they had whilft they believed it to be in a natural ftate: but perhaps this difference arifes chiefly from hence, that they do not confider the condition of the body, as it refults from the catholic laws fettled among things corporeal, and relates to the univerfe, but eftimate it with reference to what they fuppofe is convenient, or inconvenient, for the particular body it felf. But however it feems to me, that men's determining a body to be in a natural or preternatural ftate has much more in it, either of cafual, or of arbitrary, or both, than they are aware of. For oftentimes we think a body to be brought into a violent ftate, not becaufe really the former was not fo, but becaufe there is a notable change made in it by fome agent, which we alfo take notice of; whereas before the action of that agent, if the body were under any violence, it was exercifed by ufual, but often immanifeft agents, though perhaps their compulfion were not lefs, but only lefs heeded. And fometimes alfo no more is to be underftood by a body's being forced from its natural ftate, than that it has loft that, which it had immediately, or a pretty while before fome notable change. Which conjectures I fhall now endeavour to confirm, but with great brevity.

I have already fhewn, that matter being devoid of fenfe and appetite, cannot be truly and properly faid to affect one ftate or condition more than another, and confequently has no true defire to continue in any one ftate, or to recover it when once loft; and inanimate bodies are fuch, and in fuch a ftate, not as the material parts they confift of, elected or defired to make them, but as the natural agents, that brought together and ranged thofe parts, actually made them. As a piece of wax is unconcerned, whether you give it the fhape of a fphere, or a cone, or a pillar, or a boat; and whether, when it has that form, you change it into any other; the matter ftill retaining without willingnefs or unwillingnefs, becaufe without perception, that figure, or ftate, which the laft action of the agents (your fingers or inftruments) determined it to, and left it in.

BUT this will be beft underftood, as well as confirmed, by particular examples. I need not tell you, that the moft ufual inftance alledged to fhew, that a ftate is natural to a body, and that being put out of it by external caufes it will upon the ceffation of their violence be reftored thereunto, is, that water being heated by the fire, as foon as that adventitious heat vanifhes, returns to its native coldnefs; and fo when, by an excefs of cold, it is congealed into ice, it does upon a thaw lofe that preternatural hardnefs, and recover the fluidity, that naturally belongs to it: and the fame may be likewife faid of butter, which, being melted by by external heat into a liquor, does upon the cessation of that heat grow a consistent body again. But perhaps these instances will rather countenance our paradox than disprove it. For as to the coldness, whereto water heated by the fire returns, when it is removed thence, it may be said, that the acquired heat consisting but in the various and brisk agitation of the corpuscles of the water by an external agent, it need be no wonder, that when that agent ceases to operate, the effect of its operation should cease too, and the water be left in its former condition, whether we suppose it to have been heated by the actual pervation of the corpuscles of the fire, which must by degrees, fly away into the air; or that the heat proceeds from an agitation imparted by the fire to the aqueous corpuscles, which must, by degrees, lose that new agitation, by communicating it little by little to the contiguous air and vessel; so that, if the former agitation of the particles of the water were, as is usual, much more languid than that of our organs of feeling, in which faintness of motion the coldness of water consisted, there will be no need of any positive internal form, or any care of nature to account for the water's growing cold again. This will be confirmed by the consideration of what happens to ice, which is said to be water brought into a preternatural state by an excess of cold. For I doubt, it will not be easily demonstrated, that in reference to the nature of things, and not to our arbitrary ideas of them, ice is water preternaturally hardened by cold, and not water ice preternaturally thawed by heat. For if you urge, that ice left to it self will, when the frigorific agents are removed, return to water; I shall readily answer, that, not to mention the snow and ice, that lies all the summer long unthawed upon the tops of the Alps and other high mountains, I have learned, by enquiry purposely made, from a doctor of physick, who for divers years practised in *Muscovy*, that in some parts of *Siberia* (a large province belonging to the Russian emperor) the surface of the ground continues more months of the year frozen, by what is called the natural temperature of the climate, than thawed by the heat of the sun; and that a little beneath the surface of the ground, the water, that chances to be lodged in the cavities of the soil, continues frozen all the year; so that, when in the heat of summer the fields are covered with corn, if then you dig a foot or two, perhaps less, you shall easily find ice and a frozen soil: so that a man born and bred in the inland part of that country, and informed only by his own observation, may probably look upon water as ice violently melted by that celestial fire, the sun, whose heat is there so vehement in their short summer, as to ripen their harvest in less time than in our temperate climates will easily be credited.

On the other side, we in *England* look upon melted butter, as brought into a violent state by the operation of the fire, and therefore think, that when being removed from the fire it becomes a consistent body again, it has but recovered its native constitution. Whereas there are divers parts of the *East Indies*, and, I doubt not, of other hot countries, whose inhabitants, if they should see consistent butter (as sometimes by the care and industry of the Europeans they may do) they would think it to be brought to a preternatural state, by some artificial way of refrigeration. For in those parts of the *Indies* I speak of, (though not in all others) the constant temper of the air being capable to entertain as much of agitation as suffices for fluidity in the parts of what, in our climate, would be butter, it would be in vain to expect, that, by being left to it self in the air, it should become a consistent body. And I have learned by diligent enquiry of sea-men and travellers, both English and others, that were eye-witnesses of what they told me, that, in divers parts of those hot regions, butter, unless by the Europeans, or their disciples, purposely made in the cold, is all the year fluid, and sold, or dispensed, not as consistent bodies, by weight, but as liquors, by measure. To strengthen this observation, I shall add, what was affirmed to me by a learned man, that practised physick in the warmer parts of *America*, namely, that he met in some places with several drugs, which, though they there seem to be balsoms, as turpentine, &c. are with us, and retained that consistence in those climates; yet, when they come into our colder regions, harden into gums, and continue such both winter and summer. On the other side, enquiring also of a traveller, versed in physical things, about the effects of great heat in the inland parts of *Africa*, where he had lately been; he told me, among other things, that raisin of jalap, which, when he carried it out of *England*, was of a consistence, not only dry, but brittle, did, when, and a while before, he came to *Morocco*, melt into a substance like turpentine; so that some of it, that he had made up into pills, would no more at all retain that shape, but remain, as it were, melted all the while he stayed in that city, and the neighbouring country, though when he came back to the borders of *Spain*, it returned to its former consistence. Which I the less wondered at, because, having had the curiosity to consider some parcels of gum lacca, (of which sealing-wax is made) newly brought ashore from the *East Indies*, though it be a hard and solid gum, yet I found by several instances, that, passing through the Torrid Zone, divers pieces of it, notwithstanding the shelter afforded it by the great ship it came in, had been, by the heat of the climate, melted, and made to stick together, though afterwards they regained their former consistence, though not altogether their former colour. And on this occasion I shall add, that I learned by enquiry from a particular acquaintance of mine, who brought me divers rarities out of *America*, that having at the place, where it was made, among other things, furnished himself with a quantity of the best aloes, he observed, that whilst he sailed through very hot climates, it was so soft, that, like liquid pitch, it would often have fallen out of the wide-mouthed vessel he kept it in, if he had not from time to time been careful to prevent it. But when he came within a hundred

leagues

leagues of the coast of *England*, it grew hard, and so continued, though this were in a very warm season of the year, being about the dog-days.

FOR further confirmation of what has been hitherto discoursed, be pleased to consider with me that most obvious body the air, or the atmosphere we live and breathe in. For though several opinions and argumentations are founded upon what their authors call the natural and preternatural or violent state of the air, yet he, that considers, shall find it no easy thing to determine, what state of the air ought to be reputed its truly natural state, unless in the sense I formerly told you I employ that expression in. I will not insist on the heat and coldness of the air; for that being manifestly very differing in the heart of winter, and in the heat of summer, and in differing regions of the air, as at the top and bottom of high mountains, at the same time, and constantly in differing regions of the earth, as in *Barbary* and *Greenland*, it will not be so easy to determine, what state is natural to the air. But that only, which I shall now consider, is its state or tone in reference to rarity and density. For since the air is believed to be condensed by cold, and expanded by heat, I demand, at what time of the year, and in what country, the air shall be reputed to be in its natural state? For if you name any one time, as the winter or the summer, I will ask, why that must be the standard of the tone of the air rather than another season, or at least exclusively to all others? And the like difficulty may be made about the climate or the place. And these scruples are the more allowable to be proposed, because learned men have delivered, that in some countries the mercury in the Torricellian experiment is kept higher than in others, (as in *Sweden* than in *Italy*,) and our baroscopes inform us, that oftentimes, in the same place and day, the quick-silver in the same instrument does considerably vary its height; which shews, that the air or atmosphere must necessarily vary its weight, and therefore probably its degree of rarity or density.

BUT I have yet to propose a further consideration in this affair: For what if it shall appear, that neither in winter nor in summer, in *Sweden* or in *Italy*, or in whatever country, region, or season you please, the air we breathe in is in any other than a preternatural state? nay, that even when we have vehemently agitated and expanded it by an intense heat of the fire, it is not yet violently rarified, but yet violently constipated, unless, in our sense before declared, you understand with me the preternatural state of rarefaction in the air, in reference to the tone it had before the last notable change was produced in it. This will, I question not, seem a surprizing, if not a wild, paradox: but yet to make it probable, I shall only desire you to reflect upon two or three of my physico-mechanical experiments; and there you will see, first, that the air being a body abounding with springy particles, not devoid of gravity, the inferior must be compressed by the weight of all the incumbent. And next, that this compression is so great, that though by the heat of the fire neither others, nor we, could bring a portion of included air to be expanded to above fourscore times its former space; yet without heat, by barely taking off the pressure of the superior air, by the help of our pneumatical engine, the air was rarified more than twice as much: and since those experiments were published, I more than once rarified it to above five hundred times its usual dimensions; so that, if, according to what is generally agreed on and taught, a body be then in a preternatural state, when, by an external force, it is kept in a condition, from which it incessantly tends to get free; and if it be then most near its natural state, when it has the most prosperously endeavoured to free it self from external force, and comply with its never ceasing tendency; if this be so, I say, then the air we live in is constantly in a preternatural state of compression by external force. And when it is most of all rarified by the fire, or by our engine, its spring having then far more conveniency than before to display themselves, which they continually tend to do, it answerably approaches to its natural state, which is to be yet less compressed or not at all. And I have carefully tried, for many months together, that when the air has been rarified much more than even a vehement heat will bring it to be, yet if it were fenced from the pressure of the external air, it would not shrink to its former dimensions, as if it had been put into a violent state, from whence nature would reduce it to them, but continued in that great and seemingly preternatural degree of extension, as long as I had occasion to observe it. One might here shew, that this odd constitution of the air is so expedient, if not necessary for the motion, respiration, and other uses of animals, and in particular of men, that the providence and goodness of the wise author of the universe is thereby signally declared; if it were not improper, in such a paper as this, to employ final causes. Wherefore, to avoid the imputation of impertinence, I will conclude, by taking notice, that, from what has been delivered, we may learn two things considerable enough, if not in themselves, yet to some passages of the treatise, whereof this paper makes a part. And first, we may deduce from what has been said of the air, that, according to what is noted above, that may sometimes generally be granted, and believed to be the natural state of a body, not which it really affects to be in, or (to speak more properly) has a tendency to attain, but that which it is brought into, and kept in by the action, or resistance of neighbouring bodies, or by such a concourse of agents and causes, as will not suffer it to pass into another state, And the second thing we may hence learn, is, that whatever men say of nature's never missing her aim, and that nothing violent is durable; yet, bating an inconsiderable portion of aerial particles at the upper surface, for aught we know, the whole mass of the air we live in, and which invirons the whole terraqueous globe, has been, from the world's beginning, and will be to its end, kept in a state of violent compression.

A STATICAL HYGROSCOPE

Propofed to be further Tried,

Together with

A BRIEF ACCOUNT

OF THE

UTILITIES of HYGROSCOPES.

A

STATICAL HYGROSCOPE, &c.

In a LETTER to H. OLDENBURG, Efq; Secretary to the ROYAL SOCIETY.

SIR,

THOUGH I writ to you from *Stanton*, an account of thofe hygrofcopes, whereof I now prefent you one; yet fince I remember, that it was in the year 1665, that I fent you that paper, I fear you may, by this time, have forgotten much of what it contained, and thereby made it fit for me, in this letter, both to remind you of fome former paffages, and to add fome obfervations, that lately occurred to me; and this the rather, becaufe I do not prefent you with this trifle, merely to gratify your curiofity, but that you, and fome of your ingenious friends, may, by your remarks, help me to difcover, to what inconveniences our inftrument is liable, how far they may be avoided, or leffened, or what the ufes, or advantages, of it may be, notwithftanding its inevitable inconveniences, or imperfections.

HAVING had occafion, amongft other fubjects relating to the air, to confider its moifture, and its drynefs, I eafily difcerned, that they had no fmall influence upon divers bodies, and, among the reft, upon thofe of men, as the ambient air we breath in, either paffes from one of thofe qualities to the other, or even from one degree to the other, in the fame quality.

WHEREFORE, I began to caft about, fomewhat follicitoufly, for a way, that might, better than any I had yet tried, or elfewhere met with, difcover the changes of the air, as to moifture and drynefs, and the degrees of either quality. For which purpofe, it feemed to me, that if a ftatical hygrofcope could be had, it would be very convenient, in regard of its fitnefs, both to determine the degrees of the moifture, or drynefs of the air, and to tranfmit the obfervations made of them to others. Whereupon, confidering further, that among bodies, otherwife well qualified for fuch a purpofe, that was likelieft to give the fenfibleft informations of the changes of the air, which, in refpect of its bulk, had the moft of its furface expofed thereunto; I quickly pitched upon a fine fpunge, as that, which is eafily portable, not eafy to be divided, or diffipated, which, by its readinefs to foak in water, feemed likely to imbibe the aqueous particles, that it may meet with difperfed in the air, and which, by its great poroufnefs throughout, has much more of fuperficies, in reference to its bulk, than any body, not otherwife lefs fit for the intended ufe, that came into my thoughts.

IF you recal to mind, when, and whence I firft gave you notice, that I employed our little inftrument, you will eafily believe, that the inducements I had to pitch upon it, were, that I fhould need but fuch light and parable things, as I could eafily both procure in the country (where I then was) and carry about with me, in the frequent removes I was obliged to make; and therefore, that I did not represent this trifle as the best hygroscope, that could be devised, or even as the best, that, perhaps, I myself could have propounded, if I would have framed an elaborate engine with wheels, springs, or equivalent weights, pullies, indices, and other contrivances, some of which I divers years ago made use of. For I little doubt, but that mechanical heads may frame hygroscopes much curiouser and perfecter, than that I now send you, or any other I have used, or seen, if they may be accommodated with sufficient room, and dextrous artificers, that will work exactly according to directions; whereas, my design being not so much to make a machinal, or engine-like, as a statical hygroscope, and such an one as may be simple, cheap, contained, and set up in a little room, easy to be made and transported, I thought it might be of some use, especially to those, that are not furnished with curiosities and mechanical accommodations, if, among the several forms of hygroscopes, that I had in my mind, I chose one, that being statical and easy, might be as commodious by its simplicity, as some others by their elaborateness; especially, if we consider, that, as slight an instrument as it seems, it may be applied to various uses, some of which are not slight, as will ere long be made probable.

If I should be here told by one, that grants the preferableness of statical hygroscopes in the general, that there are divers bodies, other than that pitched upon by me, whose weight may vary, when the temperature of the air is considerably altered, as to dryness and moisture, and that, perhaps, among these, some one may be found, that may imbibe the aqueous particles of the air better than our spunge; I shall not resolutely deny it, and therefore shall leave you to make trials with what other bodies you shall think fit, contenting myself to have suggested, in general, the conveniency of making hygroscopes, where the differing changes of the air may be estimated by weight: but this I shall tell you, in favour of our spunge, that when I was considering, what bodies were the fittest to be employed for the making of statical hygroscopes, I made trial of more than one, that seemed not the least promising. I know, that common, or sea-salt, will much relent in moist air, and salt of tartar will do it much more; but then those salts, especially the latter, will not so easily, as they should, part with the aqueous corpuscles they have once imbibed, and are in other regards, (which it were not worth while to insist on,) less convenient than a spunge. I made trial also with lute-strings, which were purposely chosen very slender, that they might have the greater surface, in respect of their bulk: these I found, at first, to do very well, as to the imbibing of the moisture of the air, but afterwards, they did not continue to answer my expectation. I caused likewise to be turned out of a light wood a cup, which, that it might less burden a tender balance, had, instead of a foot, a little button, to which a hair might be tied, to suspend it by; and this cup being purposely turned very thin, that it might have much surface exposed to the air, proved for a pretty while so good a hygroscope, as invited me to make divers observations with it, some of which I have still by me. It agreed also with several trials, that I had made on other occasions, of the porousness of such bodies, that white sheeps leather, such as surgeons used to spread plaisters upon, would be very convenient for my purpose. And indeed I found by many observations, whose success you may command a sight of, that if this leather were a substance as little obnoxious to corruption as a spunge, it would, by its copious imbibitions, and emissions of the aerial moisture, be a fitter matter, than any other I had employed for a hygroscope.

But taking all things together, I found no body so convenient for my purpose as a *spunge*; which you will, perhaps, the more easily believe, if I add, that to help me to make some estimate of the porosity of it, [we weighed out a drachm of fine spunge, and having suffered it to soak up what water it could, it was held in the air, not only whilst the weight of the water would easily make it run out, but till it dropped so very slowly, that a hundred was reckoned after one drop, before another fell; then putting it into the balance it had been weighed in before, we found, that as its dimensions were increased to the eye, so its weight was increased upon the scale, amounting now to somewhat above two ounces and two drachms; so that one drachm of spunge, though it seemed not altogether so fine as the portion we had chosen out for our hygroscopes, did imbibe and retain seventeen times its weight of water.]

Now, when one is resolved to employ a spunge, there will not need to be much added about the turning it into a hygroscope. For, having weighed it, when the air is of a moderate temperature, it requires but to be put into one of the scales of a good balance, suspended on a gibbet, as they call it, or some other fixed and stable supporter. For the spunge being carefully counterpoised, at first, with a metalline weight (because that alters not sensibly with the changes of the air) it will, by its decrement, or increase of weight, shew, how much the neighbouring air is grown drier, or moister, in the place, where the instrument is kept. The weight of the spunge may be greater, or less, according to the bigness and goodness of the balance, and the accurateness you desire in the discoveries it is to make you. For my part, though I have, for curiosity's sake, with very tender scales employed, for a good while, but half a drachm of spunge, and I found it to answer my expectation well enough; and though, when I used a bulk divers times as great, in a stronger, but proportionably less accurate balance, I found not the experiment successless; yet, after trials with differing quantities of spunge, I preferred, both to a greater and lesser weight, that of a drachm, as not being heavy enough to overburden the finer sort of goldsmiths scales, and yet great enough to discover changes considerably minute, since they would turn, discernibly, with a sixteenth, or twentieth part, and manifestly, with half a quarter of a grain.

WITH such hygroscopes as these (wherein the balance ought to be still kept suspended and charged) I made several trials, as my removes and accommodations would permit, sometimes in the spring, and sometimes in the autumn, and sometimes also in the summer and winter. But nevertheless, it would be very welcome to me, if you, and some of your friends, would be pleased to make trials yourselves, and compare them with mine, and especially take notice, if you can, whether, in any reasonable tract of time, there will be any loss, worth noting, of the substance of the spunge itself; I having not hitherto discovered any. In the mean time, to invite you to give yourselves this trouble, after I have told you, that having once, among divers removes, had the opportunity to keep a drachm of spunge suspended during a whole spring, and a great part of the preceding winter, and subsequent summer, I did not think my pains lost, though divers of the observations they afforded me have unhappily been so, among many other memorials about experiments of differing kinds; notwithstanding which unseasonable loss, I shall venture to suggest some things to you, that occurred to me about the utilities of the instruments I am treating of.

A

BRIEF ACCOUNT

OF THE

UTILITIES of HYGROSCOPES.

THE use of a hygroscope is either general, or particular; the former is almost coincident with the qualifications to be wished for, and aimed at, in the instrument itself; the latter points out the particular applications, that may be made of it, when it is duly qualified. Of each of these, I shall briefly subjoin what readily occurs to me.

The general use of a hygroscope is, "To "estimate the changes of the air, as to moi-"sture, and dryness, by ways of measuring "them, easy to be known, provided, and "communicated."

I might here pretend, that as these are the principal things, that have been desired in hygroscopes, so it is obvious, from the description and account we have given of our instrument, that these advantages belong to it in no very despicable degree: and that to make such hygroscopes, as will perform all these things in perfection, whatever it may seem to a mental contriver, will, I fear, prove no easy task to those, that really attempt it. To these things I might add, that if such allowances be made, as what I have represented may invite you to grant, the qualifications lately mentioned, as desirable in a hygroscope, may, in a tolerable measure, be found in ours, when we shall come to mention the particular uses of it. And as for that of conveying to others the observations made with it, you may please to consider, that the things I employ to measure the degrees of dryness and moisture in the air, being grains, parts of grains, and greater weights, the accessions of moisture, which the spunge receives, or the losses, that it suffers, can be easily, and at the same time, both found and determined. And as the weights employed to determine these differences are easily procurable; so the observations made with them may (together with patterns, if it should be needful, of the weights themselves) with the same facility be communicated by letters even to remote parts. In which conveniency, whether, and how far, our instrument has the advantage of that made with an oaten beard, and some others, that I have employed, I leave you to consider.

I might farther alledge on the behalf of our instrument, that whereas, besides the qualifications above-mentioned, there is another, namely, durableness, which, though not so necessary to constitute a hygroscope, yet is necessary, as will ere long appear, to some of the considerablest uses of it: and whereas such a durableness is wished, as may not only keep the instrument from having its substance rotted, or corrupted by the air, but may also preserve it in a capacity to continue pretty uniformly its informations of the air's moisture, even when that increases very much, or lasts very long; whereas, I say, these things are much desired in a hygroscope, our spunge seems herein preferable to the oaten beard, lute-strings, &c. For in those, and the like bodies, the self-contracting, or relaxing power, (as it is supposed) or the disposition to imbibe, and part with the moisture of the air uniformly, or after a due manner, is wont to be in no very

very long time altered, or impaired; and particularly, when they have imbibed much aerial moisture, they are very faintly affected by the supervening degrees of it, and so the operation is too disproportionate to what the like cause would have produced, when the instrument was well disposed; whereas, in our spunge, neither the degree of springiness, nor any such-like quality is considered, and it is capable of imbibing so much more of the aqueous particles, than even moist airs and seasons are wont to supply it with, that there is little fear, that it will be glutted, or have its pores choked up with them, so that the decrements, and accessions of weight, will be more proportionate to the degree of moisture in the air, and more reducible to known and determinate measures.

BUT though these, and the like specious things, may be represented in favour of our statical hygroscope; yet, to deal ingenuously with you, I much fear, that it will be very difficult to bring either statical ones, or perhaps any other, to be so complete, as to satisfy a nice and severe critick, And you would perhaps easily assent to my opinion, if it were not too tedious to entertain you with all the speculative doubts and scruples, as well mechanical as physical, which my accustomed diffidence has now and then suggested to me. But because such a sceptical discourse would be too tedious, and also somewhat improper, to be proposed by one, that would recommend hygroscopes, I shall only now take notice of one great imperfection, which all, that I have been acquainted with, are liable to; namely, that men have not yet found, nor perhaps so much as dreamed of seeking a standard of the dryness and moisture of the air, by relation to which hygrometers may at first be adjusted, and so be compared with one another, as we see many of those sealed thermoscopes, that have been made and justned by Mr. *Shotgrave*, the dextrous operator of the Royal Society. I deny not, that, by virtue of a standard to estimate moisture by, I have endeavoured to remedy this inconvenience; but, as my hopes were but small, so neither was my success great, but I am not sure, that happier wits, or I myself, at some other and luckier time, may not more prosperously attempt it. In the mean while, perchance, you will not think it altogether nothing, if the trifle I present you, perform, at least, some of the things desired in a hygrometer less imperfectly, than any you have yet met with. And that you may not be discouraged by what I have lately acknowledged of the defects of such instruments, I think it now seasonable to proceed to the mention of the particular uses, for which, notwithstanding any inevitable defects, a hygroscope, and even such a one as I now present you, may be made easily to serve.

USE I.

To know the differing variations of weather in the same month, day and hour.

IT may be useful for divers purposes, to know, both that the air is wont to be less moist at one part of the artificial day (and so of the night,) than at any other, and at what particular time of the day or night it most usually is so. And on this occasion I remember, that usually, when the weather was at a stand, it was observed, that the spunge had manifestly gained in the night, though it were kept in a bed-chamber, and grew lighter again between the morning and noon. This observation, which was made towards the end of winter, would not hold, in case frosty nights or some other powerful cause intervened. It were not amiss also to observe, whether there be not a correspondence betwixt the hygroscope and baroscope; and if there be, in what kind of weather or constitution of air it is most or least to be discerned. And this enquiry seems the more dubious, because the same changes of the atmosphere may, upon differing accounts, have either the like, or quite contrary operations upon these two instruments. For in summer, when the atmosphere is usually heavier, the hygroscope is usually lighter; some strong winds, as with us the north-west, may make both the atmosphere and baroscope lighter, whereas southerly winds, especially if accompanied with rain, often make the atmosphere lighter and the spunge heavier. And on the other side I observe, that easterly winds, especially when they begin to blow in winter, though, by reason of their dryness, they are wont to make the hygroscope lighter, yet they are wont, at least here at the west-end of *London*, to make the baroscope shew the air to be heavier. It were likewise fit to be observed, particularly by those, that live on the sea-coast, whether the daily ebbing or flowing of the sea do not sensibly alter the weight of the hygroscope. It were very well worth while also to take notice, at what time of the day or night, *cæteris paribus*, the air is the most damp and most dry, and not only in several parts of the same day, but in several days of the same month; especially on those days, wherein the full and new moons happen. And this seems a more hopeful way of discovering, whether the full moon diffuses a moisture in the air, than those vulgar traditions of the plumpness of oysters and shell-fish, and brains in the heads of some animals, and of marrow in their bones, and divers other phænomena, which, as I have shewn in another paper, it is not easy to be sure of. It may also be noted, whether monthly spring-tides, especially when they fall out near the middle of *March* or *September*, have any sensible operation upon our instrument.

USE II.

To know, how much one year and season is dryer or moister than another.

THIS cannot be so well performed by the hygroscope made of an oaten beard, if they, that have made use of them more than I, do complain with reason, that after some months (for I cannot tell you precisely how many) they begin to dry up and shrink; so that their sense of the varying degrees of the moisture

moisture of the air is not so quick as before, and the informations they give of the degrees of it, especially towards the outmost bounds of their power, to shew the air's alterations, recede more and more from uniformity. But the lastingness and other convenient qualifications of our spunge making its capacity of doing service more durable, may the better help us to compare the greatest moisture and dryness, both of the same season, and of the seasons of one year with the correspondent ones of another. And if the weight of the spunge at a convenient time, when the temperature of the air is neither considerably moist, nor considerably dry, be taken for a standard, a person, that should think it worth his pains, may, by computing how many days at such an hour, and how much at that hour, it was heavier or lighter than the standard, and also by comparing the result of such an account in one year with the result of the like account in another year, be assisted to make a more particular and near estimate of the differing temperature of the air, as to moisture and dryness, in one year than in another, and in any correspondent season or month, assigned in each of the two years proposed. And how much the collation, or continuance of such observations, both in the same place, and also in differing countries and climates, may be of use to physicians in reference to those diseases, where the moisture and dryness of the air has much interest; and the husbandman to foresee what seasons will prove friendly or unkind to such and such soils and vegetables; it must be the work of time to teach us, though in the mean while we have no reason to despair, that the uses to be made of such observations may prove considerable. And the rather, because if by help of the result of many observations men be enabled to foresee (though at no great distance off) the temperature of a year, or even of a season, it may advantage not only physicians and plowmen, but other professions of men, who receive much profit or prejudice by the dryness or excessive moisture of the seasons. And not to mention those, who cultivate hops, saffron, and other plants, that are tender and bear a great price; such a foresight, as we are speaking of, may be of great use to shepherds, who, in divers parts of *England*, are oftentimes much damnified, if not quite undone, by the rot of sheep, which usually happens through excess of moisture in certain months of the year. And in order to the providing of foundations, whereupon to build predictions, it may not be amiss to register the number, bigness, and duration of the considerabler spots, that may at this or that time of the year happen to appear, or be dissipated on or near the sun, or to take notice of any extraordinary absence of them, and to observe, whether their apparition, or dissipation, produce any changes in the hygroscope: which curiosity I should not venture to propose, but that (as I elsewhere note) I find, that eminent astronomers have casually observed great drynesses to attend the extraordinary absence or fewness of the solar spots. And those persons, that are astrologically given, may, if they please, extend their curiosity in the use of this instrument to observe, whether eclipses of the sun and moon, and the great conjunctions of the superior planets, have any notable operation upon it.

USE III.

To discover and compare the changes of the temperature of the air made by winds, strong or weak; frosty, snowy, and other weather.

THIS may conveniently enough be done, as to winds, either by our whole instruments, or (perhaps better and more safely) by the spunge alone, which may be taken off and hung by a string, for as long time, as is thought fit, in the wind, and then restored to its former place. For I found by removing it into the wind, that it soon received a very considerable alteration in point of weight, as also it did, when removed out of a room into a garden where the sun shined; for though the season were not warm, it being then the month of *January*; yet in three quarters of an hour the spunge lost the 24th part of its weight. We may also in some cases usefully substitute to a spunge a somewhat broad piece of good sheeps-leather displayed to the wind. For this having, by reason of its thinness (or very small depth,) in proportion to its breadth, a very large superficies immediately exposed to the wind, we found it to be notably altered thereby, in so much, that half an ounce of well prepared sheeps-leather, (that we had long employed as an hygroscope) being kept an hour in a place, where the sun-beams might not beat upon it, did, in a strong wind, vary in that short time an eighteenth part of its original weight. But though I think it very possible to make such observations of the temperature of particular winds, as will frequently enough prove so true as to be useful, at least to those, that live in the places where they are made; yet I am of opinion, that, to be able to settle rules any thing general, to determine with any certainty the qualities of winds, according to the corners, whence they blow, as from the east or west, north-east, south-west, &c. there will be a great deal of wariness required; and he, that has not some competent skill in physicks and cosmography, will easily be subject to mistakes in forming his rules. To countenance which advertisement, I shall now make use but of these two considerations, whereof the first is; that winds, that blow from the same quarter, are not in some countries of the same quality, that they are in most others, the wind participating much of the nature of the region, over which it blows in its passage to us. At the famous port of *Archangel* they observe, that whereas a northerly wind, almost every where else without the tropicks, produces frost in winter, there it is wont to be attended with a thaw, so as to make the eves to drop. Of which the reason seems to be, that this wind comes over the sea, which lies north from that place; and on the contrary, a southerly wind, blowing over a thousand or twelve hundred miles of frozen land, does rather increaſe the froſt than bring a thaw. This was by the inhabitants averred to the Ruſſian emperor's phyſician, who was more than once at *Archangel*, and from whom I had the account. The northern winds, that are elſewhere wont to be drying, are ſaid in *Egypt* to be moiſt. I remember Mr. *Sandys*, in his excellent Travels, giving an account of what he obſerved about the largeſt of the famed Egyptian pyramids, has this conſiderable paſſage; "Yet this hath been too great a morſel for "time to devour, having ſtood, as may be "probably conjectured, about three thouſand "and two hundred years, and now rather old "than ruinous: yet the north ſide moſt worn, "by reaſon of the humidity of the northern "wind, which here is the moiſteſt." *Sandys* in *Purchas*'s Pilgrimage. And it is yet more conſiderable to our purpoſe what I find related by monſieur *de Serres*, in his uſeful book of huſbandry, ſince by that it appears, that even in not very diſtant provinces of the ſame kingdom, the winds, that blow from the ſame quarter, may have very differing qualities and effects. For, ſpeaking of the changes of the air, in reference to huſbandry, in ſeveral parts of *France*, he informs us, that it is obſerved, that in the quarters about *Tholouſe* the ſouth-wind dies the ground, and the north gives rains. Whereas on the contrary, from *Narbonne* to *Lyons*, all over *Provence* and *Dauphiné*, this laſt named wind cauſes dryneſs, and the other brings moiſture. And this may ſuffice for my firſt conſideration. My ſecond is this, that the vehemence or the faintneſs of the winds, though blowing over the ſame country, may much diverſify its operation on the hygroſcope; and the ſame wind, which, when it blows but faintly, or even moderately, is wont to appear moiſt by the hygroſcope, may, when vehement or impetuous, make the inſtrument grow lighter, diſcuſſing and driving away more vapours by the agitation of parts it makes in the ſpunge, than is countervailed by thoſe aqueous vapours, that are brought along with it. But on ſuch things as theſe I have not leiſure to inſiſt, and therefore I ſhall proceed to take notice, in very few words, of ſome other operations of differing weathers on our inſtrument, and tell you, that froſty weather often made the hygroſcope grow lighter even at night: ſnowy weather, which laſted not long, added ſomething to the weight of the ſpunge. And it has been obſerved, that miſts and foggy weather uſed to add weight to it, even notwithſtanding froſt.

Lib. VI. cap. 8. Sect. 3.

Theat. d'Agricult. lib. I. cap. 7.

To which may be added an obſervation made by my amanuenſis, who having a convenienter chamber than mine, (wherein a fire was daily made,) was diligent and curious to ſet down the changes of the hygroſcope, that was left in his lodging; for this obſervation makes it probable, that a tranſient cloud in fair weather may be (for I ſay not, that it always is) manifeſtly obſervable by our inſtrument. For by his diary it appears, that the ninth of *September*, being for the moſt part a very fair ſun-ſhiny day, though about ten a clock in the morning the ſun ſhone brightly, the ſpunge began to preponderate; which unexpected phænomenon made him look out at the window, where he diſcovered a cloud, that darkened the ſun, but after a while, that being paſt, the balance returned to an æquilibrium. On this occaſion I ſhall intimate, that I have more than once or twice obſerved, eſpecially in ſummer, that when the air grew heavier, the hygroſcope either continued at a ſtand, or perhaps, alſo grew lighter; as if, when ſuch caſes happen, the effluvia, that get into the air, either from the terreſtrial, or ſome other mundane globe, were not fit, like vapours, to enter and lodge in the pores of the ſpunge, and ſo were corpuſcles of another nature, with which, when we find by the baroſcope, that the air is plentifully ſtocked, it may be worth while to obſerve, whether any, and if any, what kind of meteor, as wind, or rain itſelf, or hail, or in the winter ſnow or froſt, will commonly be ſignified and produced.

USE IV.

To compare the temperature of differing houſes, and differing rooms in the ſame houſe.

As this is of great uſe, both in reſpect of mens health, eſpecially if they be of a tender, or ſickly conſtitution, and in reſpect of conveniency for the keeping fleſh, ſweat-meats, and ſeveral ſorts of wares and goods, and even houſhold-ſtuff, that are ſubject to be indamaged by moiſt air; ſo it is readily and manifeſtly derivable from our inſtrument. For, by removing it into ſeveral houſes, or into ſeveral parts of the ſame houſe, and letting it ſtand in each a competent time, to be affected with the temperature of the air of that particular place, we have divers times obſerved a notable difference, as you may gueſs by the two or three notes I met with among ſome old papers.

October 13. [THREE or four days ago, a piece of fine ſpunge being taken out of a cabinet, and clipped, till it came to weigh juſt half a drachm in a nice pair of ſcales, and a warm room, was afterwards removed into a neighbouring room deſtitute of a chimney, (and yet within three or four yards of a chimney ſeldom without fire:) this ſtatical hygroſcope, conſiſting of the ſcales and the frame they hung on, was yeſterday night removed into the former room, and the ſpunge was found to have gained three grains and $\frac{1}{6}$, or better, and conſequently more than a tenth part, in reference to its firſt weight; but being ſuffered to ſtand in this warm room, in leſs than twelve hours, it loſt a grain and about $\frac{1}{8}$ of its former weight, though the time it ſtood in this room were, for the moſt part, night and rainy weather.]

[WE took a piece of very fine ſpunge, which formerly had weighed juſt a drachm, but having been many months kept in a very warm room, where fires were kept every day, it was grown much lighter; for, removing it into an upper chamber in a neighbouring houſe, and weighing it in tender ſcales, in the evening it was found to want of a drachm, four grains, and $\frac{1}{4}$ of a grain; and though there

there was a fire in the room, and the scales stood not far from it, yet, in a short time, (the day being foggy and rainy,) the spunge visibly depressed its scale ¼, and the next morning was found to want but one grain and a half of a drachm, so that it had gained about three grains and a quarter, and the following evening, being the second of *January*, it weighed one drachm, a grain, and almost half a grain. So that in about one natural day, the spunge had acquired six grains from the moisture of the air, that is, a tenth part of its first weight (I mean a drachm) and a greater proportion in reference to the weight it had the day before. The third of *January*, the weather being yet moist, the weight exceeded two grains, but about three or four of the clock in the afternoon, it began to lose of that great weight, which diminished more by the next morning, the weather having changed that night, and become somewhat frosty.]

IN another paper I also find this note. [The drachm of a spunge, that had for divers weeks been kept in a dry room, was (*January* the tenth) carried out into a room, where fire is not wont to be kept, the weather being extraordinarily foggy; this morning, being brought into the former room, though now the weather be clear (yet not frosty) it appears to have gained in weight about eleven grains; yet it soon lost two grains by standing in this room all the while in the balance.]

USE V.

To observe in a chamber the effects of the presence, or absence of a fire in a chimney or stove.

THIS is easily done, and the more easily, if the room be small. For in such chambers, I have often observed a moderate fire to alter the weight of the instrument, placed at a distance from it, after it had been well kindled but a very little while; but in wet weather, if the fire were not seasonably renewed with fresh fuel, the decay of it would, in no long time, begin to be discernible by the instrument.

USE VI.

To keep a chamber at the same degree, or at an assigned degree of dryness.

SUPPOSING the alteration of weight in our spunge to depend only upon the degree of the moisture of the air, the last named use will be but an obvious corollary of the former. For, if a convenient part of the room be chosen for the hygroscope, and it be kept constantly there, it is easy, by casting one's eye on it from time to time, to perceive, when it will be requisite to encrease or moderate the fire, so as to keep the spunge at that weight it was of, when the temperature of the air of the chamber, as to dryness and moisture, was such as was desired. I will not trouble you with some scruples, which, I confess, the consideration of this use of our instrument suggested to me, because I have not now the leisure to discuss them. I had thoughts to try, whether, and how far a good quantity of salt of tartar, or even dried sea-salt, being kept in a closet, or some closer room, might by imbibing lessen the moisture of the air in it, but I did not perfect any observation of this kind. But I will add to what I have already referred to this sixth head, that I have sometimes noted with pleasure, how manifest and great a change in the weight of our spunge would be made, when the room was washed, and a good while after, notwithstanding that a good fire was kept in it, to hasten the drying of it.

BESIDES the hitherto mentioned uses of our hygroscope, I know not, whether there may not be divers others, and whether we may not, by a little altering and helping it, make it capable of shewing us some difference betwixt steams of differing natures, as those of water, spirit of wine, chemical oils, and perhaps new kinds of substances (such as we have not yet taken notice of) in the air, in which I confess, I suspect there may sometimes be dispersed store of corpuscles, that I do not yet well know what to think of. For I have more than once observed (not without some wonder) the hygroscope not to be affected with the alteration of weather, answerably to what the manifest constitutions, or variations of it seem plainly to require: whether unobserved corpuscles performed this, by making the other steams in point of figure, or size, incongruous to the minute pores of the spunge, and so unfit to enter them; or by dissipating, or otherwise procuring the avolation of more of the watery particles than they could countervail, I now examine not. And I am not sure, but by associating this instrument with the thermoscope, baroscope, and some others, that may be proposed, it might be so improved, as to help us to foresee divers considerable things, that either are themselves changes of the air, or are wont to be consequences of them: as sickly and healthful constitutions of the air, both as to man and cattle; and healthful, barren, or plentiful seasons in particular places or countries; and perhaps also strong hurricanes, earthquakes, inundations, and their ill effects, especially those accidents, that depend much upon the surcharge of the air, with other exhalations and moist vapours, which operate before sensibly upon our instrument, and therefore may be discernible by it a good while before they arrive at that height, that makes them formidable meteors. And if it were but the foretelling approaching rain, this very thing may, on divers occasions, prove very serviceable, and recommend our instrument, which often receives much earlier impressions from the steams, that swim up and down in the air, than our senses do; so that I have been able to foresee a shower of rain, especially in dry weather, a not inconsiderable while before it fell.

AND here I should dismiss our subject, which I have already dwelt on longer than I designed, but that remembering a caution I gave you, when I was speaking of winds, I think it but fit to add two or three lines, to keep you from being by that advertisement discouraged from endeavouring See USE III.

endeavouring to make, in the general, such hygroscopical observations, as may be reduced to hypotheses. For as I elsewhere discoursed concerning barometrical theories, if I may so call them; so I shall here represent, concerning hygroscopical ones, that if a theory or hypothesis, that is itself rational, be found agreable to what happens the most usually in observation; it ought not lightly to be rejected, or so much as laid aside, though sometimes we find particular instances, that seem to call it in question. For it is very possible, that the theory or hypothesis may be as good, as a wise man would require about so mutable a subject as the weather. And the cause assigned by the hypothesis may really act suitably to what that requires, though a contrary effect ensue by reason of that cause's being accidentally mastered and over-ruled by some more powerful cause or agent, that happens for that time to invade the air. As we know, that tides do for the main correspond with the motions of the moon, (whose phases are therefore argued from them,) and do generally ebb and flow at such times, and in such measures, as the theory, that has been grounded on that correspondency, requires; but yet seamen find, that in this, or that particular harbour, or mouth of a river, fierce contrary winds, great land-floods, and other casually intervening causes, do sometimes both very much disturb the regular course of the tides, and encrease or lessen them.

A

NEW EXPERIMENT

AND

OTHER INSTANCES

OF THE

EFFICACY of the AIR's MOISTURE.

Subjoined by Way of

APPENDIX to his STATICAL HYGROSCOPE.

SINCE it may probably recommend hygroscopes to you, if that quality of the air, which these instruments are useful to give us an account of, be made appear to be more powerful, and have considerabler effects, than is commonly believed; it will not be from my purpose to present you here some instances, that have led me to think, that the effects of the moisture of the air may be considerable, not only upon men's healths, but upon subjects far less tender, and less curiously contrived, than human bodies. But I hope you will easily believe, that by the moisture of the air, I mean not a mere and abstracted quality, but moist air itself, or rather those humid corpuscles, (chiefly of an aqueous nature,) that abound, and rove to and fro, in our common air.

THAT the moisture of the air may have no small influence, and usually a bad one, upon men's healths, is that, which, though experience did not so often teach us, I should venture to argue from what I have observed of the operation of moist air upon the dry and firmly contex parts of animal, and even in those cases, where, for want of time, or other impediments, this moisture cannot produce any sensible degree of putrefaction.

THAT the skins of animals may be easily invaded by the moist particles of the air, is the more probable, because of the numerousness of their pores, which may be concluded from their hairiness, or their sweat, or both. And I formerly observed to you, that I found sheeps-leather to imbibe the moisture of the air, and encrease in weight upon it, as plentifully as almost any body I exposed to it.

BUT to shew you, that much closer membranes, and which nature made to be impervious to such a liquor as urine itself, may be affected by the vapours of the air, I shall add, that having purposely taken pieces of bladders, fine and well blown, and, as far as appeared, of a very close contexture, and counterpoised them in a good balance, I found, according to expectation, that they would considerably encrease their weight in moist, and loose it again in dry weather; so that I might have employed the most membranous part of a bladder (for I thought not fit to make use of the neck or the adjoining part) to make a statical hygroscope.

AND as for other membranes and fibres, I ſhall have by and by occaſion to take notice, that even when they are ſtrongly and artificially wreathed together into gut-ſtrings, they may imbibe enough of the moiſture of the air to be broken by it. And I remember, I formerly told you, that I had obſerved lute-ſtrings to grow heavier in moiſt air.

AND whereas bones are by all confeſſed to be the firmeſt and ſolideſt parts of animals, and as it were, the pillars, by which the fabrick is ſuſtained; yet it ſeems, that even they may be pierced into, and ſenſibly affected by the moiſture of the air. For I remember, that having cauſed the ſkeleton of a human body to be ſo made by a famous and very ſkilful artiſt, that, by the help only of ſlender wires artificially ordered, the motions, which the muſcles make of the bones of a living body, might be well imitated in the ſkeleton, I obſerved, that though in dry and fair weather the flexures of the limbs might be readily made, yet in very moiſt weather the joints were not eaſily bent, as if the parts were grown ſtiff and rigid; which ſeemed to proceed hence, that moiſt particles of the air, having plentifully inſinuated themſelves at the pores into the bones, had every way diſtended them, and thereby made the parts bear hard againſt one another, (which they did not at all before) at the junctures or articulations.

BUT it will be the more readily believed, that the moiſture of the air may operate conſiderably upon the tender and curiouſly contrived bodies of men and other animals, if, proceeding to the obſervations I chiefly deſign, I make it appear, that the moiſtening particles, that rove up and down in the air, are able to exerciſe a notable (and, if I may ſo call it, a mechanical) force, even upon inanimate and inorganical bodies: which may well ſuggeſt a ſuſpicion, that hygroſcopes being the proper inſtruments to diſcover a quality in the air, whoſe efficacy reaches farther than is commonly taken notice of, they may in time be found uſeful to divers other purpoſes, beſides thoſe, that relate to the health of men.

THAT wood, eſpecially when it has been ſeaſoned, is a ſolid of a ſtrong and firm contexture, if it were not obvious by the daily uſe made of it in building ſhips, houſes, &c. might be eaſily concluded from the weight or force required to alter its contexture, by making any conſiderable, or, perhaps, ſenſible compreſſion of it. And yet that wood may ſuffer a kind of divulſion of the multitude of its parts, and be manifeſtly diſtended by aqueous corpuſples getting into its pores, I remember, I proved by this experiment. I got a piece of ſound and ſeaſoned wood of about an inch (or an inch and half) in diameter, to be by a ſkilful artiſt made cylindrical, and alſo a ring of ſome ſolid matter, as braſs or ivory, to be exactly turned to fit this cylinder, ſo that it might, without much eaſe, or much difficulty, be put on and taken off again: then we put the turned piece of wood into fair water, and left it to ſoak there for many hours; at the end of which it was viſibly ſwelled: and though I cannot now tell you, (for want of a paper concerning that experiment,) how much it was encreaſed in diameter, yet I well remember, the increment was conſiderable, and that the ring, that was adjuſted to it before, was manifeſtly too little to be put again upon it, or with its orifice to cover the whole baſis of the diſtended cylinder, which afterwards being dried in the air ſhrunk into a capacity of entering the ring again. And in this experiment I took notice, that the great intumeſcence of the wood was not produced all at once, or ſoon after it was put into water, but it ſwelled by degrees, and lay ſoking there many hours, before it arrived at its utmoſt diſtention, the aqueous corpuſcles requiring, it ſeems, ſo much time to inſinuate themſelves ſufficiently into the wood; which argues, that the internal parts were likewiſe affected, though, when even they came to ſwell, they had a good thickneſs of wood about them to hinder their dilatation.

I expect you ſhould now tell me, that this diſtention of ſo firm a body was made by water it ſelf, and not by the humid vapours of the air. On which occaſion I might repreſent to you, that by the ſweating (as men commonly call the adheſion of wateriſh drops to the ſurface) of poliſhed marble and ſome other cold and ſmooth bodies, that ſometimes happens even in the heat of ſummer, if they be cold, and the ambient air be moiſt enough, it appears, that both in hot weather the air may be plentifully ſtocked with aqueous vapours, and that theſe vapours need to do no more, than convene together, to conſtitute viſible and tangible water. And on this occaſion, if I were ſure I had not told you of it already, I ſhould ſubjoin an experiment, which would detect the vulgar error of thoſe, that think the adhering drops, lately mentioned, to ſome come from internal moiſture derived by its preſſion, or percolation, from the marble or the other body they are faſtened to; and at the ſame time I ſhall ſhew (what is not wont to be imagined) that in the heat of ſummer the air is furniſhed with inviſible and yet aqueous ſteams. The experiment I long ſince tried in winter with ſnow and ſalt, included in a glaſs veſſel, and then put to diſſolve in a balance. But becauſe neither ice nor ſnow is at all eaſy to be come by among us in *England* in ſummer; and becauſe at that ſeaſon, the air in fair weather is preſumed to be dry as well as hot, I choſe, within ſome days of Midſummer, and in clear ſun-ſhiny weather, to make the following trial.

WE took a pint glaſs-bottle, and having put into it a convenient quantity of water (for room muſt be left for the ſalt) we placed them and four ounces of beaten ſal armoniack in one ſcale of a good balance, and a counterpoiſe in the other; and then, putting the ſalt into the water, I obſerved, that tho' for a while, the æquilibrium remained, yet when the frigorific mixture had ſufficiently cooled the outſide of the bottle, the roving vapours of the air, that chanced to paſs along the ſurface of the veſſel, were, by the contact of that cold body, arreſted, and turned into a kind of a dew, which from time to time made the ſcale, that held the glaſs, preponderate more and more, and at length the drops growing greater and greater, ran down in ſmall rivulets the ſides

sides of the glass, and in less than an hour, by my estimate, the condensed steams amounted to near a drachm, which weight was afterwards much encreased within about two hours more: whereby it sufficiently appears, both that this dew came from without, (since if it had been a transudation, it would not have added weight to the scale, that received it,) and that there is, even in clear summer weather, a vast number of moist particles dispersed through the air, since in about an hour's time, such a multitude of them, as the liquor produced, may be supposed to consist of, and may by heat be actually resolved into, could in course come to touch so small a surface, as that of that part of so small a bottle, which contained the frigorific mixture. For the rest of the vessels surface was not cold enough to condense the vapours into liquor. But to return to what we were saying of wood swelled by water; because, notwithstanding these considerations, I am willing to allow, that the experiment of the cylinder does not fully come home to our purpose, and that I produced it not so much to prove directly the force of moist air, as to countenance what I am about to say, by shewing, what a sufficient number of aqueous corpuscles may do in the solid wood they penetrate, I shall now add some instances of the force these particles may exercise upon solids, when they invade them but in the form of vapours.

THAT in this form the multitude, figures, and motions of these insinuating particles may enable them to display no small force in their operations on some bodies, we have one instance, that often happens, though but seldom reflected on, in the breaking of the strings of musical instruments, first brought to a good tension, upon the supervening of rainy weather. For the cause seems to be, that the vapours, that then wander through the air, insinuating themselves into these strings, (which the musician often forgets to let down or relax after having skrewed them up,) distend and swell them, and thereby endeavour to shorten them, and that so forcibly, that they not seldom break with a smart noise and great violence; which, because it happens without any visible efficient, men commonly think and say, that such strings break of themselves. But to take no farther notice of this popular surmize, if we consider, how much weight some of those bigger strings, especially of base viols, that have been observed to break in rainy weather, will require to stretch any of them to a rupture, you will easily be induced to think, that this operation of the moist air exacts, and therefore argues more than a languid force.

BUT here, probably, you will tell me, that the instances you expected were concerning wood, which is a far solider body than gut-strings. To this I say, that the newly-recited instance belongs directly to the title of this paper, and, being above referred to, ought not to be pretermitted. And as to your expecting instances concerning wood, I might content myself to refer you to what is observed about the uneasy opening and shutting some doors well adjusted to the door-case in very rainy weather. But though this observation favours my design, yet I had rather give you instances in wood, purposely and carefully seasoned. And therefore I shall now inform you of these two things; one, that I found by trial, as I have elsewhere noted, that wood, counterpoised in a good balance, would grow sensibly heavier in wet weather, and lighter again in dry; and the other, that, to satisfy myself yet further, I consulted an ancient musician, to whom I had once been a disciple, and a famous organ-maker, to know, whether they had not observed, that the wood itself, &c. of musical instruments, would receive such alterations from the moisture of the air, as might be discerned by the ear? upon which enquiries, the master of musick answered me, that though metalline strings will not change with the weather, like gut-strings; yet virginals (for instance) though furnished with wire-strings, will, for the most part of them, (for some he has observed to be so well seasoned, that they are not altered by the weather,) be out of tune in wet weather, the strings generally then affording their notes sharper than they should, or are wont to do. And the organ-maker confessed to me, that, upon great changes of weather, divers organs would (after they had been long ago tuned) grow out of tune, and that not only the wooden pipes would be thereby swelled, but the metalline pipes untuned.

BUT if bodies be of such a constitution, as not only to admit, but assist the operation of the moist air, the penetrancy and efficacy of this may be found much more considerable than in the fore-going instances. For there are some kinds of those marchasites, that yield vitriol, which, whilst they lie under ground, or are covered with the sea-water, on whose shores they are, in some places, to be found, retain a stone-like hardness, and are often taken for meer stones; and yet some credible persons, that are conversant about vitriol, have casually observed, that these, being exposed to the air, would, in tract of time, be so penetrated by the moist particles of it, though perhaps not meerly as moist, that (probably by the help of the vitriolate corpuscles they met with among the stony matter) these hard and solid marchasites are brought to swell so much as to burst. That this will happen to such kind of stones (though they be of a close and heavy nature) by the help of rain, experience has persuaded me; and that it may also happen even to very hard and stone-like marchasites, (for many are not such,) when they are meerly exposed to the air, I am apt to think upon some trials of my own. For from shining marchasites, though but kept in my chamber window, I have had vitriolate efflorescencies, that seemed to be produced by the action of the piercing moisture of the air upon the mineral. And I remember, that very hard and heavy lumps, that were of a marchasitical substance, though not at all glistering, which seemed to be stony, were so disposed to be wrought on by the air, that though they were kept partly in my own chamber, and partly in other covered places, yet in no very long time they were so penetrated by the moist cor-

corpuscles of the air, that they were not only burst, but broken into many pieces; insomuch that many of them did of themselves fall off from one another, and several of the divided portions would easily be crumbled betwixt one's fingers. And of some of these I have observed with pleasure, that a vitriolate substance was produced more copiously in their innermost parts, than on, or near their outside. So that, when I considered how great, an external force would have been requisite to make such a comminution of minerals so solid and hard, it was obvious for me to look upon the air's moisture, as capable, when it meets with fitly-disposed bodies, to exercise a far greater force, than is wont to be conceived.

To these phænomena I might add some others to the same purpose; but because the marchasites, and other bodies, required to the producing of them, are not easy to be come by, and the success often exacts a good length of time, I shall conclude this paper, by subjoining a far shorter experiment, that I devised not only to shew in general, that the moisture of the air may have a considerable efficacy, but to assist a virtuoso to make some estimate in known measures of the mechanical force of the aerial moisture. And though I now find, to my trouble, that I want some of the notes, that concern the circumstances, and the progress of the trial, yet enough having escaped to furnish me with the following account of it, what I shall set down, may, I hope, at least put you in the way of repairing my misfortune.

THINKING it then probable, that ropes themselves would considerably imbibe, and dismiss the moisture of the air, and that so, as to shrink in rainy weather, though clogged with a weight fastened at the lower end, I was discouraged from attempting the following trial, by considering, that the weight would stretch the rope, and consequently hinder the presumed effect of the air's moisture to be perceived. For I supposed, that after a time, this unusual stretch of the rope would cease; and when the weight, as such, could not lengthen it any more, it would then be capable of being contracted or relaxed, according as the weather should be moist or dry, and so afford me a kind of hygroscope. Upon these grounds, I first caused a rope, that was about twenty or twenty-two yards in length, but of no great thickness, to have one of its ends fastened to an immoveable body at a convenient height from the ground, and then caused a pully to be so fastened to another stable body, at the distance of eighteen, or twenty yards from the first, that the rope, resting upon the pully, lay almost horizontally. But to the end of that part of the rope, which from the pully reached within two or three foot of the ground, was fastened, by a ring, a leaden weight of at least fifty pound. To which was also fastened a light index placed horizontally, whose end moved along an erected board, which, by transverse lines, was divided into inches, and parts of inches, reaching both a good way upwards and downwards, that the index might, within those bounds, have room to play up and down, according to the alterations of the weather.

IT being then summer, this trial was made in a garden, though partly under a pent-house, that the rope might be more exposed to the air, than it would have been within doors; and two or three days, if I mis-remember not the time, were spent, before the weight had brought the rope to the utmost stretch it was able to give it, after which, it began manifestly to shrink, and lengthen, according to the weather. And I find, in one of my notes, that once I looked, when I was ready to go to bed, upon the suspended weight, and marked, how low it reached upon the divided board; and that a great part of the night having been rainy, looking again about half an hour after eight in the morning, I found the cord so shrunk, that the weight was raised above five inches, and yet the day growing dry and windy, and sometimes warm, the weight had at night stretched the rope more than the moisture had contracted it the day before.

AFTERWARDS having procured a far greater weight, but therefore unapt to be near so much raised, I substituted it in the place of that formerly mentioned; and having suffered it to stretch the rope as far as it could, I made and registered some observations, two whereof having been preserved, I shall transcribe them just as I find them.

June the 4th. AT half an hour after nine of the clock at night, I looked upon the hundred pound weight, that hung at the bottom of the rope, the weather being then fair, and a mark being put at that part of the erected board, where the bottom of the weight touched; I perceived the sky, a while after, to grow cloudy and overcast, but without rain; wherefore, going to view the weight again, I found it to be risen a quarter of an inch, or more, and looking on my watch, perceived there had passed an hour and quarter since the mark was made.

June the 6th. BEING not well yesterday, the weight was observed by two of my servants, and it then rested at the eleventh inch of the erected board. This morning, about eight of clock, I visited it myself, and found it to be risen about half a quarter of an inch above the eighth inch, the morning being cloudy, though the ground very dry and dusty. The weather being more overcast, within somewhat less than an hour afterwards, I visited the weight again, (some scattered drops of rain then beginning to fall) and found it to be risen about half an inch above the newly-mentioned eighth mark. How much more the rope would have been contracted in such lasting moist weather, as usually happens in winter, I cannot say, having been reduced to break off the experiment, upon a removal, I was, long before that season, obliged to make.

I am sorry I cannot add my other observations; but these I hope may suffice to let you see, that the force of the air's moisture is not small, since it could raise such a weight as an hundred pound, especially considering the slenderness of the rope it affected. For having measured the diameter near the weight, I found it (as one of my notes informs me) to be but about the * third part of an inch.

THE

* It was $\frac{3}{10}$ and 4 decimal parts of $\frac{1}{10}$.

THE

EXCELLENCY

OF

THEOLOGY,

COMPARED WITH

NATURAL PHILOSOPHY,

(As both are OBJECTS of MEN'S STUDY.)

DISCOURSED OF

In a LETTER to a FRIEND.

TO WHICH ARE ANNEXED

Some Occafional Thoughts about the EXCELLENCY and GROUNDS of the MECHANICAL HYPOTHESIS.

The PUBLISHER's ADVERTISEMENT to the READER.

WHEN I fhall have told the reader, that the following difcourfe was written in the year 1665, while the author, to avoid the great plague, that then raged in *London*, was reduced, with many others, to go into the country, and frequently to pafs from place to place, unaccompanied with moft of his books; it will not, I prefume, be thought ftrange, that in the mention of fome things taken from other writers, as his memory fuggefted them, he did not annex in the margent the precife places, that are referred to. And upon the fame fcore, it ought not to feem ftrange, that he has not mentioned fome late difcoveries and books, that might have been pertinently taken notice of, and would well have accommodated fome parts of his difcourfe; fince things, that may thus feem to have been omitted, are of too recent a date to have been known to him when he writ. But if it be demanded, why then a difcourfe finifhed fo long ago, did not come abroad much fooner? I muft acquaint the reader, that it was chiefly his real concern for the welfare of the ftudy he feems to depreciate, that kept thefe papers fo long by him. For he refifted for feveral years the defires of perfons, that have much power with him, and fuppreffed the following difcourfe, whilft he feared it might be mifapplied by fome enemies to experimental philofophy, that then made a noife againft it, without fuffering thefe papers to come abroad, till the addreffes and encomiums of many eminent foreign virtuofi, and their defire to be admitted into the Royal Society, had fufficiently manifefted, how little its reputation was prejudiced, or like to be endangered, by the attempts of fome envious or mifinformed perfons. And to this reafon muft be added the author's backwardnefs to venture abroad a difcourfe of an unufual nature, on which account, among others, he declined to have his name prefixed to it; though now the book is printed, he finds caufe to fear, that it will not be long concealed; fince he meets with fome marginal references to other tracts of his, which (thefe papers having long lain by him) he forgot to have been fet down for private ufe, and which fhould not have been expofed to publick view.

THE

The AUTHOR's PREFACE.

I AM not so little acquainted with the temper of this age, and of the persons, that are likeliest to be perusers of the following tract, as not to foresee it to be probable enough, that some will ask, for what reason a discourse of this nature was written at all; and that others will be displeased, that it has been written by me.

THOSE, that would know, by what inducements my pen was engaged on this subject, may be in great part informed by the epistle it self, in divers places whereof, as especially about the beginning, and at the close, the motives, that invited me to put pen to paper, are sufficiently expressed. And though several of those things are peculiarly applied, and (if I may so speak) appropriated to the person the letter is addressed to; yet that under-valuation, I would dissuade him from, of the study of things sacred, is not his fault alone, but is grown so rife among many (otherwise ingenious) persons, especially studiers of physicks, that I wish the ensuing discourse were much less seasonable than I fear it is.

BUT I doubt, that some readers, who would not think a discourse of this nature needless or useless, may yet not be pleased at its being written by one, whom they imagine the acceptance his endeavours have met with, ought to oblige to spend his whole time in cultivating that natural philosophy, which in this letter he would persuade to quit the precedency, they think it may well challenge, before all other sorts of learning.

I am not unsensible of the favourable reception, that the philosophical papers I have hitherto ventured abroad, have had the happiness to receive from the curious: but I hope, they will not be displeased, if I represent, that I am no lecturer, or professor of physicks, nor have ever engaged myself, by any promise made to the publick, to confine myself, never to write of any other subject; nor is it reasonable, that what I did, or may write, to gratify other men's curiosity, should deprive me of mine own liberty, and confine me to one subject; especially, since there are divers persons, for whom I have a great esteem and kindness, who think they have as much right to sollicit me for composures of the nature of this, that they will now have to go abroad, as the virtuosi have to exact of me physiological pieces. And though I be not ignorant, that, in particular, the following discourse, which seems to depreciate the study of nature, may, at first sight, appear somewhat improper for a person, that has purposely written to shew the excellence and usefulness of it; yet I confess, that upon a more attentive consideration of the matter, I cannot reject, no, nor resist their reasons, who are of a quite differing judgment.

AND 1. My condition, and my being a secular person (as they speak) are looked upon as circumstances, that may advantage an author, that is to write upon such a subject as I have handled. I need not tell you, that as to religious books in general, it has been observed, that those penned by lay-men, and especially gentlemen, have (*cæteris paribus*) been better entertained, and more effectual, than those of ecclesiasticks. And indeed it is no great wonder, that exhortations to piety, and dissuasions from vice, and from the lusts and vanities of the world, should be the more prevalent for being pressed by those, who have, and yet decline, the opportunities to enjoy plentifully themselves the pleasures they dissuade others from. And (to come yet closer to our present purpose) though I will not venture to say with an excellent divine, that whatever comes out of the pulpit, does with many pass but for the foolishness of preaching; yet it cannot well be denied, but that if all other circumstances be equal, he is the fittest to commend divinity, whose profession it is not; and that it will somewhat add to the reputation of almost any study, and consequently to that of things divine, that it is praised and preferred by those, whose condition and course of life exempting them from being of any particular calling in the common-wealth of learning, frees them from the usual temptations to partiality to this or that sort of study, which others may be engaged to magnify, because it is their trade or their interest, or because it is expected from them; whereas these gentlemen are obliged to commend it, only because they really love and value it.

BUT there is another thing, that seems to make it yet more fit, that a treatise on such a subject should be penn'd by the author of this: for professed divines are supposed to be busied about studies, that even, by their being of an higher, are confessed to be of another nature, than those, that treat of things corporeal. And since it may be observed, that there is scarce any sort of learned men, that is more apt to undervalue those, that are versed only in other parts of knowledge, than many of our modern naturalists, (who are conscious of the excellency of the science they cultivate,) it is much to be feared, that what would be said of the pre-eminences of divinity above physiology, by preachers (in whom the study of the latter is thought either but a preparatory thing, or an excursion) would be looked upon as the decision of an incompetent, as well as interested judge; and their undervaluations of the advantages of the study of the creatures would be (as their depreciating the enjoyment of the creatures too often is,) thought to proceed but from their not having had sufficient opportunities to relish the pleasures of them. But these prejudices will not lie against a person,, who has made the indagation of nature somewhat more than a parergon, and having, by a not-lazy, nor short enquiry, manifested, how much he loves and can relish the delight it affords, has had the good fortune to make some discoveries in it, and the honour to have them publickly, and but too complimentally, taken notice of by the virtuosi. And it may be not impertinent to add, that those, who make natural philosophy their mistress, will, probably, be the less offended to find her in this tract represented, if not as an handmaid to divinity, yet as a lady of a lower rank; because the inferiority of the study of nature is maintained by a person, who, even whilst he asserts it, continues, if not a passionate, an assiduous courter of nature: so that, as far as his example can reach, it may shew, that as on the one side a man need not be acquainted with, or unfit to relish the lessons taught us in the book of the creatures, to think them less excellent than those, that may be learned in the book of the scriptures; so on the other side, the preference of this last book is very consistent with an high esteem and an assiduous study of the first.

And if any should here object, that there are some passages, which I hope are but very few, that seem a little too unfavourable to the study of natural things; I might alledge for my excuse the great difficulty, that there must be in comparing two sorts of studies, both of which a man much esteems, so to behave one's self, as to split a hair between them, and never offend either of them: but I will rather represent, that in such kind of discourses, as the ensuing, it may justly be hoped, that equitable readers will consider, not only what is said, but on what occasion, and with what design it is delivered. Now it is plain by the series of the following discourse, that the physeophilus, whom it most relates to, was by me looked upon as a person, both very partial to the study of nature, and somewhat prejudiced against that of the scripture; so that I was not always to treat with him, as with an indifferent man, but according to the advice given in such cases by the wise, I was (to use *Aristotle*'s expression) to bend the crooked stick the contrary way, in order to the bringing it to be strait, and to depreciate the study of nature somewhat beneath its true value, to reduce a great over-valuer to a just estimate of it. And to gain the more upon him, I allowed myself now and then to make use of the contempt he had of the peripatetick and vulgar philosophy, and in some passages to speak of them more slightingly, than my usual temper permits, and than I would be forward to do on another occasion; that, by such a complaisance for his opinions, I might have rises to argue with him from them.

But to return to the motives, that were alledged to induce me to the publication of these papers, though I have not named them all, yet all of them together would scarce have proved effectual, if they had not been made more prevalent by the just indignation I conceived, to see even inquisitive men depreciate that kind of knowledge, which does the most elevate, as well as the most bless, mankind, and look upon the noblest and wisest employments of the understanding, as signs of weakness in it.

It is not, that I expect, that whatever can be said, and much less what I have had occasion to say here, will make proselytes of those, that are resolved against the being made so, and had rather deny themselves the excellentest kinds of knowledge, than allow, that there can be any more excellent, than what they think themselves masters of: but I despair not, that what is here represented, may serve to fortify in a high esteem of divine truths those, that have already a just veneration for them, and preserve others from being seduced by injurious, though sometimes witty insinuations, to undervalue that kind of knowledge, that is as well the most excellent in itself, as the most conducive to man's happiness. And for this reason I am the less displeased to see, that the following letter is swelled to a bulk far greater than its being but a letter promises, and than I first intended. For I confess, that when the occasion happened, that made me put pen to paper, as I chanced to be in a very unsettled condition (which I fear has had too much influence on what I have written) so I did not design the insisting near so long upon my subject as I have done; but new things springing up, if I may so speak, under my pen, I was content to allow them room in my paper, because writing as well for my own satisfaction, as for that of my friend, I thought it would not be useless to lay before my own eyes, as well as his, those considerations, that seemed proper to justify to myself, as well as to him, the preference I gave divine truths (before physiological ones) and to confirm myself in the esteem I had for them. And though I freely confess, that the following discourse doth not consist of nothing but ratiocinations, and consequently is not altogether of an uniform contexture; yet that will, I hope, be thought no more than was fit in a discourse, designed not only to convince, but to persuade: which if it prove so happy as to do, as I hope the peruser will have no cause to regret the trouble of reading it, so I shall not repent that of writing it.

The

The INTRODUCTION.

SIR,

I Hoped you had known me better, than to doubt in good earneſt, how I reliſhed the diſcourſe your learned friend entertained us with yeſter-night. And I am the more troubled at your queſtion, becauſe your way of enquiring, how much your friend's diſcourſe obtained of my approbation, gives me cauſe to fear, that you vouchſafe it more of yours than I could wiſh it. But before I can ſafely offer you my ſenſe of the diſcourſes, about which you deſire to know it, I muſt put you in mind, that they were not all upon one ſubject, nor of the ſame nature: and I am enough his ſervant to acknowledge, without the leaſt reluctancy, that he is wont to ſhew a great deal of wit, when he ſpeaks like a naturaliſt, only of things purely phyſical; and when he is in the right, ſeldom wrongs a good cauſe by his way of managing it. But as for thoſe paſſages, wherein he gave himſelf the liberty of diſparaging the learned Dr. *N.* only becauſe that doctor cultivates theological, as well as phyſical ſtudies, and does both oftentimes read books of devotion, and ſometimes write them; I am not ſo much a courtier, as to pretend, that I liked them. 'Tis true, he did not deny the doctor to be a learned, and a witty man, as indeed the wiſe providence of God has ſo ordered it, that to ſtop the bold mouths of ſome, who would be eaſily tempted to imagine, and more eaſily to give out, that none are philoſophers, but ſuch as, like themſelves, deſire to be nothing: elſe our nation is happy in ſeveral men, who are as eminent for humane, as ſtudious of divine learning; and as great a veneration as they pay to *Moſes*, and St. *Paul*, are as well verſed in the doctrine of *Ariſtotle*, and of *Euclid*; nay, of *Epicurus* and *Des Cartes* too, as thoſe, that care not to ſtudy any thing elſe. But though, for this reaſon, Mr. *N.* had not the confidence to deſpiſe the doctor, and ſome of his reſemblers, whom he took occaſion to mention; yet he too plainly diſcloſed himſelf to be one of thoſe, who, though they will not deny, but that ſome, who own a value for theology, are men of parts; yet they talk, as if ſuch perſons were ſo, in ſpight of their being religiouſly given; that being, in their opinion, ſuch a blemiſh, that a man muſt have very great abilities otherwiſe, to make amends for the diſadvantage of valuing ſacred ſtudies, and ſurmount the diſparagement it procures him. Wherefore, ſince this diſdainful humour begins to ſpread, much more than I could wiſh it did, among different ſorts of men, among whom I ſhould be glad not to find any naturaliſts; and ſince the queſtion you aſked me, and the eſteem you have for your friend, makes me fear you may look on it with very favourable eyes; I ſhall not decline the opportunity you put into my hands of giving you, together with a profeſſion of my diſlike of his practice, ſome of my reaſons for that diſlike; and the rather, becauſe I may do it without too much exceeding the limits of an epiſtle, or thoſe which the haſte, wherewith I muſt write this, does preſcribe to me. For your friend does not oppoſe, but only undervalue theology; and profeſſing to believe the ſcriptures, (which I ſo far credit, as to think he believes himſelf when he ſays ſo) we agree upon the principles: ſo that I am not to diſpute with him, as againſt an atheiſt, that denies the author of nature, but only againſt a naturaliſt, that over-values the ſtudy of it. And the truths of theology are things, which I need not bring arguments for, but am allowed to draw arguments from them.

But though, as I juſt now intimated, I deſign brevity; yet, for fear the fruitfulneſs, and importance of my ſubject, ſhould ſuggeſt things enough to me, to make ſome little method requiſite to keep them from appearing confuſed; I ſhall divide the following epiſtle into two diſtinct parts. In the former of which, I ſhall offer you the chief poſitive conſiderations, by which I would repreſent to you the ſtudy of divinity, as preferable to that of phyſick: And, in the ſecond part, I ſhall conſider the allegations, that I foreſee your friend may interpoſe, in favour of natural philoſophy. From which diſtribution you will eaſily gather, that the motives on the one hand, and the objections on the other, will challenge to themſelves diſtinct ſections, in the reſpective parts whereto they belong. So that, of the order of the particulars you will meet with, I ſhall not need to trouble you with any further account.

THE

THE EXCELLENCY OF THEOLOGY:

OR,

The PRE-EMINENCE of the STUDY OF DIVINITY, above that of NATURAL PHILOSOPHY.

THE FIRST PART.

TO addreſs myſelf then, without any farther circumſtance, or preamble, to the things themſelves, that I mainly intend in this diſcourſe, I conſider in the general, that as there are ſcarce any motives accounted fitter to engage a rational man in a ſtudy, than that the ſubject is noble, that it is his duty to apply himſelf to it, and that his proficiency in it will bring him great advantages; ſo there is not any of theſe three inducements, that does not concur, in a very plentiful meaſure, to recommend to us the ſtudy of theological truths.

SECTION I.

AND firſt, the excellency and ſublimity of the object we are invited to contemplate, is ſuch, that none, that does truly acknowledge a deity, can deny, but that there is no ſpeculation, whoſe object is comparable, in point of nobleneſs, to the nature and attributes of God. The ſouls of inquiſitive men are commonly ſo curious, to learn the nature and condition of ſpirits, as that the over-greedy deſire to diſcover ſo much, as that there are other ſpiritual ſubſtances, beſides the ſouls of men, has prevailed with too many to try forbidden ways of attaining ſatisfaction; and many have choſen, rather to venture the putting themſelves within the power of dæmons, than remain ignorant whether or no there are any ſuch beings: as I have learned by the private acknowledgments made me of ſuch unhappy (though not unſucceſsful) attempts, by divers learned men, (both of other profeſſions, and that of phyſick,) who themſelves made them in differing places, and were perſons neither timorous, nor ſuperſtitious: (but this only upon the by.) And certainly that man muſt have as wrong, as mean a notion of the deity, and muſt but very little conſider the nature and attributes of that infinitely perfect Being, and as little the nature and infirmities of man, who can imagine the divine perfections to be ſubjects, whoſe inveſtigation a man may (inculpably) deſpiſe, or be ſo much as fully ſufficient for. Not only the ſcripture tells
us, That his greatneſs is incomprehenſible, and Pſal. cxlv.
his wiſdom is inſcrutable; That he humbles Pſ. cxlvii.
himſelf to look into (or upon) the heavens and 5.
the earth; and, That not only this, or that Pſ. cxiii. 6.
man, but all the nations of the world are, in Iſa. xl. 15.
compariſon of him, but like the ſmall drop of a bucket, or the ſmaller duſt of a balance: but even the heathen philoſopher, who wrote that eloquent book *de Mundo*, aſcribed to *Ariſtotle* in his riper years, ſpeaks of the power, and wiſdom, and amiableneſs of God, in terms little leſs lofty, though neceſſarily inferior to ſo infinitely ſublime a ſubject; which they, that think they can, eſpecially without revelation, ſufficiently underſtand, do very little underſtand themſelves.

BUT perhaps your friend will object, that, to the knowledge of God there needs no other than natural theology; and I readily confeſs, being warranted by an apoſtle, that
the γνωστὸν τοῦ Θεοῦ was not unknown to the hea- Rom. i. 19.
then philoſophers; and that ſo much knowledge of God is attainable by the light of nature, duly employed, as to encourage men to exerciſe themſelves, more than moſt of them do, in that nobleſt of ſtudies, and render their being no proficients in it, injurious to themſelves, as well as to their maker. But notwithſtanding this, as God knows himſelf infinitely better than purblind man knows him, ſo the informations he is pleaſed to vouchſafe us, touching his own nature, and attributes, are exceedingly preferable to any account, that we can give ourſelves of him, without him. And, methinks, the differing proſpects we may have of heaven, may not ill adumbrate to us the differing diſcoveries, that may be made of the attributes of its maker. For as though a man may, with his naked eye, ſee heaven to be a very glorious object, enobled with radiant ſtars of ſeveral ſorts; yet, when his eye is aſſiſted with a good teleſcope, he cannot only diſcover a

a number of stars, (fixed and wandering,) which his naked eye would never have shewn him; but those planets, which he could see before, will appear to him much bigger, and more distinct: so, although bare reason, well improved, will suffice to make a man behold many glorious attributes in the deity; yet the same reason, when assisted by revelation, may enable a man to discover far more excellencies in God, and perceive them, he contemplated before, far greater and more distinctly. And to shew how much a dim eye, illuminated by the scriptures, is able to discover of the divine perfections, and how unobvious they are to the most piercing philosophical eyes, that enjoy but the dim light of nature; we need but consider, how much more suitable conceptions and expressions concerning God are to met with in the writings of those fishermen and others, that penned the new testament, and those illiterate Christians, that received it, than amongst the most civilized nations of the world (such as anciently the Greeks and Romans, and now the Chineses and East-Indians) and among the eminentest of the wise-men and philosophers themselves, (as *Aristotle*, *Homer*, *Hesiod*, *Epicurus*, and others.)

Besides that the book of scripture discloses to us much more of the attributes of God, than the book of nature, there is another object of our study, for which we must be entirely beholden to theology: for though we may know something of the nature of God by the light of reason, yet we must owe the knowledge of his will, or positive laws, to his own revelation. And we may guess, how curious great princes and wise men have been to inform themselves of the constitutions established by wise and eminent legislators; partly by the frequent travels of the ancient sages and philosophers into foreign countries, too bserve their laws and government, as well as bring home their learning; and partly by those royal and sumptuous expences, at which that great and learned monarch *Ptolomeus Philadelphus* stuck not to procure an authentick copy of the law of *Moses*, whom he considered but as an eminent legislator. But certainly that, and other laws recorded in the bible, cannot but appear more noble and worthy objects of curiosity to us Christians, who know them to proceed from an omniscient deity, who being the author of mankind, as well as of the rest of the universe, cannot but have a far perfecter knowledge of the nature of man, than any other of the law-givers, or all of them put together can be conceived to have had.

But there is a farther discovery of divine matters, wherewith we are also gratified by theology: for besides what the scripture teaches us of the nature and the will of God, it contains divers historical accounts (if I may so call them) of his thoughts and actions. The great *Alexander* thought himself nobly employed, when he read the Grecian actions in *Homer*'s verses; and, to know the sentiments of great and wise persons upon particular occasions, is a curiosity so laudible, and so worthy of an inquisitive soul, that the southern queen has been more praised than admired, for coming from the remoter parts of the earth, to hear the wisdom of *Solomon*. Now the scripture does in many places give our curiosity a nobler employment, and thereby a higher satisfaction, than the king of *Macedon*, or the queen of *Sheba* could enjoy; for in many places it does, with great clearness and ingenuity, give us accounts of what God himself hath declared of his own thoughts, of divers particular persons and things, and relates, what he, that knows and commands all things, was pleased to say and do upon particular occasions. Of this sort of passages are the things recorded to have been said by God to *Noah*, about Genes. vi.
the sinful world's ruin, and that just man's preservation; and to *Moses* in the case of daughters Numb.
of *Zelophehad*. And of this sort are the con- xxvii. 7.
ferences, mentioned to have passed betwixt God and *Abimelech*, concerning *Abraham*'s Genes.
Wife; betwixt God and *Abraham*, touching xx.
the destruction of *Sodom*; betwixt God and Genes.
Solomon, about that king's happy choice; be- xviii.
twixt God and *Jonah*, about the fate of the 1 Kings
greatest city of the world: and above all these, iii. Jonah iv.
those two strange and matchless passages, the one in the first book of *Kings*, touching the 1 Kings
seducing spirit, that undertook to seduce *Ahab*'s xxii. from
prophets; and the other, that yet more wonder- ver. 19.
ful relation of what passed betwixt God and Sa- to ver. 24. Job i.
tan, wherein the deity vouchsafes, not only to 6, 7, &c.
praise, but (if I may so speak with reverence) Job ii. 3.
to glory in a mortal. And the being admitted to the knowledge of these transactions of another world, (if I may so call them) wherein God has been pleased to disclose himself so very much, is an advantage afforded us by the scripture, of so noble a nature, and so unattainable by the utmost improvement we ourselves can make of our own reason, that, did the scripture contain nothing else, that were very considerable, yet that book would highly deserve our curiosity and gratitude.

And on this occasion, I must by no means leave unobserved another advantage, that we have from some discourses made us in the bible, since it too highly concerns us, not to be a very great one; and it is, that the scripture declares to us the judgment, that God is pleased to make of some particular men, upon the estimate of their life and deportment. For though reason alone, and the grounds of religion in general, may satisfy us in some measure, that God is good and merciful, and therefore it is likely he may pardon the sins and frailties of men, and accept of their imperfect services; yet besides that we do not know, whether he will pardon, unless we have his promise of it; besides this, (I say) though by vertue of general revelation, such as is pretended to in divers religions, we may be assured, that God will accept, forgive, and reward those, that sincerely obey him, and perform the conditions See Heb.
of the covenant, whether it be express, or im- v. 9.
plicite, that he vouchsafes to make with them; Psal. ciii.
yet since it is he, that is the judge of the per- 17, 18.
formance of the conditions, and of the sincerity of the person; and since he is omniscient, and a Καρδιογνώστης and so may know more ill of us, Acts i. 21.
than even we know of ourselves; a concerned

1 Joh. iii. conscience may rationally doubt, whether in God's estimate any particular man was so sincere as to be accepted. But when he himself is pleased to give elogiums (if I may with due respect so style them) to *David*, *Job*, *Noah*, *Daniel*, &c. whilst they were alive, and to others after they were dead, (and consequently having finished their course, were passed into an irreversible state) we may learn with comfort, both that the performance of such an obedience, as God will accept, is a thing really practicable by men; and that even great sins and misdemeanors are not (if seasonably repented of) certain evidences, that a man shall never be happy in the future life. And it seems to be for such an use of consolation to frail men, (but not at all to encourage licentious ones) that the lapses of holy persons are so frequently recorded in the scriptures. And bating those divine writings, I know no books in the world, nor all of them put together, that can give a considering Christian, who has due apprehensions of the inexpressible happiness or misery of an immortal state in heaven or in hell, so great and well grounded a consolation, as may be derived from three or four lines in St. *John*'s apocalypse, where he says, "That
Revel. vii. 9. "he saw in heaven a great multitude, not to "be numbered of all nations, and tribes, and "people, and tongues, standing before the "throne, and before the lamb, clothed in "white robes, with palms (the ensigns of "victory) in their hands;" and the praises of God and of the lamb in their mouths. For from thence we may learn, that heaven is not reserved only for prophets, and apostles, and martyrs, and such extraordinary persons, whose sanctity the church admires, but that, through God's goodness, multitudes of his more imperfect servants have access thither.

THOUGH the infinite perfections and prerogatives of the deity be such, that theology itself can, no more than philosophy, afford us another object for our studies, any thing near so sublime and excellent, as what it discloses to us of God; yet divinity favours us with some other discoveries, namely, about angels, the universe, and our own souls, which, though they must needs be inferior to the knowledge of God himself, are, for the nobleness of their objects, or for their importance, highly preferable to any, that natural philosophy has been able to afford its votaries.

BUT before I proceed to name any more particulars, disclosed to us by revelation, it will be requisite, for the prevention or removal of a prejudice, to mind you, that we should not make our estimates of the worth of the things we owe to revelation, by the impressions they are wont now to make upon us christians, who learned divers of them in our catechisms, and perhaps have several times met with most of the rest in sermons, or theological books. For it is not to be admired, that we should not be strongly affected at the mention of those truths, which (how valuable soever in themselves) were for the most part taught us when we were either children, or too youthful to discern and prize their excellency and importance. So that though afterwards they were presented to our riper understanding, yet their being by that time become familiar, and our not remembering, that we ignored them, kept them from making any vigorous impressions on us. Whereas, if the same things had been (with circumstances evincing their truth) discovered to some heathen philosopher, or other vertuous and inquisitive man, who valued important truths, and had nothing but his own reason to attain them with, he would questionless have received them with wonder and joy. Which to induce us to suppose we have sundry instances, both in the records of the primitive times, and in the recent relations of the conversion of men to Christianity among the people of *China*, *Japan*, and other literate nations. For though bare reason cannot discover these truths, yet when revelation has once sufficiently proposed them to her, she can readily embrace, and highly value divers of them; which being here intimated once for all, I now advance to name some of the revelations themselves.

AND first, as for angels, I will not now question, whether bare reason can arrive at so much as to assure us, that there are such beings in *rerum naturâ*. For though reason may assure, that their existence is not impossible, and perhaps too, not improbable; yet I doubt, whether 'twere to meer ratiocination, or clear experience, or any thing else but revelation, con-
vey'd to them by imperfect tradition, that those Matth. xxvi. 53.
heathen philosophers, who believed, that there Dan. vii.
were separate spirits other than humane, owed 10.
that persuasion; and particularly as to good Joh. i. 3.
angels, I doubt, whether those antient sages had Heb. i. 7. Luke xx.
any cogent reasons, or any convincing historical 35, 36.
proofs, or, in short, any one unquestionable e- Col. i. 16.
vidence of any kind, to satisfy a wary person so Matth. xxiv. 36.
much as of the being (much less to give a far- Mark
ther account) of those excellent spirits. Where- xiii. 32.
as theology is enabled by the scripture to inform Matth. xviii. 10.
us, that not only there are such spirits, but a Isa. vi. 2, 3.
vast multitude of them; that they were made Matth.
by God and Christ, and are immortal, and pro- vi. 10.
pagate not their species; and that these spirits 2 Sam. xiv. 20.
have their chief residence in heaven, and enjoy Mark xiii.
the vision of God, whom they constantly praise, 32.
and punctually obey, without having sinned a- 2 King.
gainst him; that also these good angels are very xix. 35. 1 Thess.
intelligent beings, and of so great power, that iv. 16.
one of them was able in a night to destroy a vast Jude ix.
army; that they have degrees among them- Dan. x.
selves, are enemies to the devils, and fight a- 13, 21. Col. i. 16.
gainst them; that they can assume bodies shaped Revel.
like ours, and yet disappear in a trice; that they xii. 7.
are sometimes employed about human affairs, Acts xii. 7, 8, 9, 10.
and that not only for the welfare of empires and Dan x.
kingdoms, but to protect and rescue single good 13.
men. And though they are wont to appear in a Acts xii.
dazling splendor, and an astonishing majesty, 11. 2 Kings
yet they are all of them ministring spirits, em- vi. 17.
ployed for the good of the designed heirs of Luke
salvation. And they do not only refuse men's xxiv. 4. Judg. xiii.
adoration, and admonish them to pay it unto 6.
God; but, as they are in a sense made by Jesus Heb. i. 14.
Christ, who was true man as well as God; so Rev. xix.
they do not only worship him, and call him sim- 10. Rev. xxii.

Matth. xxviii. 6. Rev. xix. 10. ply, as his own followers were wont to do, the Lord, but stile themselves fellow servants to his disciples.

And as for the other angels, though the Gentiles, as well philosophers as others, were commonly so far mistaken about them, as to adore them for true gods, and yet many of them Joh. i. 3. to doubt whether they were immortal; the Colos. i. 16. scripture informs us, that they are not self-originated, but created beings; Matth. viii. 7. that however a great part of mankind worship them, they are Luke iv. 33. wicked and impure spirits, enemies to mankind, Joh. viii. 34. and seducers of our first parents to their ruin; that though they beget and promote confusion 1 Pet. v. 8. among men, yet they have some order among 2 Cor. xi. 3. themselves, as having one chief, or leader; that Rev. xii. 9. they are evil spirits, not by nature, but apos- Rev. xii. 7. tacy; that their power is very limited, in- Matth. xxv. 41. somuch that a legion of them cannot invade so contemptible a thing as a herd of swine, with- 1 Joh. iii. 8. out particular leave from God; that not only Jude 6. good angels, but good men, may, by resisting Mark v. 9, 10, 13. them, put them to flight, and the sincere Chris- Jam. iv. 7. tians, that worsted them here, will be among 1 Pet. v. 9. those, that shall judge them hereafter; that their 1 Cor. vi. being immortal, will make their misery so too; Matth. xxv. 41. that they do themselves believe, and tremble at Jam. ii. 19. those truths, they would persuade men to reject; and that they are so far from being able to con- 2 Pet. ii. 4. fer that happiness, which their worshippers ex- Jude 6. 13. pect from them, that themselves are wretched Matth. xxv. 41. creatures, reserved in chains of darkness to the judgment of the great day; at which they shall be doomed to suffer everlasting torments, in the company of those wicked men, that they shall have prevailed on.

We may farther consider, that as to things corporeal themselves, which the naturalist challenges as his peculiar theme, we may name particulars, and those of the most comprehensive nature, and greatest importance, whose knowledge the naturalist must owe to theology. Of which truths I shall content my self to give a few instances in the world itself, or the universal aggregate of things corporeal; that being looked upon as the noblest and chiefest object, that the physicks afford us to contemplate.

And first, those that admit the truths revealed by theology, do generally allow, that God is not only the author, but creator of the world. I am not ignorant of what *Anaxagoras* taught, of what he called νοῦς — (and *Tully* mentions) in the production of the world; and that what many other Grecians afterwards taught of the world's eternity, is peculiarly due to *Aristotle*, who does little less than brag, that all the philosophers that preceded him were of another mind. Nor will I here examine (which I elsewhere do) whether, and how far, by arguments meerly physical, the creation of the world may be evinced. But whether or no meer natural reason can reach so sublime a truth, yet it seems not, that it did actually, where it was not excited by revelation-discovery. For though many of the ancient philosophers believed the world to have had a beginning, yet they all took it for granted, that matter had none; nor does any of them, that I know of, seem to have so much as imagined, that any substance could be produced out of nothing. Those, that ascribe much more to God than *Aristotle*, make him to have given form only, not matter to the world, and to have but contrived the pre-existent matter into this orderly system we call the universe.

Next, whereas very many of the philosophers, that succeeded *Aristotle*, suppose the world to have been eternal; and those, that believed it to have been produced, had not the confidence to pretend to the knowing how old it was; unless it were some extravagant ambitious people, such as those fabulous Chaldeans, whose fond acccount reached up to forty thousand or fifty thousand years: theology teaches us, that the world is very far from being so old by thirty or forty thousand years as they, and by very many ages, as divers others have presumed: and does, from the scripture, give us such an account of the age of the world, that it has set us certain limits, within which so long a duration may be bounded, without mistaking in our reckoning. Whereas philosophy leaves us to the vastness of indeterminate duration, without any certain limits at all.

The time likewise, and the order, and divers other circumstances of the manner, wherein the fabrick of the world was compleated, we owe to revelation; bare reason being evidently unable to inform us of particulars, that preceded the origin of the first man; and though I do not think religion so much concerned, as many do, in their opinion and practise, that would deduce particular theorems of natural philosophy from this or that expression of a book, that seems rather designed to instruct us about spiritual than corporeal things. I see no just reason to embrace their opinion, that would so turn the two first chapters of *Genesis*, into an allegory, as to overthrow the literal and historical sense of them. And though I take the scripture to be mainly designed to teach us nobler and better truths, than those of philosophy; yet I am not forward to condemn those, who think the beginning of *Genesis* contains divers particulars, in reference to the origin of things, which though not unwarily, or alone to be urged in physicks, may yet afford very considerable hints to an attentive and inquisitive peruser.

And as for the duration of the world, which was by the old philosophers held to be interminable, and of which the Stoicks opinion, that the world shall be destroyed by fire, (which they held from the *Jews*) was physically precarious; theology teaches us expresly from divine revelation, that the present course of na- Τρόχος τῆς γενέσεως. Jam. iij. 6. ture shall not last always, but that one day this world, or at least, this vortex of ours, shall either be abolished by annihilation, or, which seems far more probable, be innovated, and, as it were, transfigured, and that, by the interven- 2 Pet. iii. 7, 10, 13. tion of that fire, which shall dissolve and destroy the prsent frame of nature: so that either way, the present state of things, (as well natural as political), shall have an end.

And as theology affords us these informations about the creatures in general; so touching the chiefest and noblest of the visible ones, men, revelation discovers very plainly divers

very

very important things, where reason muſt needs be in the dark.

AND firſt, touching the body of man; the Epicureans attributed its original, as that of all things elſe, to the caſual concourſe of atoms; and the Stoicks abſurdly and injuriouſly enough (but much more pardonably than their follower herein, Mr. *Hobbes*) would have men to ſpring up like muſhrooms out of the ground; and whereas other philoſophers maintain conceits about it, too wild to be here recited; the book of *Geneſis* aſſures us, that the body of man was firſt formed by God in a peculiar man-
Gen. ii. 7. ner, of a terreſtrial matter; and it is there deſcribed, as having been perfected before the ſoul was united to it. And as theology thus teaches us, how the body of man had its firſt beginning; ſo it likewiſe aſſures us, what ſhall
Acts xxiv. 15. become of the body after death, though bare natural reaſon will ſcarce be pretended to reach to ſo abſtruſe and difficult an article as that of
Acts xvii. 20, 32. a reſurrection; which, when propoſed by St. *Paul*, produced among the Athenian philoſophers nothing elſe but wonder or laughter.

NOT to mention, that theology teaches us divers other things about the origin and con-
Gen. ii. dition of men's bodies; as, that all mankind
Acts xvii. 26. is the offspring of one man and one woman; that the firſt woman was not made of the ſame matter, nor after the ſame manner as the firſt man, but was afterwards taken from his ſide;
Gen. ii. 21, 22. that both *Adam* and *Eve* were not, as many Epicureans and other philoſophers fancied, that the firſt men were firſt infants; whence they did, as we do, grow by degrees, to be mature and compleat human perſons, but were made
Acts xxv. 15. ſo all at once; and that hereafter, as all mens bodies ſhall riſe again, ſo they ſhall all (or at
Luke xx. 35, 36. leaſt, all thoſe of the juſt) be kept from ever dying a ſecond time.

AND as for the human ſoul, though I willingly grant, that much may be deduced from the light of reaſon only, touching its exiſtence, properties and duration; yet divine revelation teaches it us with more clearneſs, and with greater authority; as ſure he, that made our ſouls, and upholds them, can beſt know what they are, and how long he will have them laſt. And as the ſcripture expreſly teaches us, that the rational ſoul is diſtinct from the body, as
Matth. x. 28. not being to be deſtroyed by thoſe very enemies, that kill the body; ſo about the origin of this immortal ſoul (about which philoſophers can give us but wide and precarious conjectures) theology aſſures us, that the ſoul of man had not ſuch an origination, as thoſe of other animals, but was God's own immediate work-
Gen. ii. 7. Zek. xii 1. manſhip, and was united to the body already formed: and yet not ſo united, but that upon
Luke xx. 35, 36. Matth. xxv. 46. their divorce, ſhe will ſurvive, and paſs into a ſtate, in which death ſhall have no power over her.

I expect you will here object, that for the knowledge of the perpetual duration of ſeparate ſouls, we need not be beholden to the ſcripture, ſince the immortality of the ſoul may be ſufficiently proved by the ſole light of nature, and particularly has been demonſtrated by your great *Des Cartes*. But you muſt give me leave to tell you, that beſides that a matter of that weight and concernment cannot be too well proved, and conſequently ought to procure a welcome for all good mediums of probation; beſides this, I ſay, I doubt many Carteſians do, as well as others, miſtake both the difficulty under conſideration, and the ſcope of *Des Cartes*'s diſcourſe. For I grant, that by natural philoſophy alone, the immortality of the ſoul may be proved againſt its uſual enemies Atheiſts and Epicureans. For the ground, upon which theſe men think it mortal, being, that it is not a true ſubſtance, but only a modification of the body, which conſequently muſt periſh, when the frame or ſtructure of the body, whereto it belongs, is diſſolved; their ground being this, I ſay, if we can prove, by ſome intellectual operations of the rational ſoul, which matter, however modified, cannot reach, that it is a ſubſtance diſtinct from the human body, there is no reaſon, why the diſſolution of the latter ſhould infer the deſtruction of the former, which is a ſimple ſubſtance, and as real a ſubſtance as matter it ſelf, which yet the adverſaries affirm to be indeſtructible. But though by the mental operations of the rational ſoul, and perhaps by other mediums it may, againſt the Epicureans, and other mere naturaliſts, who will not allow God to have any thing to do in the caſe, be proved to be immortal in the ſenſe newly propoſed; yet the ſame proofs will not evince, that abſolutely it ſhall never ceaſe to be, if we diſpute with philoſophers, who admit, as the Carteſians and many others do, that God is the ſole creator and preſerver of all things. For how are we ſure, but that God may have ſo ordained, that though the ſoul of man, by the continuance of his ordinary and upholding concourſe, may ſurvive the body, yet, as it is generally believed not to be created, till it be juſt to be infuſed into the body; ſo it ſhall be annihilated, when it parts with the body, God withdrawing at death that ſupporting influence, which alone kept it from relapſing to its firſt nothing. Whence it may appear, that notwithſtanding the phyſical proofs of the ſpirituality and ſeparableneſs of the human ſoul, we are yet much beholden to divine revelation for aſſuring us, that its duration ſhall be endleſs. And now to make good what I was intimating above, concerning the Carteſians, and the ſcope of *Des Cartes*'s demonſtration, I ſhall appeal to no other than his own expreſſions to evince, that he conſidered this matter for the main as we have done, and pretended to demonſtrate, that the ſoul is a diſtinct ſubſtance from the body; but not that abſolutely ſpeaking it is immortal. *Cur* (anſwers that ex- *Des Cartes reſponſione ad objectiones ſecundas, pag. m. 95.*
cellent author) *de immortalitate animæ nihil ſcripſerim, jam dixi in ſynopſi mearum Meditationum. Quod ejus ab omni corpore diſtinctionem ſatis probaverim, ſupra oſtendi. Quod vero additis, ex diſtinctione animæ à corpore non ſequi ejus immortalitatem, quia nihilominus dici poteſt, illam à deo talis naturæ factam eſſe, ut ejus duratio ſimul cum duratione vitæ corporeæ finiatur, fateor à me refelli non poſſe. Neque enim tantum mihi aſſumo, ut quicquam de iis, quæ à libera Dei voluntate dependent, humanæ rationis vi determinare*

minare aggrediar. Docet naturalis cognitio, &c. Sed si de absoluta Dei potestate quæratur, an forte decreverit, ut humanæ animæ iisdem temporibus esse desinant, quibus corpora quæ illis adjunxit; solius Dei est, respondere. And if he would not assume to demonstrate by natural reason so much as the existence of the soul after death, unless upon a supposition; we may well presume, that he would less take upon him to determine, what shall be the condition of that soul after it leaves the body. And that you may not doubt of this, I will give you for it his own confession, as he freely writ it in a private letter to that admirable lady, the princess *Elizabeth*, first daughter to *Frederick* king of *Bohemia*, who seems to have desired his opinion on that important question, about which he sends her this answer, *Pour ce qui, &c. i. e.* As to the state of the soul after this life, my knowledge of it is far inferior to that of monsieur (he means Sir *Kenelm) Digby*. For, setting aside that, which religion teaches us of it, I confess, that by mere natural reason we may indeed make many conjectures to our own advantage, and have fair hopes, but not any assurance. And accordingly in the next clause he gives the imprudence, of quitting what is certain for an uncertainty, as the cause why, according to natural reason, we are never to seek death.

NOR do I wonder he should be of that mind. For all, that mere reason can demonstrate, may be reduced to these two things; one, that the rational soul, being an incorporeal substance, there is no necessity, that it should perish with the body; so that, if god have not otherwise appointed, the soul may survive the body, and last for ever: the other, that the nature of the soul, according to *Des Cartes*, consisting in its being a substance, that thinks, we may conclude, that though it be by death separate from the body, it will nevertheless retain the power of thinking. But now, whether either of these two things, or both, be sufficient to endear the state of separation after death, to a considering man, I think may be justly questioned. For immortality or perseverance in duration, simply considered, is rather a thing presupposed to, or a requisite of felicity, than a part of it; and being in itself an adiaphorous thing, assumes the nature of the state or condition, to which it is joined, and does not make that state happy or miserable, but makes the possessors of it more happy, or more miserable, than otherwise they would be. And though some schoolmen, upon airy metaphysical notions, would have men think it is more eligible to be wretched, than not to be at all; yet we may oppose to their speculative subtilties the sentiments of mankind, and the far more considerable testimony of the Saviour of mankind, who speaking of the disciple, that betrayed him,
Mark xiv. 21. says, "That it had been good for that man, if "he had never been born." And eternity is generally conceived to aggravate no less the miseries of hell, than it heightens the joys of heaven. And here we may consider, first, that mere reason cannot so much as assure us absolutely, that the soul shall survive the body; for the truth of which we have not only *Cartesius*'s confession, lately recited, but a probable argument, drawn from the nature of the thing, since, as the body and soul were brought together, not by any mere physical agents, and since their association and union, whilst they continued together, was made upon conditions, that depended solely upon God's free and arbitrary institution; so, for aught reason can secure us of, one of the conditions of that association may be, that the body and soul should not survive each other. Secondly, supposing, that the soul be permitted to outlive the body, mere reason cannot inform us, what will become of her in her separate state, whether she will be vitally united to any other kind of body or vehicle; and if to some, of what kind that will be, and upon what terms the union will be made. For possibly she may be united to an unorganized, or very imperfectly organized body, wherein she cannot exercise the same functions she did in her human body. As we see, that even in this life the souls of natural fools are united to bodies, wherein they cannot discourse, or, at least, cannot philosophize. And it is plain, that some souls are introduced into bodies, which, by reason of paralytical and other diseases, they are unable to move, though that does not always hinder them from being obnoxious to feel pain. So that, for aught we naturally know, a human soul, separated from the body, may be united to such a portion of matter, that she may neither have the power to move it, nor the advantage of receiving any agreeable informations by its interventions, having upon the account of that union no other sense than that of pain. But let us now consider, what will follow, if I should grant, that the soul will not be made miserable, by being thus wretchedly matched. Suppose we then, that she be left free to enjoy what belongs to her own nature; that being only the power of always thinking, it may be well doubted, whether the exercise of that power will suffice to make her happy. You will perchance easily believe, that I love, as well as another, to entertain my self with my own thoughts, and to enjoy them undisturbed by visits and other avocations: I would, only accompanied by a servant and a book, go to dine at an inn upon a road, to enjoy my thoughts the more freely for that day. But yet, I think, the most contemplative men would, at least in time, grow weary of thinking, if they received no supply of objects from without, by reading, seeing, or conversing; and if they also wanted the opportunity of executing their thoughts, by moving the members of their bodies, or of imparting them, either by discoursing, or writing of books, or by making of experiments. On this occasion I remember, that I knew a gentleman, who was in *Spain* for a state-crime, which yet he thought an heroic action, kept close prisoner for a year in a place, where, though he had allowed him a diet not unfit for a person of note as he was; yet he was not permitted the benefit of any light, either of the day or candles, and was not accosted by any human creature, save at certain

times by the goaler, that brought him meat and drink, but was ſtrictly forbidden to converſe with him. Now, though this gentleman, by his diſcourſe, appeared to be a man of a lively humour; yet being aſked by me, how he could do to paſs the time in that ſad ſolitude, he confeſſed to me, that though he had the liberty of walking to and fro in his priſon, and though, by often recalling into his mind all the adventures and other paſſages of his former life, and by ſeveral ways combining, and diverſifying his thoughts, he endeavoured to give his mind as much variety of employment as he was able; yet that would not ſerve his turn, but he was often reduced, by drinking large draughts of wine, and then caſting himſelf upon his bed, to endeavour to drown that melancholly, which the want of new objects caſt him into. And I can eaſily admit, he found a great deal of difference between the ſenſe he had of thinking when he was at liberty, and that, which he had, when he was confined to that employment, whoſe delightfulneſs, like fire, cannot laſt long, when it is, as his was, denied both fuel and vent. And, in a word, though I moſt readily grant, that thinking, interwoven with converſation and action, may be a very pleaſant way of paſſing one's time; yet man being by nature a ſociable creature, I fear that alone would be a dry and weariſome employment to ſpend eternity in.

BEFORE I proceed to the next ſection, I muſt not omit to take notice, that though the brevity I propoſed to myſelf, keeps me from diſcourſing of any theological ſubjects, ſave what I have touched upon about the divine attributes, and the things I have mentioned about the univerſe in general, and the human ſoul; yet there are divers other things, knowable by the help of revelation, and not without it, that are of ſo noble and ſublime a nature, that the greateſt wits may find their beſt abilities both fully exerciſed, and highly gratified by making enquiries into them. I ſhall not name for proof of this the adorable myſtery of the trinity, wherein it is acknowledged, that the moſt ſoaring ſpeculators are wont to be poſed, or to loſe themſelves: but I ſhall rather mention the redemption of mankind, and the decrees of God concerning men. For though theſe ſeem to be leſs out of the ken of our natural faculties; yet it is into ſome things, that belong to the former of them, that
1 Pet. i. 12. the ſcripture tells us, *The angels deſire to pry*; and it was the conſideration of the latter of them, that made one, that had been caught up
Rom. xi. 33. into the manſion of the angels, amazedly cry out, ὦ βάθος, &c.

NOR are theſe the only things, that the ſcripture itſelf terms myſteries, though, for brevity ſake, inſtead of ſpecifying any of them, I ſhall content myſelf to repreſent to you in general; that ſince God's wiſdom is boundleſs, it may, ſure, have more ways than one to diſplay itſelf. And though the material world be full of the productions of his wiſdom; yet that hinders not, but that the ſcripture may be cnobled with many excellent impreſſes, and, as it were, ſignatures of the ſame attribute. For, as I was beginning to ſay, it cannot but be highly injurious to the Deity, in whom all other true perfections, as well as omniſcience, are both united and tranſcendent, to think, that he can contrive no ways to diſcloſe his perfections, beſides the ordering of matter and motion, and cannot otherwiſe deſerve to be the object of men's ſtudies, and their admiration, than in the capacity of a creator.

AND I think, I might ſafely add, that beſides theſe grand and myſterious points I came from mentioning, there are many other noble and important things, wherein unaſſiſted reaſon leaves us in the dark; which though not ſo clearly revealed in the ſcripture, are yet in an inviting meaſure diſcovered there, and conſequently deſerve the indagation of a curious and philoſophical ſoul. Shall we not think it worth enquiring, whether the ſatisfaction of Chriſt was neceſſary to appeaſe the juſtice of God, and purchaſe redemption for mankind? Or whether God, as abſolute and ſupreme governor of the world, might have freely remitted the penalties of ſin? Shall we not think it worth the enquiring, upon what account, and upon what terms, the juſtification of men towards God is tranſacted, eſpecially conſidering how much it imports us to know, and how perplexedly a doctrine, not in itſelf abſtruſe, is wont to be delivered? Shall not we enquire, whether or no the ſouls of men, before they were united to their bodies, pre-exiſted in a happier ſtate, as many of the ancient and modern Jews and Platoniſts, and (beſides Origen) ſome learned men of our times do believe? And ſhall not we be curious to know, whether, when the ſoul leaves the body, it do immediately paſs to heaven or hell (as it is commonly believed,) or for want of organs be laid, as it were, aſleep in an inſenſible and unactive ſtate, till it recover the body at the reſurrection, as many Socinians and others maintain? or whether it be conveyed into ſecret receſſes, where, though it be in a good or bad condition, according to what it did in the body, it is yet reprieved from the flames of hell, and reſtrained from the beatifick viſion till the day of judgment? (which ſeems to have been the opinion of many, if not moſt of the primitive fathers and Chriſtians.) Shall not we be curious to know, whether, at that great decretory day, this vaſt fabrick of the world, which all confeſs muſt have its frame quite ſhattered, ſhall be ſuffered to relapſe into its firſt nothing, as ſeveral divines aſſert? or ſhall be, after its diſſolution, renewed to a better ſtate, and as it were, transfigured? And ſhall not we enquire, whether or no, in that future ſtate of things, which ſhall never have Gen. ii.
an end, we ſhall know one another? (as *Adam*, 21, 22,
when he awaked out of his profound ſleep, 23.
knew *Eve*, whom he never ſaw before;) and whether thoſe perſonal friendſhips and affections, we had for one another here, and the pathetic conſideration of the relations (as of father and ſon, huſband and wife, chaſte miſtreſs and virtuous lover, prince and ſubject,) on which many of them were grounded, ſhall

continue?

continue? Or whether all those things, as antiquated and slight, shall be obliterated, and, as it were, swallowed up? (as the former relation of a cousin a great way off is scarce at all considered, when the persons come so to change their state, as to be united by the strict bonds of marriage.)

BUT it were tedious to propose all the other points, whereof the divine takes cognizance, that highly merit an inquisitive man's curiosity; and about which, all the writings of the old Greek and other heathen philosophers put together, will give us far less information, than the single volume of canonical scripture. I foresee indeed, that it may nevertheless be objected, that in some of these enquiries, revelation incumbers reason, by delivering things, which reason is obliged to make its hypothesis consistent with. But besides, that this cannot be so much as pretended of all; if you consider, how much unassisted reason leaves us in the dark about these matters, wherein she has not been able to frame so much as probable determinations, especially in comparison of those probabilities, that reason can deduce from what it finds one way or other delivered in the scripture: if you consider this, I say, you will, I presume, allow me to say, that the revealed truths, which reason is obliged to comply with, if they be burdens to it, are but such burdens, as feathers are to a hawk, which, instead of hindering his flight by their weight, enable him to soar toward heaven, and take a larger prospect of things, than, if he had not feathers, he could possibly do.

AND, on this occasion, Sir, the greater reverence I owe to the scripture itself, than to its expositors, prevails upon me to tell you freely, that you will not do right, either to theology, or (the greatest repository of its truths) the bible, if you imagine, that there are no considerable additions to be made to the theological discoveries we have already, nor no clearer expositions of many texts of scripture, or better reflections on that matchless book, than are to be met with in the generality of commentators, or of preachers, without excepting the antient fathers themselves. For there being, in my opinion, two things requisite, to qualify a commentator to do right to his theme, a competency of critical knowledge, and a concern for the honour and interest of Christianity in general, assisted by a good judgment, to discern and select those things, that may most conduce to it; I doubt, there are not many expositors, as they are called, of the scripture, that are not deficient in the former, or the latter of these particulars, and I wish there be not too many, that are defective in both.

THAT the knowledge of at least Greek and Hebrew is requisite to him, that takes upon him to expound writings penned originally in those languages, if the nature of the thing did not manifest it, you might easily be persuaded to believe, by considering, with what gross mistakes the ignorance of languages has oftentimes blemished, not only the interpretations of the school men and others, but even those of the venerable fathers of the church. For though generally they were worthy men, and highly to be regarded, as the grand witnesses of the doctrines and government of the antient churches; most of them very pious, many of them very eloquent, and some of them (especially the two criticks, *Origen* and *Jerom*) very learned; yet so few of the Greek fathers were skilled in Hebrew, and so few of the Latin fathers either in Hebrew or Greek, that many of their homilies, and even comments, leave hard texts as obscure as they found them; and, sometimes misled by bad translations, they give them senses exceeding wide of the true: so that many times in their writings they appear to be far better divines than commentators, and in an excellent discourse upon a text, you shall find but a very poor exposition of it; many of their eloquent and devout sermons being much better encomiasts of the divine mysteries they treat of, than unvailers. And though some modern translations deserve the praise of being very useful, and less unaccurate than those, which the Latin fathers used; yet when I read the scriptures (especially some books of the Old Testament) in their originals, I confess I cannot but sometimes wonder, what came into the mind of some, even of our modern translators, that they should so much mistake, and sometimes injure certain texts as they do; and I am prone to think, that there is scarce a chapter in the bible (especially that part of it, which is written in Hebrew,) that may not be better translated, and consequently more to the credit of the book itself.

THIS credit it misses of, not only by men's want of sufficient skill in critical learning, but (to come to the second member of our late division) for want of their having judgment enough to observe, and concern enough to propose those things in the scripture, and in theology, that tend to the reputation of either. For I fear there are too many, both commentators and other divines, that (though otherwise perhaps pious men) having espoused a church or party, and an aversion from all dissenters, are sollicitous, when they peruse the scripture, to take notice chiefly, if not only (I mean in points speculative) of those things, that may either suggest arguments against their adversaries, or answers to their objections. But I meet with much fewer than I could wish, who make it their business to *search the scriptures* for those things (such as unheeded prophecies, over-looked mysteries, and strange harmonies,) which being clearly and judiciously proposed, may make that book appear worthy of the high extraction it challenges (and consequently of the veneration of considering men) and who are sollicitous to discern and make out, in the way of governing and of saving men, revealed by God, so excellent an oeconomy, and such deep contrivances, and wise dispensations, as may bring credit to religion, not so much as it is Roman, or protestant, or Socinian, but as it is Christian. But (as I intimated before) these good

Ἐρευνᾶντας γραφάς. John v. 39.

good affections for the repute of religion in general are to be assisted by a deep judgment. For men, that want either that, or a good stock of critical learning, may easily over-see the best observations (which usually are not obvious) or proposed as mysteries, things that are either not grounded, or not weighty enough; and so (notwithstanding their good meaning) may bring a disparagement upon what they desire to recommend. And I am willing to grant, that it is rather for want of good skill and good judgment, than good will, that there are so few, that have been careful to do right to the reputation of the scripture, as well as to its sense. And indeed when I consider, how much more to the advantage of those sacred writings, and of Christian theology in general, divers texts have been explained and discoursed of by the excellent *Grotius*, by *Episcopius*, *Masius*, Mr. *Mede*, and Sir *Francis Bacon*, and some other late great wits (to name now no living ones) in their several kinds, than the same places have been handled by vulgar expositors, and other divines: and when I remember too, that none of these newly named worthies was at once a great philosopher, and a great critick; (the three first being not so well versed in philosophical learning, and the last being unacquainted with the eastern tongues:) I cannot but hope, that when it shall please God to stir up persons of a philosophical genius, well furnished with critical learning, and the principles of true philosophy, and shall give them a hearty concern, for the advancement of his truths; these men, by exercising upon theological matters that inquisitiveness and sagacity, that has made in our age such a happy progress in philosophical ones, will make explications and discoveries, that will justify more than I have said in praise of the study of our religion, and the divine books, that contain the articles of it. For these want not excellencies, but only skilful unvailers. And if I do not tell you, that you should no more measure the wisdom of God couched in the bible, by the glosses or systems of common expositors and preachers, than estimate the wisdom he has expressed in the contrivance of the world by *Magirus*'s or *Eustachius*'s physicks; yet I shall not scruple to say, that you should as little think, that there are no more mysteries in the books of scripture, besides those, that the school-divines and vulgar commentators have taken notice of, and unfolded; as, that there are no other mysteries in the book of nature, than those, which the same schoolmen (who have taken upon them to interpret *Aristotle* and nature too) have observed and explained. All the fine things, that poets, orators, and even lovers have hyperbolically said in praise of the beauty of eyes, will nothing near so much recommend them to a philosopher's esteem, as the sight of one eye skilfully dissected, or the unadorned account given of its structure, and the admirable uses of its several parts, in *Scheiner*'s *Oculus*, and *Des Cartes*'s excellent dioptricks. And though I do not think myself bound to acquiesce in, and admire every thing, that is proposed, as mysterious and rare by many interpreters and preachers; yet I think, I may safely compare several things in the books we call the scripture, to several others in that of nature, in (at least) one regard. For, though I do not believe all the wonders, that *Pliny*, *Ælian*, *Porta*, and other writers of that stamp, relate of the generation of animals; yet by perusing such faithful and accurate accounts, as sometimes *Galen de usu Partium*, sometimes *Vesalius*, sometimes our *Harvey* (*de Ovo*) and our later anatomists, and sometimes other true naturalists, give of the generation of animals, and of the admirable structure of their bodies, especially those of men, and such other parts of zoology, as *Pliny*, and the other writers I named with him, could make nothing considerable of; by perusing these, I say, I receive more pleasure and satisfaction, and am induced more to admire the works of nature, than by all their romantick and superficial narratives. And thus (to apply this to our present subject) a close and critical account of the more vailed and pregnant parts of scripture, and theological matters, with such reflections on them, as their nature and collation would suggest to a philosophical, as well as critical speculator, would far better please a rational considerer, and give him a higher, as well as a better grounded veneration, for the things explained, than a great many of those slighter or ill founded remarks, wherewith the expositions and discourses of superficial writers, though never so florid or witty, gain the applause of the less discerning sort of men.

AND here, on this occasion, I shall venture to add, that I despair not, but that a farther use may be made of the scripture, than either our divines or philosophers seem to have thought on. Some few theologues indeed have got the name of Supralapsarians, for venturing to look back beyond the fall of *Adam* for God's decrees of election and reprobation. But besides that, their boldness has been disliked by the generality of divines, as well as other Christians, the object of their speculation is much too narrow to be any thing near and adequate to such an hypothesis as I mean. For methinks, that the Encyclopedia's and Pansophia's, that even men of an elevated genius have aimed at, are not diffused enough to comprehend all, that the reason of a man, improved by philosophy, and elevated by the revelations already extant in the scripture, may, by the help of free ratiocination, and the hints contained in those pregnant writings (with those assistances of God's spirit, which he is still ready to vouchsafe to them, that duly seek them,) attain unto in this life. The gospel comprises indeed, and unfolds the whole mystery of man's redemption, as far forth as it is necessary to be known for our salvation: Acts xx. 27. and the corpuscularian or mechanical philosophy strivesto deduce all the phænomena of nature from adiaphorous matter, and local motion. But neither the fundamental doctrine of Christianity, nor that of the powers and effects of matter and motion, seems to be more than an epicycle (if I may so call it) of the great and universal

universal system of God's contrivances, and makes but a part of the more general theory of things, knowable by the light of nature, improved by the information of the scriptures: so that both these doctrines, though very general, in respect of the subordinate parts of theology and philosophy, seem to be but members of the universal hypothesis, whose objects I conceive to be the nature, counsels, and works of God, as far as they are discoverable by us (for I say not to us) in this life.

For those, to whom God has vouchsafed the privilege of mature reason, seem not to enlarge their thoughts enough, if they think, that the omniscient and almighty God has bounded the operations of his power, and wisdom, and goodness, to the exercise, that may be given them for some ages, by the production and government of matter and motion, and of the inhabitants of the terrestrial globe, which we know to be but a physical point in comparison of that portion of universal matter, which we have already discovered.

For I account, that there are four grand communities of creatures, whereof things meerly corporeal make but one; the other three, differing from these, are distinct also from one another. Of the first sort are the race of mankind, where intellectual beings are vitally associated with gross and organical bodies. The second are dæmons, or evil angels; and the third good angels; (whether in each of those two kinds of spirits, the rational beings be perfectly free from all union with matter, though never so fine and subtile; or whether they be united to vehicles, not gross, but spirituous, and ordinarily invisible to us.)

Nor may we think, because angels and devils are two names quickly uttered, and those spirits are seldom or never seen by us, there are therefore but few of them, and the speculation of them is not considerable. For, as their excellency is great, (as we shall by and by shew) so for their number, they are represented in scripture as an heavenly host, standing on the right and left hand of the throne of God. And of the good angels, our saviour speaks of having
Matth. xxvi. 53. more than twelve legions of them at his command. Nay, the prophet *Daniel* saith, that to
Dan. vii. 10. the ancient of days, no less than millions ministred unto him, and hundreds of millions stood before him. And of the evil angels, the
Mark v. 9. gospel informs us, that enough to call them a
Luke viii. 30. legion (which, you know, is usually reckoned, at a moderate rate, to consist of betwixt six and seven thousand) possessed one single man. For my part, when I consider, that matter, how vastly extended, and how curiously shaped soever, is but a brute thing, that is only capable of local motion, and its effects and consequents on other bodies, or the brain of man, without being capable of any true, or at least any intellectual perception, or true love or hatred; and when I consider the rational soul as an immaterial and immortal being, that bears the image of its divine maker, being endowed with a capacious intellect, and a will, that no creature can force: I am by these considerations disposed to think the soul of man a nobler and more valuable being, than the whole corporeal world; which though I readily acknowledge it to be admirably contrived, and worthy of the almighty and omniscient author, yet it consists but of an aggregate of portions of brute matter, variously shaped and connected by local motion (as dough, and roles, and loaves, and cakes, and vermicelli, wafers, and pie-crust, are all of them diversified meal; but without any knowledge either of their own nature, or of that of their author, or of that of their fellow-creatures.) And as the rational soul is somewhat more noble and wonderful, than any thing merely corporeal, how vast soever it can be, and is of a more excellent nature, than the curiousest piece of mechanism in the world, the human body; so to enquire what shall become of it, and what fates it is like to undergo hereafter, does better deserve a man's curiosity, than to know what shall befal the corporeal universe, and might justly have been to *Nebuchadnezzar* a more desirable Dan. ii.
part of knowledge, than that he was so trou- 31, 32, &c.
bled for want of, when it was adumbrated to him in the mysterious dream, that contained the characters and fates of the four great monarchies of the world. And as man is intrusted with a will of his own, whereas all material things move only as they are moved, and have no self-determining power, on whose account they can resist the will of God; and as also of angels, at least some orders of them, are of a higher quality (if I may so speak) than human souls; so it is very probable, that in the government of angels, whether good or bad, that are intellectual voluntary agents, that is required and employed far grater displays of God's wisdom, power, and goodness, in the guidance of adiaphorous matter; and the method of God's conduct in the government of these, is a far nobler object for men's contemplation, than the laws, according to which the parts of matter hit against, and justle one another, and the effects or results of such motions.

And accordingly we find in scripture, that, whereas about the production of the material world, and the setting of the frame of nature, God employed only a few commanding words, which speedily had their full effects; to govern the race of mankind, even in order to their own happiness, he employed not only laws and commands, but revelations, miracles, promises threats, exhortations, mercies, judgments, and divers other methods and means; and yet oftentimes, when he might well say, as he did once by his prophet, "What could I have "done more to my vineyard, that I have not "done it?" he had just cause to expostulate as he did in the same place, "Wherefore, when "I looked, that it should bring forth grapes, "brought it forth wild grapes?" and to complain of men, as by that very prophet he did even of *Israel*, "I have spread out my hands Isa. lxv. 2.
"all the day to a rebellious people." But not to wander too far in this digression; what we have said of men, may render it probable, that the grand attributes of God are more signally exercised, and made more conspicuous in the making and governing of each of the three in-

tellectual communities, than in the framing and upholding the community of mere bodily things. And since all immaterial substances are for that reason naturally immortal, and the universal matter is believed so too, possibly those revolutions, that will happen after the day of judgment, wherein though probably not the matter, yet that state and constitution of it, on whose account it is this world will be destroyed, and make way for quite new frames and sets of things corporeal, and the beings, that compose each of these intellectual communities, will, in those numberless ages they shall last, travel through I know not how many successive changes and adventures; perhaps, I say, these things will no less display, and bring glory to the divine attributes, than the contrivance of the world, and the oeconomy of man's salvation, though these be (and that worthily) the objects of the naturalists and the divines contemplation. And there are some passages in the prophetical part of the scripture, and especially in the book of the Apocalypse, which, as they seem to intimate, that as God will perform great and noble things, which mechanical philosophy never reached to, and which the generality of divines seem not to have thought of; so divers of those great things may be, in some measure, discovered by an attentive searcher into the scriptures, and that so much to the advantage of the devout indagator, that St. *John*, near the beginning of his *Revelations*, pronounces them happy, that read the matters
Rev. i. 3. contained in this prophecy, and * *observe* the things written therein. Which implies, that by heedful comparing together the indications couched in those prophetick writings, with events and occurrences in the affairs of the world, and the church, we may discover much of the admirable oeconomy of providence in the governing of both: and I am prone to think, the early discoveries of such great and important things to be, in God's account, no mean vouchsafements, not only because the title of happy is here given to him, that attains them, but because of the two persons, to whom the great discoveries of this kind were made, I mean, the prophet *Daniel* and St. *John*; the first is by the angel said to be, on that account, a person highly favoured; and the other is, in the gospel, represented as our Saviour's beloved disciple. And you will the more easily think the foreknowledge of the divine dispensations gatherable from scripture to be highly valuable, if you consider, that, according to St. *Paul*, those very angels, that are called principalities and powers in heavenly places, learnt by the church some abstruse points of the manifold wisdom of God. But I must no longer indulge speculations, that would carry my curiosity beyond the bounds of time itself, and therefore beyond those, that ought to be placed to this occasional excursion.

Πολυποίκιλ© σοφία τȣ̃ Θεȣ̃. Ephes. iii. 10.

And yet, as on the one side, I shall not allow myself the presumption of framing conjectures about those remote dispensations, which will not, most of them, have a beginning before this world shall have an end; so on the other side I would not discourage you, or any pious enquirer, from endeavouring to advance in the knowledge of those attributes of God, that may successfully be studied, without prying into the secrets of the future.

AND here, Sir, let me freely confess to you, that I am apt to think, that if men were not wanting to God's glory, and their own satisfaction, there would be far more discoveries made, than are yet attained to, of the divine attributes. When we consider the most simple, or uncompounded essence of God, we may easily be persuaded, that what belongs to any of his attributes (some of which thinking men generally admire) must be an object of enquiry exceeding noble, and worthy of our knowledge. And yet the abstruseness of this knowledge is not in all particulars so invincible, but that I strongly hope, a philosophical eye, illustrated by the revelations extant in the scripture, may pierce a great deal farther than has yet been done, into those mysterious subjects, which are too often (perhaps out of a mistaken reverence) so poorly handled by divines and schoolmen, that not only what they have taught, is not worthy of God (for that is a necessary, and therefore excusable deficiency) but too frequently it is not worthy of men, I mean, of rational creatures, that take upon them to treat of such high points, and instruct others about them. And I question not but your friend will the less scruple at this, if he call to mind those new and handsome notions about some of the attributes of God, that his master *Cartesius*, though but moderately versed in the scriptures, has presented us with. Nor do I doubt, but that a much greater progress might be made in the discovery of subjects, where, though we can never know all, we may still know farther, if speculative genius's would propose to themselves particular doubts and enquiries, about particular attributes, and frame and examine hypotheses, establish theorems, draw corollaries; and (in short) apply to this study the same sagacity, assiduity, and attention of mind, which they often employ about enquiries of a very much inferior nature; insomuch, as *Des Cartes* (how profound a geometrician soever he were) confesses in one of his epistles, that he employed no less than six weeks to find the solution of a ploblem or question of *Pappus*. And *Pythagoras* was so addicted to, and concerned for geometrical speculations, that when he had found that famous proposition, which makes the 47th in *Euclid*'s First Book, he is recorded to have offered a hecatomb, to express his joy and gratitude for the discovery: which yet was but of one property of one sort of right-lined triangles. And certainly, if Christian philosophers did rightly estimate, how noble and fertile subjects the divine

* To render the original word (*observe*, or) *watch*, rather than *keep*, seems more congruous to the sense of the text, and is a criticism suggested to me by an eminent mathematician, as well as divine, who took notice, that the word τηρεῖ is used by the Greeks, as a term of art to express the astronomical observation of eclipses, planetary conjunctions, oppositions, and other celestial phænomena.

divine attributes are, they would find in them wherewithal to exercise their best parts, as well as to recompence the employment of them. But because, what I would dissuade, does not, perhaps, proceed only from laziness, but from a mistake; as if there were little to be known of so incomprehensible an object as God, save, that in general, all his attributes are like himself, infinite, and consequently not to be fully known by human understandings, because they are finite; I shall add, that though it be true, that by reason of God's infinity, we cannot comprehend him, that is, have a full and adequate knowledge of him; yet, we may not only know very many things concerning him, but, which is more, may make an endless progress in that knowledge. As no doubt, *Pythagoras* (newly mentioned) knew very well what a triangle was, and was acquainted with divers of its properties and affections, before he discovered that famous one. And though, since him, *Euclid*, *Archimedes*, and other geometricians have demonstrated, I know not how many other affections of the same figure, yet they have not to this day exhausted the subject: and possibly I (who pretend not to be a mathematician) may now and then, in managing certain æquations I had occasion for, have lighted upon some theorems about triangles, that occurred not to any of them. The divine attributes are such fruitful themes, and so worthy of our admration, that the whole fabrick of the universe, and all the phænomena exhibited in it, are but imperfect expressions of God's wisdom, and some few of his other attributes. And I do not much marvel, that the angels themselves are represented in scripture, as employed in adoring God, and admiring his perfections. For even they being but finite, can frame but inadequate conceptions of him; and consequently must endeavour, by many of them, to make amends for the incompleatness of every one of them; which yet they can never but imperfectly do. And yet God's infinity can but very improperly be made a discouragement of our enquiries into his nature and attributes. For (not now to examine, whether infinity, though expressed by a negative word, be not a positive thing in God) we may, notwithstanding his infinity, discover as much of him as our nature is capable of knowing: and what harm is it to him, that is drinking in a river, that he cannot drink up all the water, if he have liberty fully to quench his thirst, and take in as much liquor as his stomach can contain? Infinity therefore should not hinder us from a generous ambition to learn as much as we can of an object, whose being infinite does but make our knowledge of it the more noble and desirable, which indeed it is, in such a degree, that we need not wonder, that the angels are represented as never weary of their employment of contemplating and praising God. For, as I lately intimated, that they can have but inadequate ideas of those boundless perfections, and by no number of those ideas can arrive to make amends for the incompleatness of them; so it need not seem strange, that in fresh discoveries of new parts (if I may so call them) of the same object, it being such a one, they should find nobler and happier entertainments, than any where else variety could afford them.

Isa. vi. 2, 3. Luke ii. 13, 14. Rev. v. 11, 12.

SECTION II.

HAVING thus taken notice of some particulars of those many, which may be employed to shew, how noble the objects are, that theology proposes to be contemplated; I now proceed to some considerations, that may make us sensible, how great an obligation there lies on us, to addict ourselves to the study of them.

YET, of the particulars, whereon this obligation may be grounded, I shall now name but two, they being indeed comprehensive ones, obedience, and gratitude.

AND first, let me represent, that it needs not, I suppose, be solicitously proved, that it is the will and command of God, that men should learn those truths, that he has been pleased to teach, whether concerning his nature or attributes, or the way, wherein he will be served and worshipped by man. For if we had not injunctions of scripture to that purpose, yet your friend is too rational a man to believe, that God would so solemnly cause his truths to be published to mankind, both by preaching and writing, without intention to oblige those (at least) that have the capacity and opportunity to enquire into some of them; and if it appear to be his will, that a person so qualified should search after the most important truths, that he hath revealed, it cannot but be their duty to do so. For though the nature of the thing itself did not lay any obligation on us, yet the authority of him, that commands it, would; since, being the supreme and absolute Lord of all his creatures, he has as well a full right to make what laws he thinks fit, and enjoin what service he thinks fit, as a power to punish those, that either violate the one, or deny the other; and accordingly it is very observable, that before *Adam* fell, and had forfeited his happy state, by his own transgression, he not only had a law imposed upon him, but such a law, as, being about a matter itself indifferent, (for so it was to eat, or not to eat, of the tree of life, as well as of any other,) derived its whole power of obliging from the mere will and pleasure of the law-giver. Whence we may learn, that man is subject to the laws of God, not as he is obnoxious to him, but as he is a rational creature, and that the thing, that is not a duty in its own nature, may become an indispensible one, barely by its being commanded. And indeed, if our first parent, in the state of innocency and happiness, wherein he tasted of God's bounty, without as yet standing in need of his mercy, was most strictly obliged, out of mere obedience, to conform to a law, the matter of which was indifferent in itself; sure we, in our lapsed condition, must be under a high obligation to obey the declared will of God, whereby we are enjoined to study his truths, and perform that, which has so much of intrinsick goodness in it, that

Gen. ii. 16, 17.

that it would be a duty, though it were not commanded; and has ſuch recompences propoſed to it, that it is not more a duty, than it will be an advantage.

But it is not only obedience, and intereſt, that ſhould engage us to the ſtudy of divine things, but gratitude; and that exacted by ſo many important motives, that he, who ſaid, *ingratum ſi dixeris, omnia dixeris*, could not think ingratitude ſo much worſe than ordinary vices, as a contempt of the duty I am preſſing would be worſe than an ordinary ingratitude.

It were not difficult, on this occaſion, to manifeſt, that we are extremely great debtors unto God, both as he is the author, and the preſerver of our very beings; and as he (immediately, or mediately,) fills up the meaſure of thoſe continual benefits, with all the prerogatives, and other favours we do receive from him, as men; and the higher bleſſings, which (if we are not wanting to ourſelves,) we may receive from him, as Chriſtians.

But to ſhew, in how many particulars, and to how high a degree, God is our benefactor, were to launch out into two immenſe a ſubject; which it were the leſs proper for me to do, be-
Seraph cauſe I have, in other papers, diſcourſed of
Love. thoſe matters already. I will therefore ſingle out a motive of gratitude, which will be peculiarly pertinent to our preſent purpoſe. For, whereas your friend does ſo highly value himſelf upon the ſtudy of natural philoſophy, and deſpiſes not only divines, but ſtateſmen, and even the learnedeſt men in other parts of philoſophy and knowledge, becauſe they are not verſed in phyſicks; he owes to God that very
Pſal. xxxii. ſkill, among many other vouchſafements. For
9. it is God, who *made man unlike the horſe and the mule, who have no underſtanding*, and endowed him with that noble power of reaſon, by the exerciſe of which, he attains to whatever knowledge he has of natural things above the beaſts that periſh. For that may juſtly be applied to our other acquiſitions, which *Moſes*, by God's appointment, told the Iſraelites concerning the acquiſts of riches; where he bids the people beware, that when their herds, and their flocks, and other treaſures were multiplied, their heart be not lifted up, and prompt
Deut. viii. them to ſay, "My power, and the might of my
10, 11, 12, "hand, hath gotten me this wealth." But, (ſubjoins that excellent perſon, as well as matchleſs
13, 14, law-giver,) Thou ſhalt remember thy Lord thy God, for it is he, that giveth thee power
18. to get wealth. But to make men rational creatures, is not all God has done towards the making them philoſophers. For, to the knowledge of particular things, objects are as well requiſite as faculties; and if we admit the probable opinion of divines, who teach us, that the angels were created before the material world,
Job as being meant by thoſe *ſons of God*, and
xxxviii. 5, *morning ſtars*, that, with glad ſongs and accla-
6, 7. mations, celebrated the foundations of the earth; we muſt allow, that there were many creatures endowed with, at leaſt, as much reaſon as your friend, who yet were unacquainted with the myſteries of nature, ſince ſhe herſelf had not yet received a being. Wherefore, God having as well made the world, as given man the faculties, whereby he is enabled to contemplate it; naturaliſts are as much obliged to God for their knowledge, as we are for our intelligence to thoſe, that write us ſecrets in cyphers, and teach us the ſkill of decyphering things ſo written; or to thoſe, who write what would fill a page in the compaſs of a ſingle penny, and preſent us to boot a microſcope to read it. And as the naturaliſt hath peculiar inducements to gratitude, for the endowment of knowledge; ſo ingenuity lays this peculiar obligation on him to expreſs his gratitude, in the way I have been recommending, that it is one of the acceptableſt ways it can be expreſſed in; eſpecially ſince, by this way, philoſophers may not only exerciſe their own gratitude towards God, but procure him that of others. How pleaſing men's hearty praiſes are to God, may appear, among other things, by what is ſaid and done by that royal poet, whom God was pleaſed to declare a man after his own heart; for he introduces God pronouncing, "Whoſo offereth Pſal. l. 23.
"praiſe, glorifieth me;" where the word our interpreters render offereth, in the *Hebrew*, ſignifies to ſacrifice; with which agrees, that elſewhere thoſe, that pay God their praiſes, are ſaid to ſacrifice to him "the calves of their lips." Hoſ. xiv.
And that excellent perſon, to whom God vouch- 2.
ſafed ſo particular a teſtimony, was ſo aſſiduous in this exerciſe, that the book which we, following the *Greek*, call *Pſalms*, is, in the original, from the things it moſt abounds with, called *Sepher Tehillim*, i. e. the book of praiſes. And to let you ſee, that many of his praiſes were ſuch, as the naturaliſt may beſt give, he exclaims, in one place, "How manifold are thy Pſal. civ.
"works, O Lord? how wiſely haſt thou made 24.
"them," (as *Junius* and *Tremellius* render it, and the *Hebrew* will bear;) and elſewhere, "The Pſal xix.
"heavens declare the glory of God, and the fir- 1.
"mament ſheweth his handy-work, *&c.*" Again, in another place, "I will praiſe thee, becauſe I Pſal.
"am fearfully and wonderfully made. Marvel- cxxxix.
"lous are thy works, and that my ſoul knoweth 14.
"right well." And not content with many of the like expreſſions, he does ſeveral times, in a devout tranſport, and poetical ſtrain, invite the heavens, and the ſtars, and the earth, and the ſeas, and all the other inanimate creatures, to join with him in the celebration of their common maker. Which, though it ſeem to be merely a poetical ſcheme, yet, in ſome ſort, it might become a naturaliſt, who, by making out the power, wiſdom, and goodneſs of the Creator; and by reflecting thence on thoſe particulars, wherein thoſe attributes ſhine, may, by ſuch a devout conſideration of the creatures, make them, in a ſenſe, join with him in glorifying their author.

In any other caſe, I dare ſay, your friend is not ſo ill-natured, but that he would think it an unkind piece of ingratitude, if ſome great and excellent prince, having freely and tranſcendently obliged him, he ſhould not concern himſelf to know what manner of man his benefactor is; and ſhould not be ſolicitous to inform himſelf of thoſe particulars, relating to the perſon and affairs of that obliging monarch, which

which were not only in themſelves worthy of any man's curioſity, but about which the prince had ſolemnly declared, he was very deſirous to have men inquiſitive. And ſurely it is very diſingenious, to undervalue, or neglect the knowledge of God himſelf, for a knowledge, which we cannot attain without him, and by which he deſigned to bring us to that ſtudy we neglect for it: which is not only, not to uſe him as a benefactor, but as if he meant to puniſh him, (if I may ſo ſpeak,) for having obliged us, ſince we ſo abuſe ſome of his favours, as to make them inducements to our unthankful diſregard of his intentions in the reſt. And this ingratitude is the more culpable, becauſe the laws of ingenuity, and of juſtice itſelf, charge us to glorify the maker of all things viſible, not only upon our own account, but upon that of all his other works. For, by God's endowing of none but man here below, with a reaſonable ſoul, not only he is the ſole viſible being, that can return thanks and praiſes in the world, and thereby is obliged to do ſo, both for himſelf, and for the reſt of the creation; but it is for man's advantage, that God has left no other viſible beings in the world, by which he can be ſtudied and celebrated. For reaſon is ſuch a ray of divinity, that, if God had vouchſafed it to other parts of the univerſe beſides man, the abſolute empire of man over the reſt of the world muſt have been ſhared, or abridged. So that he, to whom it was equally eaſy to make creatures ſuperior to man, (as the ſcripture tells us of legions, and myriads of angels,) as to make them inferior to him, dealt ſo obligingly with mankind, as rather to truſt (if I may ſo ſpeak) our ingenuity, whether he ſhall reap any celebrations from the creatures we converſe with, than leſſen our empire over them, or our prerogatives above them.

But I fear, that, notwithſtanding all the excellency of revealed truths, and conſequently of that only authentic repoſitory of them, the ſcripture, you, as well as I, have met with ſome (for I hope there are not many) virtuoſi, that think to excuſe the neglect of the ſtudy of it, by alledging, that, to them, who are laymen, not eccleſiaſticks, there is required to ſalvation the explicit knowledge but of very few points, which are ſo plainly ſummed up in the apoſtles creed, and are ſo often and conſpicuouſly ſet down in the ſcripture, that one needs not much ſearch, or ſtudy, to find them there.

In anſwer to this allegation, I readily grant, 1 Tim. ii. 4. that through the great goodneſs of God, who is willing to have all men ſaved, and come to the knowledge of the truth, that is neceſſary to be ſo, there are much fewer articles abſolutely neceſſary to be by all men diſtinctly believed, than may be met with in divers long confeſſions of faith, ſome of which have, I fear, leſs promoted knowledge than impaired charity. But then it may be alſo conſidered, 1. That it is not ſo eaſy for a rational man, that will trouble himſelf to enquire no farther than the apoſtles creed, to ſatisfy himſelf upon good grounds, that all the fundamental articles of Chriſtianity are contained in it. 2. That the creed propoſes only the *credenda*, not the *agenda* of religion; whereas the ſcriptures were Joh. xiij. 7. Heb. v. 9. deſigned, not only to teach us what truths we are to believe, but by what rules we are to live; the obedience to the laws of Chriſtianity being as neceſſary to ſalvation, as the belief of its myſteries. 3. That beſides the things, which are abſolutely neceſſary, there are ſeveral, that are highly uſeful, to make us more clearly underſtand, and more rationally and firmly believe, and more ſteadily practice, the points, that are neceſſary. 4. And ſince, whether or no thoſe words of our Saviour to the Jews, ἐρευνᾶτε τὰς γραφὰς *, be to be rendered in the Joh. v. 39. imperative or indicative mode; St. *Paul* would have the word of Chriſt to dwell richly in us, (by which, whether he mean the holy ſcriptures Coloſſ. iii. 16. then extant, or the doctrine of Chriſt, is not here material;) thereby teaching us, that ſearching into the matters of religion may become neceſſary, as a duty, though it were not otherwiſe neceſſary, as a means of attaining ſalvation. And indeed it is far more pardonable to want or miſs the knowledge of truths, than to deſpiſe or neglect it. And the goodneſs of God to illiterate or miſtaken perſons is to be ſuppoſed meant in pity to our frailties, not to encourage our lazineſs; nor is it neceſſary, that he, that pardons thoſe ſeekers of his truths, that miſs them, ſhould excuſe thoſe deſpiſers, that will not ſeek them.

But whether or no by this deſigned neglect of theology the perſons, I deal with, do ſufficiently conſult their own ſafety, I doubt they will not much recommend their ingenuity. For to have received from God a greater meaſure of intellectual abilities than the generality of Chriſtians, and yet willingly to come ſhort of very many of them, in the knowledge of the myſteries, and other truths of Chriſtianity, which he often invites us, if not expreſsly commands, to ſearch after, is a courſe, that will not reliſh of over-much gratitude. Is it a piece of that, and of ingenuity, to receive one's underſtanding and one's hopes of eternal felicity from the goodneſs of God, without being ſollicitous of what may be known of his nature and purpoſes, by ſo excellent a way as his own revelation of them? to diſpute anxiouſly about the properties of an atom, and be careleſs about the enquiry into the attributes of the great God, who Prov. xxvi. 10. formed all things; to inveſtigate the ſpontaneous generation of ſuch vile creatures as inſects, than the myſterious generation of the adorable Son of God; and, in a word, to be more concerned to know every thing, that makes a corporeal part of the world, than the divine and incorporeal author of the whole?

And then, is it not, think you, a great piece of reſpect, that theſe men pay to thoſe truths, which God thought fit to ſend ſometimes prophets and apoſtles, ſometimes angels, and ſometimes his only Son himſelf, to reveal, that ſuch truths are ſo little valued by them, that rather than take the pains to ſtudy them,

* Search, or, you ſearch the Scriptures.

they will implicitly, and at adventures believe, what that society of Christians, they chance to be born and bred in, have, truly or falsely, delivered, concerning them? And does it argue a due regard to points of religion, that those, who would not believe a proposition in staticks, perhaps about a mere point, the centre of gravity, or in geometry, about the properties of some nameless curve line, or some such other things, (which to ignore, is usually not a blemish, and about which to be mistaken, is more usually without danger) should yet take up the articles of faith, concerning matters of great and everlasting consequence, upon the authority of men, fallible as themselves, when satisfaction may be had without them from the infallible word of
Acts xvii. 11. God? in this very unlike those Bereans, whom the Evangelist honours with the title of Noble, that when the doctrines of the gospel were proposed to them, "they searched the scriptures "daily, whether those things were so."

AGAIN, if a man should refuse to learn to read any more, than just as much as may serve his turn, by intituling him to the benefit of the clergy, to save him from hanging, would these men think so small a measure of literature, as he had acquired on such an account, could prove that man to be a lover of learning; and yet a neglecter of the study of all not absolutely necessary-divine truths, during one's life, because the belief of the articles of the creed may make a shift to keep him from being doomed to hell for ignorance after his death, will not by (what in a learned man must be) so pitiful a degree of knowledge be much better intitled to that ingenuous love of God and his truths, that becomes a rational creature and a Christian.

THE ancient prophets, though honoured by God with direct illuminations, were yet
1 Pet. i. 10, 11. very solicitous to find out and learn the very circumstances of the evangelical dispensations, which yet they did not know. And some of the gospel mysteries are of so noble and excel-
1 Pet. i. 12. lent a nature, that "the angels themselves de-"sire to look into them." And though all the evangelical truths are not precisely necessary to be known, it may be both a duty not to despise the study of them, and a happiness to employ ourselves about it. It was the earnest prayer of a great king, and no less a pro-
Psal. cxix. 18. phet, that his eyes might be opened to behold (not the obvious and necessary truths, but) the wondrous things of God's law. He is pronounced happy in the beginning of the
Rev. i. Apocalypse, that reads and observes the things contained in that dark and obscure part of scripture. And it is not only those truths, that make articles of the creed, but divers other doctrines of the gospel, that Christ himself judged worthy to be concluded with this epi-
Mat. xi. 15. Mark iv. 9, 23. Luke viii. 8. phonema, "He that hath ears to hear, let him "hear;" on which the excellent *Grotius* makes this just paraphrase, *Intellectus nobis à Deo potissimum datus est, ut eum intendamus documentis ad pietatem pertinentibus.*

SECTION III.

I COME now to our third and last inducement to the study of divine things, which consists in, and comprises the advantages of that study, which do as much surpass those of all other contemplations, as divine things transcend all other objects. And indeed, the utility of this study is so pregnant a motive, and contains in it so many invitations, that your friend must have as little sense of interest, as of gratitude, if he can neglect such powerful and such engaging invitations.

FOR, in the first place, theological studies ought to be highly endeared to us, by the delightfulness of considering such noble and worthy objects, as are therein proposed.

THE famous answer given by an excellent philosopher, who being asked, what he was born for? replied, "To contemplate the sun," may justly recommend their choice, who spend their time in contemplating the maker of the sun, to whom that glorious planet it self is but a shadow. And perhaps that philosopher failed more in the instance than in the notion: for his answer implies, that man's end and happiness consist in the exercise of his noblest faculties on the noblest objects. And surely the seat of formal happiness being the soul, and that happiness consequently consisting in the operations of her faculties; as the supreme faculty of the mind is the understanding, so the highest pleasures may be expected from the due exercise of it upon the sublimest and worthiest objects. And therefore I wonder not, that though some of the school-men would assign the will a larger share in man's felicity, than they will allow the intellect; yet the generality of them are quite of another mind, and ascribe the preheminence, in point of felicity, to the superior faculty of the soul. But whether or no this opinion be true in all cases, it may, at least, be admitted in ours: for the chief objects of a Christian philosopher's contemplation being as well the infinite goodness, as the other boundless perfections of God, they are naturally fitted to excite in his mind an ardent love of that adorable Being, and those other joyous affections and virtuous dispositions, that have made some men think happiness chiefly seated in the will. But having intimated thus much by the way, I pass on to add, that the contentment afforded by the assiduous discovery of God and divine mysteries has so much of affinity with the pleasures, that shall make up men's blessedness in heaven it self, that they seem rather to differ in degree than in kind. For the happy state even of angels is by our Saviour represented by this employment, "That they continually see the face of his father, who is in heaven." And the same infallible teacher, intending elsewhere to express the celestial joys, that are reserved for those, who for their sake denied themselves sensual pleasures, employs the vision of God as an emphatical periphrase of felicity, "Blessed,

said

Matth. v. 8. said he, are the pure in heart, for they shall see God." And as *Aristotle* teaches, that the soul doth after a sort become that, which it speculates, St. *Paul* and St. *John* assure us, that God is a transforming object, and that in heaven "we shall be like him, for (or, because) 1 John iii. 2. Ὅτι. "we shall see him, as he is." And though I readily admit, that this beatifick vision of God, wherein the understanding is the proper instrument, includes divers other things, which will concur to the complete felicity of the future life; yet I think, we may be allowed to argue, that that ravishing contemplation of divine objects will make no small part of that happy estate, which in these texts take its denomination from it.

I have above intimated, that the scripture attributes to the angels themselves transports of wonder and joy upon the contemplation of God, and the exercises they consider of his wisdom, justice, or some other of his attributes. But lest, in referring you to the angels, you should say, that I do in this discourse lay aside the person of a naturalist, in favour of divines; I will refer you to *Des Cartes* himself, whom I am sure your friend will allow to have been a rigid philosopher, if ever there were any. Thus then speaks he in that treatise, where he thinks he employs a more than mathematical rigor; and where he was obliged to utter those (I had almost said passionate) words, I am going to cite from him, only by the impressions made on him by the transcendent excellency of the object he contemplated, Medit. tertia sub finem. *Sed priusquam* (says he) *hoc diligentius examinem, simulque in alias veritates, quæ inde colligi possunt, inquiram, placet hic aliquandiu in ipsius Dei contemplatione immorari, ejus attributa apud me expendere, & immensi hujus luminis pulchritudinem, quantum caligantis ingenii mei acies ferre poterit, intueri, admirari, adorare. Ut enim in hac sola Divinæ Majestatis contemplatione summam alterius vitæ felicitatem consistere fide credimus; ita etiam jam ex eadem, licet multo minus perfecta, maximam, cujus in hac vita capaces simus, voluptatem, percipi posse experimur.*

But as high a satisfaction as the study of divine things affords by the nobleness of its object, the contentment is not much inferior, that accrues from the same study, upon the score of the sense of a man's having in it performed his duty. To make actions of this nature satisfactory to us, there is no need, that the things we are employed about, should in themselves be excellent or delightful; the inward gratulations of conscience for having done our duties is able to gild the bitterest pills, and like the wood, that grew by the waters of *Marah*, to correct and sweeten that liquor, Exod xv. 25. which before was the most distasteful. Those ancient Pagan heroes, whose virtues may make us blush, being guided but by natural reason, and innate principles of moral virtues, could find the most difficult and most troublesome duties, upon the bare account of their being duties, not only tolerable, but pleasant. And though to deny some lusts be, in our Saviour's esteem, no less uneasy, than for a man to pluck out his right eye, or cut off his right Matth. v. hand; yet even ladies have with satisfaction 29, 30. chosen, not only to deny themselves the greatest pleasures of the senses, but to sacrifice the seat of them, the body it self, to preserve the satisfaction of being chaste. Nor are they only the dictates of obedience, that we comply with in this study, but those of gratitude; and that is a virtue, that has so powerful an ascendant upon ingenuous minds, that those, whose principles and aims were not elevated by religion, have, in acknowledgment to their parents and their country, courted the greatest hardships, and hazards, and sufferings, as if they were as great delights and advantages. And a grateful person spends no part of his life to his greater satisfaction, than that, which he ventures or employs for those to whom he is obliged for it; and oftentimes finds a greater contentment even in the difficultest acknowledgments of a favour, than he did in receiving of it.

Another advantage, and that no mean one, that may accrue from the contemplation of theological truths, is, the improvement of the contemplator himself in point of piety and virtue. For, as the gospel is stiled, the mystery of godliness; and St. *Paul* elsewhere calls 1 Tim. what it teaches, the truth, which is according iii. 16. to godliness, that is, a doctrine framed and Tit. i. 1. fitted to promote the interest of piety and virtue in the world: so this character and encomium belongs (though perhaps not equally) to the more retired truths discovered by speculation, as well as to those more obvious ones, that are familiarly taught in catechisms and confessions of faith. I would by no means lessen the excellency and prerogatives of fundamentals; but, since the grand and noblest engagements to piety and virtue are a high veneration for God and his Christ, and an ardent love of them; I cannot but think, that those particular enquiries, that tend to make greater discoveries of the attributes of God, of the nature, and offices, and life of our saviour, and of the wisdom and goodness they have displayed in the contrivance and effecting of man's redemption, do likewise tend to encrease our admiration, and inflame our love, for the possessors of such divine excellencies, and the authors of such invaluable benefits. And as the brazen serpent, that was but a Numb. type of one of the gospel mysteries, brought xxi. 9. recovery to those, that looked up to it; so the mysteries themselves, being duly considered, have had a very sanative influence on many, that contemplated them. Nor is it likely, that he, that discerns more of the depth of God's wisdom and goodness, should not, *cæteris paribus*, be more disposed than others to admire him, to love him, to trust him, and so to resign up himself to be governed by him: which frame of mind both is itself a great part of the worship of God, and doth directly tend to the production and increase of those vertues, without the practice of which, the scripture plainly tells us, that we can neither obey God, nor express our love to him. And from this bettering of the mind by the study of theology, will

flow

flow (to add that upon the by) another benefit, namely, that by giving us a higher value for God and his truths, it will endear heaven to us, and so not only assist us to come thither, but heighten our felicity there.

I know it may be said, that the melioration of the mind is but a moral advantage. But give me leave to answer, that besides that it is such a moral advantage, as supposes an intellectual improvement, whose fruit it is, a moral benefit may be great enough, even in the judgment of a mere philosopher, and an Epicurean, to deserve as much study as natural philosophy itself. And that you may not think, that I speak this only, because I write in this epistle as a friend to divines, I will tell you, that *Epicurus* himself, who has now a-days so numerous a sect of naturalists to follow him, studied physicks, and writ so many treatises about them for this end, that by knowing the natural causes of thunder, lightning, and other dreadful phænomena, the mind might be freed from the disquieting apprehensions men commonly had, that such strange and formidable things proceeded from some incensed deity, and so might trouble the mind, as well as the air. This account I have been giving of *Epicurus*'s design, is but what seems plainly enough intimated by his own words, preserved us by *Laertius*, near the end of his physiological epistle to *Herodotus*, where recommending to him the consideration of what he had delivered about physical principles in general, and meteors in particular, he subjoins, *Si enim ab istis non discesserimus, tum id unde oritur perturbatio, quodque metum ingerit, recta cum ratione edisseremus, nosque ab ipsis eximemus.* And to this in the close of his meteorological epistle to *Pythocles*, his best interpreter, *Gassendus*, makes him speak constantly in these words, *Maxime veró dede teipsum speculationi principiorum, ex quibus constant omnia, & infinitatis naturæ, aliorumque his cohærentium. Insuper veró criteriorum, affectuumque animi, & scopum illius, in quem ista edisserentes collineavimus, attende, tranquillitatem intelligo statumque mentes imperturbatum.* But this is not all the testimony I can give you from *Epicurus* himself to the same purpose; for among his *Ratæ Sententiæ*, preserved us by *Laertius*, (himself reputed an Epicurean) I find one, that goes further; *Si nihil*, says he, *conturbaret nos quod suspicamur, veremurque ex rebus sublimibus, neque item quod ex ipsa morte, ne quando nimirum ad nos pertineat aliquid, ac nosse præterea possemus, qui Germani fines dolorum atque cupiditatum sint* (ὀυκ ἂν προσδεόμεθα φυσιολογίας) *nihil physiologiâ indigeremus.* Thus far the testimony of *Epicurus*, of whose mind though I am not at all, as to what he would intimate, "That physiology is either "proper to free the mind from the belief of "a provident deity, and the soul's immorta-"lity, or fit for no other considerable pur-"poses;" yet this use we may well make of these declarations, that in *Epicurus*'s opinion a moral advantage, that relates to the government of the affections, may deserve the pains of making enquiries into nature. And since it hence appears, that a mere philosopher, who admitted no providence, may think it worth his pains, to search into the abstrusest parts of physicks, and the difficultest phænomena of nature, only to ease himself of one troublesome affection, fear; it need not be thought unphilosophical, to prosecute a study, that will not only restrain one undue passion, but advance all virtues, and free us from all servile fears of the Deity; and tend to give us a strong and well-grounded hope in him; and make us look upon God's greatest power, not with terror, but with joy.

Diogenis Laertii lib. 10.

THERE is yet another advantage belonging to the study of divine truths, which is too great to be here pretermitted. For whereas there is scarce any thing more incident to us whilst we inhabit our (*batté chómer*) cottages of clay, and dwell in this vale of tears, than afflictions; it ought not a little to endear to us the newly mentioned study, that it may be easily made to afford us very powerful consolations in that otherwise uneasy state.

Job. iv. 19.

I know it may be said, that the speculations, about which the naturalist is busied, are as well pleasing diversions, as noble employments of the mind. And I deny not, that they are often so, when the mind is not hindered from applying it self attentively to them; so that afflictions slight and short may well be weathered out by these philosophical avocations; but the greater and sharper sort of afflictions, and the approaches of death, require more powerful remedies, than these diversions can afford us. For in such cases, the mind is wont to be too much discomposed, to apply the attention requisite to the finding a pleasure in physical speculations; and in sicknesses, the soul is oftentimes as indisposed to relish the pleasures of merely human studies, as the languishing body is to relish those meats, which at other times were delightful: and there are but few, that can take any great pleasure to study the world, when they apprehend themselves to be upon the point of being driven out of it, and in danger of losing all their share in the objects of their contemplation. It will not much qualify our sense of the burning heat of a fever, or the painful gripes of the cholick, to know, that the three angles of a triangle are equal to two right ones; or, that heat is not a real quality (as the schools would have it,) but a modification of the motion of the insensible parts of matter; and pain not a distinct, inherent quality in the things, that produce it, but an affection of the sentiment. The naturalist's speculations afford him no consolations, that are extraordinary in, or peculiar to the state of affliction; and the avocations, they present him with, do rather amuse the mind from an attention to lesser evils, than bring it any advantages to remove or compensate them, and so work rather in the nature of opiates, than of true cordials.

BUT now, if such a person as Dr. *N.* falls into adversity, the case is much otherwise; for we must consider, that when the study of divine things is such as it ought to be, though that in itself, or in the nature of the employment, be an act or exercise of reason; yet being

ing applied to, out of obedience, and gratitude, and love to God, it is upon the account of its motives, and its aim, an act of religion; and as it proceeds from obedience, and thankfulness, and love to God, so it is most acceptable to him; and upon the account of his own appointment, as well as goodness, is a most proper and effectual means of obtaining his favour; and then I presume, it will easily be granted, that he, who is so happy as to enjoy that, can scarce be made miserable by affliction. For not now to enter upon the common place of the benefits of afflictions to them, that love God, and to them, that are loved by him, it may suffice, that he, who (as the scripture speaks) knows our frame, and has promised those, that are his, that they shall not be overburdened, is disposed and wont to give his afflicted servants, both extraordinary comforts in afflictions, and comforts appropriated to that state. For though natural philosophy be like its brightest object, the stars, which, however the astronomer can with pleasure contemplate them, are unable, being mere natural agents, to afford him a kinder influence than usual, in case he be cast upon his bed of languishing, or into prison; yet the almighty and compassionate maker of the stars, being not only a voluntary, but the most free agent, can suit and proportion his reliefs to our necessities, and alleviate our heaviest afflictions by such supporting consolations, that not only they can never surmount our patience, but are oftentimes unable so much as to hinder our joy; and when death, that king of terrors, presents itself, whereas the mere naturalist sadly expects to be deprived of the pleasure of his knowledge, by losing those senses and that world, which are the instruments and the objects of it; and perhaps, discovering beyond the grave nothing but either a state of eternal destruction, or of eternal misery, fears either to be confined for ever to the sepulchre, or exposed to torments, that will make even such a condition desirable; the pious student of divine truths is not only freed from the wracking apprehensions of having his soul reduced to a state of annihilation, or cast into hell, but enjoys a comfortable expectation of finding far greater satisfaction than ever, in the study he now rejoices to have pursued; since the change, that is so justly formidable to others, will but bring him much nearer to the divine objects of his devout curiosity, and strangely elevate and enlarge his faculties to apprehend them.

Psal. ciii. 14.

1 Cor. xx. 13.

Job xviii. 14.

AND this leads me to the mention of the last advantage belonging to the study I would persuade you to; and indeed, the highest advantage, that can recommend any study, or invite men to any undertaking; for this is no less than the everlasting fruition of the divine objects of our studies hereafter, and the comfortable expectation of it here. For the employing of one's time and parts, to admire the nature and providence of God, and contemplate the divine mysteries of religion, as it is one of the chief of those homages and services, whereby we venerate and obey God; so it is one of those, to which he hath been pleased to apportion no less a recompence, than (that which can have no greater) the enjoyment of himself. The saints and angels in heaven have divers of them been employed to convey the truths of theology, and are solicitous to look into those sacred mysteries; and God hath been pleased to appoint, that those men who study the same lessons, that they do here, shall study them in their company hereafter. And doubtless, though heaven abound with unexpressible joys, yet it will be none of the least, that shall make up the happiness, even of that place, that the knowledge of divine things, that was here so zealously pursued, shall there be compleatly attained. For those things, that do here most excite our desires, and quicken the curiosity and industry of our searches, will not only there continue, but be improved to a far greater measure of attractiveness and influence. For all those interests, and passions, and lusts, that here below either hinder us from clearly discerning, or keep us from sufficiently valuing, or divert us from attentively enough considering, the beauty and harmony of divine truths, will there be either abolished, or transfigured: and as the object will be unveiled; so our eye will be enlightened, that is, as God will there disclose those worthy objects of the angels curiosity, so he will enlarge our faculties, to enable us to gaze, without being dazled, upon those sublime and radiant truths, whose harmony, as well as splendor, we shall be then qualified to discover, and consequently with transports to admire. And this enlargement and elevation of our faculties will, proportionably to its own measure, increase our satisfaction at the discoveries it will enable us to make. For theology is like a heaven, which wants not more stars than appear in it, but we want eyes, quick-sighted and piercing enough to reach them. And as the milky way, and other whiter parts of the firmament, have been full of immortal lights from the beginning, and our new telescopes have not placed, but found them, there; so, when our Saviour, after his glorious resurrection, instructed his apostles to teach the gospel, it is not said, that he altered any thing in the scriptures of *Moses* and the prophets, but only opened and enlarged their intellects, that they might understand the scriptures: and the royal prophet makes it his prayer, "That "God would be pleased to open his eyes, that "he might see wonderful things out of the "law;" being (as was above intimated) so well satisfied, that the word of God wanted not admirable things, that he is only solicitous for the improvement of his own eyes, that they might be qualified to discern them.

Dan. ix. 21, 22. Luke i. 11, 26. Acts x. 4, 5, 6. 1 Pet. i. 12.

Luke xxiv. 45. Psal. cxix. 18.

I had almost forgotten one particular about the advantages of theological studies, that is too considerable to be left unmentioned: for as great as I have represented the benefits accruing from the knowledge of divine truths; yet to endear them to us, it may be safely added, that, to procure us these benefits, the actual attainment of that knowledge is not always absolutely necessary, but a hearty endeavour after it it may suffice to entitle us to them. The patient chemist, that consumes himself and his estate in seeking after the philosopher's stone, if he miss of his idolized elixir, had as good, nay better, have never sought it, and remains as poor in effect, as he was rich in expectation. The husbandman, that employs his seed and time, to obtain from the ground a plentiful harvest, if, after all, an unkind season happen, must see his toil made fruitless;

———longique perit labor irritus anni.

Too many patients, that have punctually done and suffered for recovery, all, that physicians could prescribe, meet at last with death, instead of health. You know what entertainment has been given by skilful geometricians to the laborious endeavours, even of such famous writers as *Scaliger*, *Longomontanus*, and other Tetragonists; and that their successor Mr. *Hobbes*, after all the ways he has taken, and those he has proposed, to square the circle, and double the cube, by missing of his end, has, after his various attempts, come off, not only with disappointment, but with disgrace. And (to give an instance even in things celestial) how much pains has been taken to find out longitudes, and make astrological predictions with some certainty, which for want of coming up to what they aimed at, have been useless,
Acts xvii. if not prejudicial to the attempters?

24, 25. BUT God (to speak with St. *Paul* on another occasion) "that made the world, and all "things therein, and is lord of heaven and "earth," seeks not our services, as though he "needed any thing, seeing he giveth life, and "breath, and all things:" his self-sufficiency and bounty are such, that he seeks in our obedience the occasions of rewarding it, and prescribes us services, because the practise of them is not only suitable to our rational nature, but such as will prevail with his justice, to let his goodness make our persons happy. Agreeably
Jam. ii. to this doctrine we find in the scripture, that
21. *Abraham* is said to have been "justified by "faith, when he offered his son *Isaac* upon the "altar," (though he did not actually sacrifice him) because he endeavoured to do so; although God graciously accepting the will for the deed, accepted also of the blood of a ram instead of *Isaac*'s. And thus we know, that it was not *David*, but *Solomon*, that built the temple of *Hierusalem*, and yet God says to
2 Chron. the former of those kings (as we are told by
vi. 8, 9. the latter) "Forasmuch as it was in thine "heart to build an house for my name, thou "didst well in that it was in thine heart, not- "withstanding thou shalt not build the house, "&c." And if we look to the other circum-
2 Sam. stances of this story, as they are delivered in
vii. the second book of *Samuel*, we shall find, that upon *David*'s declaration of a design to build God an house, God himself vouchsafes to ho-
verse 5. nour him, as he once did *Moses*, with the pe-
verse 11. culiar title of his servant; and commands the prophet to say to him, "Also the Lord tells "thee, that he will make thee an house;" to which is added one of the graciousest messages, that God ever sent to any particular man. By which we may learn, that God approves and accepts even those endeavours of his servants, if they be real and sincere, that never come to be actually accomplished: good designs and endeavours are our part, but the events of those, as of all other things, are in the all-disposing hand of God, who, if we be not wanting to what lies in us, will not suffer us to be losers by the defeating dispositions of his providence; but crown our endeavours, either with success, or with some other recompence, that will keep us from being losers, by missing of that. And indeed, if we consider the great elogies, that the scripture, as well frequently as justly, gives God's goodness (which it represents as over, or as above all his works) and Hab. i. 13.
that his purer eyes punish, as well as see, the murder and adultery of the heart, when those intentional sins are hindered from advancing into actual ones; we can scarce doubt, but he, whose justice punishes sinful aims, will allow his infinite goodness to recompense pious attempts: and therefore our Saviour pronounces Matth. v.
them blessed, that hunger and thirst after righ- 6.
teousness, assuring them, that they shall be satisfied, and thereby sufficiently intimating to us, that an earnest desire, after a spiritual grace (and such is the knowledge of divine things) may intitle a man to the complete possession of it, if not in this life, yet in the next, where we shall not any more walk by faith, but by 2 Cor. v.
sight, and obtain as well a knowledge as other 7.
endowments, befitting that glorious state, wherein the purchaser of it for us, assures us, Luke xx.
that we shall be [ἰσάγγελοι] equal, or like to 36.
the angels.

THE considerations, Sir, I have hitherto laid before you, to recommend the study of divine truths, have, I hope, persuaded you, that it is on many accounts, both noble and eligible in itself; and therefore I shall here conclude the first part of this discourse. And in regard, that the under-valuation *Physiophilus* expresses for that excellent employment, seems to flow (chiefly at least) from his fondness and partiality for natural philosophy; it will next concern us to compare the study of theology with that of physicks, and shew, that the advantages, which your friend alledges in favour of the latter, are partly much lessened by disadvantageous circumstances, and partly much outweighed by the transcendent excellencies of theological contemplations: the study whereof will thereby appear to be not only eligible in itself, but preferable to its rival. And I must give you warning to expect to find the second part, which the making this comparison challenges to itself, a good deal more prolix than the first; not only because it often requires more trouble, and more words to detect and disprove an error, than to make out a truth; but also because, that divers things tending to the credit of divinity, and which consequently might have been brought into the first part of this discourse, were thought more fit to be interwoven with other things, in the answers made to the objections examined in the second.

THE

THE EXCELLENCY OF THEOLOGY:

OR,

The PRE-EMINENCE of the STUDY OF DIVINITY, above that of NATURAL PHILOSOPHY.

THE SECOND PART.

I SHALL, without preamble, begin this difcourfe, by confidering the delightfulnefs of phyficks, as the main thing, that inveigles your friend, and divers other virtuofi, from relifhing, as they ought, and otherwife would, the pleafantnefs of theological difcoveries. And to deal ingenuoufly with you, I fhall not fcruple to acknowledge, that though the addrefs I have made to nature has lafted feveral years, and has been toilfome enough, and not unexpenfive; yet I have been pleafed enough with the favours, fuch as they are, that fhe has from time to time accorded me, not to complain of having been unpleafantly employed. But though I readily allow the attainments of naturalifts to be able to give philofophical fouls fincerer pleafures, than thofe, that the more undifcerning part of mankind is fo fond of; yet I muft not therefore allow them to furpafs, or even equal the contentment, that may accrue to a foul qualified by religion, to relifh the beft things moft from fome kind of theological contemplations.

THIS, I prefume, will fufficiently appear, if I fhew you, that the ftudy of phyfiology is not unattended with confiderable inconveniencies, and that the pleafantnefs of it may be, by a perfon ftudious of divinity, enjoyed with endearing circumftances.

BUT before I name any of the particular reafons, that I am to reprefent, I fear it may be requifite to interpofe a few words, to obviate a miftake, which, if not prevented, may have an ill afpect, not only upon the firft fection, but upon a great part of the following difcourfe. For I know, that it may be faid, that whereas I alledge divers things, to leffen the lately mentioned delightfulnefs of the ftudy of phyfick, and to depreciate fome other advantages, by which the following fections would recommend it, fome of the fame things may be objected againft the delighfulnefs of the ftudy of divinity. But this objection will not, I prefume, much move you, if you confider the argument and fcope of the two parts of this letter. For in the former I have fhewn by pofitive proofs, that the ftudy of theology is attended with divers advantages, which belong to it, either only as fome of them do, or principally as others. And now in the fecond part I come to confider, whether what is alledged in behalf of the ftudy of philofophy, deferve to counter-balance thofe prerogatives or advantages; and therefore it neither need be, nor is my defign, to compare, for inftance, the delightfulnefs of the two ftudies, theology and phyficks, but by fhewing the inconveniencies, that allay the latter, to weaken the argument, that is drawn from that delightfulnefs, to conclude it preferable to the ftudy of theology. So that my work, in this and the following fections, is, not fo much to inftitute comparifons, as to obviate or anfwer allegations. For fince I have in the paft difcourfe grounded the excellency of the ftudy of divinity, chiefly upon thofe great advantages, that are peculiar to it; my reafonings would not be fruftrated, though it fhould appear, that in point of delightfulnefs, certainty, &c. that ftudy fhould, in many cafes, be liable to the fame objections with the ftudy of nature, fince it is not mainly for thefe qualities, but, as I was faying, for other and peculiar excellencies, that I recommended divinity. And therefore, fuppofing the delightfulnefs, &c. of that and of phyficks to be allayed by the fame, or equal inconveniencies or imperfections; that fuppofition would not hinder the fcales to be fwayed in favour of divinity, upon the fcore of thofe advantages, that are unqueftioned, and peculiarly belong to it. I know not, whether I need add, that notwithftanding this, you are not to expect, that I fhould give philofophy the wounds of an enemy. For my defign being not to difcourage you, nor any ingenious man, from courting her at all, nor from courting her much, but from courting her too much, and despising divinity for her, I employ against her not a sword to wound her, but a balance, to shew, that her excellencies, though solid and weighty, are less so, than the preponderating ones of theology. And this temper and purpose of mine renders my task difficult enough to have, perhaps, some right to your pardon, as well as some need of it, if I do not every way steer so exactly, as equally to avoid injuring the cause I am to plead for, and disparaging a study, which I would so little depreciate, that I allow it a great part of my inclinations, and not a little share of my time. And having said this, to keep the design of this discourse from being misunderstood, I hope we may now proceed to the particulars, whose scope we have been declaring.

Returning then to what I was about to say before this long, but needful, advertisement interrupted me, I shall resume my discourse of the delightfulness of the study of physicks, about which I was going in the first place to tell you, that I know you and your friend will freely grant me, that the knowledge of the empty and barren physiology, that is taught in the schools, as it exacts not much pains to be acquired, so it affords but little satisfaction when attained. And as I know you will give me leave to say this; so, being warranted by no slight experience of my own, I shall take leave to say also, that the study of that experimental philosophy, which is that, whereof your friend is so much enamoured, is, if it be duly prosecuted, a very troublesome and laborious employment. For, (to mention at present but this) that great variety of objects the naturalist is not only by his curiosity, but by their secret dependances upon one another, engaged to consider, and several ways to handle, will put him upon needing, and consequently upon applying himself to such a variety of mechanick people, (as distillers, druggists, smiths, turners, &c.) that a great part of his time, and perhaps all his patience, shall be spent in waiting upon tradesmen, and repairing the losses he sustains by their disappointments, which is a drudgery greater than any, who has not tried it, will imagine, and which yet being as inevitable as unwelcome, does very much counter-balance and allay the delightfulness of the study we are treating of. In which so great a part of a man's care and time must be laid out in providing the apparatus's necessary for the trying of experiments.

But this is not all. For when you have brought an experiment to an issue, though the event may often prove such as you will be pleased with, yet it will seldom prove such as you can acquiesce in. For it fares not with an inquisitive mind in studying the book of nature, as in reading of *Æsop*'s fables, or some other collection of apologues of differing sorts, and independent one upon another; where, when you have read over as many at one time as you think fit, you may leave off when you please, and go away with the pleasure of understanding those you have perused, without being solicited by any troublesome itch of curiosity to look after the rest, as those, which are needful to the better understanding of those you have already gone over, or that will be explicated by them, and scarce without them. But in the book of nature, as in a well-contrived romance, the parts have such a connection and relation to one another, and the things we would discover are so darkly or incompleatly knowable by those, that precede them, that the mind is never satisfied till it comes to the end of the book; till when all that is discovered in the progress, is unable to keep the mind from being molested with impatience, to find that yet concealed, which will not be known, till one does at least make a further progress. And yet the full discovery of nature's mysteries is so unlikely to fall to any man's share in this life, that the case of the pursuers of them is at best like theirs, that light upon some excellent romance, of which they shall never see the latter parts. For indeed (to speak now without a simile) there is such a relation betwixt natural bodies, and they may in so many ways (and divers of them unobserved) work upon, or suffer from one another, that he, who makes a new experiment, or discovers a new phænomenon, must not presently think, that he has discovered a new truth, or detected an old error. For, (at least if he be a considering man) he will oftentimes find reason to doubt, whether the experiment or observation have been so skilfully and warily made in all circumstances, as to afford him such an account of the matter of fact, as a severe naturalist would desire. And then, supposing the historical part no way defective, there are far more cases than are taken notice of, wherein so many differing agents may produce the exhibited phænomenon, or have a great influence upon the experiment or observation, that he must be less jealous than becomes a philosopher, to whom experiments do not oftentimes as well suggest new doubts, as present new phænomena.

And even those trials, that end in real discoveries, do, by reason of the connection of physical truths, and the relations that natural bodies have to one another, give such hopes and such desires of improving the acquists we have already made, to the explicating of other difficulties, or the making of further discoveries, that an inquisitive naturalist finds his work to encrease daily upon his hands, and the event of his best toils, whether it be good or bad, does but engage him into new ones, either to free himself from his scruples, or improve his successes. So that, though the pleasure of making physical discoveries is, in itself considered, very great; yet this does a little impair it, that the same attempts, which afford that delight, do so frequently beget both anxious doubts, and a disquieting curiosity. So that, if knowledge be, as some philosophers have stiled it, the aliment of the rational soul, I fear I may too truly say, that the naturalist is usually fain to live upon sallads and sauces, which, though they yield some nourishment, excite more appetite than they satisfy, and give us indeed the pleasure of eating with a good stomach, but then reduce us to an unwelcome necessity of always rising hungry from the table.

Of divers things, that lessen the delightfulness of physiological studies, I do so amply discourse in other papers, that I might well remit you thither; but indeed it is not necessary, that I should insist on this argument any further. It is true, that such a reference might be very proper, if the mysteries of theology and physick were like those of theology and necromancy, or some other part of unlawful magick, whereof the former could not be well relished without an abhorrence of the latter. But as the two great books, of nature and of scripture, have the same author; so the study of the latter does not at all hinder an inquisitive man's delight in the study of the former. The doctor I am pleading for, may as much relish a physical discovery, as *Physiophilus*; nay, by being addicted to theology and religion, he is so far from being uncapable of the contentments accruing from the study of nature, that beside those things, that recommend it to others, there are several things, that peculiarly endear it to him.

For 1. he has the contentment to look upon the wonders of nature, not only as the productions of an admirably wise author of things, but of such an one, as he intirely honours and loves, and to whom he is related. He, that reads an excellent book, or sees some rare engine, will be otherwise affected with the sight or the perusal, if he knows it to have been made by a friend, or a parent, than if he considers it but as made by a stranger, whom he has no particular reason to be concerned for. And if *Rehoboam* did not as well degenerate from the sentiments of mankind, as from his family, he could not but look upon that magnificent temple of *Solomon* with another eye, than did the throngs of strangers, that came only to gaze at it, as an admirable piece of architecture, whilst he considered, that it was his father, that built it. And if, as we see, the same heroick actions, which we read in history, of some great monarch, that strangers barely and unconcernedly admire, the natives of his country do not only venerate, but affectionately interest themselves therein, because they are his country-men, and their ancestors were his subjects: how much may we suppose the same actions would affect them, if they had the honour to be that prince's children? We may well therefore presume, that it is not without a singular satisfaction, that the contemplator, we are speaking of, does in all the wonders of nature discover, how wise, and potent, and bountiful, that author of nature is, in whom he has a great interest, and that so great an one, as both to be admitted into the number of his friends, and adopted into the number of his sons, and is thereby in some measure concerned in all the admirations and praises, that are paid, either by himself or others, to those adorable attributes, that God has displayed in that great master-piece of power and wisdom, the world. And when he makes greater discoveries in these expresses and adumbrations of the divine perfections, the delightfulness of his contemplation is proportionably increased upon such an account, as that, which endears to the passionate lover of some charming beauty an excellent, above an ordinary, picture of her; because that the same things, that make him, as it does other gazers, look upon it as a finer piece, make him look upon it as the more like his mistress, and thereby entertain him with the sublimer ideas of the beloved original; to whose transcendent excellencies he supposes, that the noblest representations must be the most resembling.

And there is a farther reason, why our contemplator should find a great deal of contentment in these discoveries. For we have in our nature so much of imperfection, and with all so much of inclination to self-love, that we do too confidently proportion our ideas of what God can do for us, to what we have already the knowledge, or the possession of. And though, when we make it our business, we are able with much ado somewhat to enlarge our apprehensions, and raise our expectations beyond their wonted pitch; yet still they will be but scantly promoted and heightened, if those things themselves be but mean and ordinary, which we think we have done enough, if we make them surpass. A country villager, born and bred in a homely cottage, cannot have any suitable apprehensions of the pleasures and magnificence of a great monarch's court. And if he should be bid to scrue up his imagination to frame ideas of them, they would be borrowed from the best tiled house he had seen in the market-towns, where he had sold his turnips or corn, and the wedding-feast of some neighbouring farmer's daughter. And though a child in the mother's womb had the perfect use of reason, yet could it not in that dark cell have any ideas of the sun or moon, or beauties or banquets, or algebra, or chemistry, and many other things, which his elder brothers, that breathe fresh air, and freely behold the light, and are in a more mature estate, are capable of knowing and enjoying. Now among thinking men, whose thoughts run much upon that future state, which they must shortly enter into, but shall never pass out of; there will frequently and naturally arise a distrust, which, though seldom owned, proves oftentimes disquieting enough. For such men are apt to question, how the future condition, which the gospel promises, can afford them so much happiness as it pretends to; since they shall in heaven but contemplate the works of God, and praise him, and converse with him; all which they think may, though not immediately, be done by men here below, without being happy. But he, that by telescopes and microscopes, dexterous dissections, and well employed furnaces, &c. discovers the wondrous power and skill of him, that contrived so vast and immense a mass of matter into so curious a piece of workmanship as this world, will pleasingly be convinced of the boundless power and goodness of the great architect. And when he sees, how admirably every animal is furnished with parts requisite to his respective nature; and that there is particular care taken, that the same animal, as for example, man, should have differing provisions made for him, according to his differing states within the womb, and out of it, (a human egg, and an embrio, being being much otherwise nourished and fitted for action, than is a (compleat) man;) he, I say, who considers this, and observes the stupendous providence, and excellent contrivances, that the curious priers into nature (and none but they) can discover, will be as well enabled as invited to reason thus within himself; that sure God, who has with such admirable artifice framed silk-worms, butterflies, and other meaner insects, and with such wonderful providence taken care, that the nobler animals should as little want any of all the things requisite to the compleating of their respective natures; and who, when he pleases, can furnish some things with qualifications quite differing from those, which the knowledge of his other works could have made us imagine, (as is evident in the load-stone, and in quick-silver among minerals, and the sensitive plant among vegetables, the camelion among animals, &c.) this God, I say, must needs be fully able to furnish those he delights to honour, with objects suitable to their improved faculties, and with all, that is requisite to the happiness he intends them in their glorified state; and is able to bring this to pass by such amazing contrivances, as perhaps will be quite differing from any, that the things we have yet seen suggest to us any ideas of. And sure he, that has in so immense, so curious, and so magnificent a fabrick, made such provision for men, who are either desperately wicked, or but very imperfectly good, and in a state, where they are not to enjoy happiness, but by obedience and sufferings to fit themselves for it, may safely be trusted with finding them in heaven employments and delights becoming the felicity he designs them there; as we see, that here below he provides as well for the soaring eagle, as for the creeping caterpillar, (and is able to keep the ocean as fully supplied with rivers, as lakes or ponds are with springs and brooks.) And as a state of celestial happiness is so great a blessing, that those things, that afford us either greater assurances, or greater foretastes of it, are of the number of the greatest contentments and advantages, that short of it we can enjoy; so it is hard for any divine to receive so much of this kind of satisfaction, as he, who by skilfully looking into the wonders of nature, has his apprehensions of God's power and manifold wisdom (as an apostle calls it) elevated and enlarged. As when the queen of *Sheba* had particularly surveyed the astonishing prudence, that *Solomon* displayed in the ordering of his magnificent court, she transportedly concluded those servants of his to be happy enough to deserve a monarch's envy, that were allowed the honour and priviledge of a constant and immediate attendance on him.

Ephes. iii. 10.

SECTION II.

I Doubt not but you have too good an opinion of your friend, not to think, that you may alledge in his favour, that the chief thing, which makes him prefer physiology to all other kind of knowledge, is, that it enables those, who are proficients in, it to do a great deal of good, both by improving of trades, and by promoting of physick it self. And I am too mindful of what I writ to *Pyrophilus*, to deny, either that it can assist a man to advance physick and trades, or that, by so doing, he may highly advantage mankind. And this I (who would not lessen your friend's esteem for physicks, but only his partiality) willingly acknowledge to be so allowable an endearment of experimental philosophy, that I do not know any thing, that to men of a humane, as well as ingenious disposition, ought more to recommend the study of nature, except the opportunity it affords men to be just and grateful to the author both of nature and of man. I do not then deny, that the true naturalist may very much benefit mankind; but I affirm, that, if men be not wanting to themselves, the divine may benefit them much more. It were not perchance either unseasonable, or impertinent, to tell you on this occasion, that he, who effectually teaches men to subdue their lusts and passions, does as much as the physician contribute to the preservation of their bodies, by exempting them from those vices, whose no less usual than destructive effects are wars, and duels, and rapines, and desolations, and the pox, and surfeits, and all the train of other diseases, that attend gluttony and drunkenness, idleness, and lust; which are not enemies to man's life and health barely upon a physical account, but upon a moral one, as they provoke God to punish them with temporal as well as spiritual judgments; such as plagues, wars, famines, and other publick calamities, that sweep away a great part of mankind; besides those personal afflictions of bodily sickness, and disquiets of conscience, that do both shorten men's lives, and imbitter them. Whereas piety having (as the scripture assures us) promises both of this life, and of that, which is to come, those teachers, that make men virtuous and religious, by making them temperate, and chaste, and inoffensive, and calm, and contented, do not only procure them great and excellent dispositions to those blessings, both of the right hand and of the left, which God's goodness makes him forward to bestow on those, who by grace and virtue are made fit to receive them; but do help them to those qualifications, that by preserving the mind in a calm and cheerful temper, as well as by affording the body all, that temperance can confer, do both lengthen their lives, and sweeten them. These things, I say, it were not impertinent to insist on; but I will rather chuse to represent to you, that the benefits, which men may receive from the didivine, surpass those, which they receive from the naturalist, both in the nobleness of the advantages, and in the duration of them.

Be it granted then, that the naturalist may much improve both physick and trades; yet since these themselves were devised for the service of the body, (the one to preserve or restore his health, and the other to furnish it with

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accommodations or delights;) the boasted use of natural philosophy, by its advancing trades and physick, will still be to serve the body, which is but the lodging and instrument of the soul, and which, I presume, your friend, and which I am sure your self will be far from thinking the noblest part of man. I know it may be said, nor do I deny it, that divers mechanical arts are highly beneficial, not only to the inventors, but to those places, and perhaps those states, where such improvements are found out and cherished. But though I most willingly grant, that this consideration ought to recommend experimental philosophy, as well to states as to private persons; yet many of these improvements do rather transfer than encrease mankind's goods, and prejudice one sort of men as much as they advantage another, (as in the case of the eastern spices, of whose trade the Portugals and Dutch by their later navigations, did, by appropriating it to themselves, deprive the Venetians) or else do but increase that, which, though very beneficial to the producers, is not really so to mankind in general: of which we have an example in the invention of extracting gold and silver out of the oar, with mercury. For though it have vastly enriched the Spaniards in the *West Indies*, yet it is not of any solid advantage to the world; no more than the discovery of the Peruvian and other American mines; by which, (especially reckoning the multitudes of unhappy men, that are made miserable, and destroyed in working them,) mankind is not put into a better condition than it was before. And if the philosopher's stone it self, (supposing there be such a thing) were not an incomparable medicine, but were only capable of transmuting other metals into gold, I should perhaps doubt, whether the discoverer of it would much advantage mankind; there being already gold and silver enough to maintain trade and commerce among men; and for all other purposes, I know not, why a plenty of iron, and brass, and quicksilver, which are far more useful metals, should not be more desirable. But not to urge this; we may consider, that these advancements of enriching trades do still bring advantages but to the outward man, and those many arts and inventions, that aim at the heightening the pleasures of the senses*, belong but to the body; and even in point of gratifying that, are not so requisite and important, as many suppose; education, custom, &c. having a greater interest, than most imagine, in the relish men have even of sensitive pleasures. And as for physick, not to mind you, that it has been loudly, (how justly, I here examine not,) complained of, that the new philosophy has made it far greater promises than have yet been performed; I shall only take notice, that since all, that physick is wont to pretend to, is, to preserve health, or restore it; there are multitudes in the world, that have no need of the assistance the naturalist would give the physician; and a healthy man, as such, is already in a better condition, than the philosopher can hope to place him in, and is no more advantaged by the naturalist's contribution to physick, than a sound man, that sleeps in a whole skin, is by all the fine tools of a surgeon's case of instruments, and the various compositions of his chest.

And as the benefits, that may be derived from theology, much surpass those, that acrue from physicks, in the nobleness of the subject they relate to; so have they a great advantage in point of duration. For all the services, that medicines, and engines, and improvements can do a man, as they relate but to this life, so they determine with it. Physick indeed, and chymistry do, the one more faintly, and the other more boldly, pretend sometimes, not only to the cure of diseases, but the prolongation of life; but since none will suspect, but that the masters of those parts of knowledge would employ their utmost skill to protract their own lives, those, that remember, that *Solomon* and *Helmont* lived no longer, than millions, that were strangers to philosophy; and that even *Paracelsus* himself, for all his boasted *Arcana*, is by *Helmont* and other chemists confessed to have died some years short of fifty; we may very justly fear, that nature will not be so kind to her greatest votaries, as to give them much more time than other men, for the payment of the last debt all men owe her. And if a few years respite could by a scrupulous and troublesome use of diet and remedies be obtained; yet that, in comparison of the eternity, that is to follow, is not at all considerable. But whereas within no great number of years, (a little sooner, or a little later) all the remedies, and reliefs, and pleasures, and accommodations, that philosophical improvements can afford a man, will not keep him from the grave, (which within very few days will make the body of the greatest virtuoso as hideous and as loathsome a carcase, as that of any ordinary man;) the benefits, that may accrue to us by divinity, as they relate chiefly, though not only, to the other world; so they will follow us out of this, and prove then incomparably greater than ever, when they alone shall be capable of being enjoyed. So that philosophy, in the capacity we here consider it, does but as it were provide us some little conveniences for our passage, like some accommodations for a cabbin, which outlasts not the voyage; but religion provides us a vast and durable estate, or, as the scripture stiles it, an unshaken kingdom, when we are arrived at our journey's end. And therefore the benefits accruing from religion, may well be concluded preferable to their competitors, since they not only reach to the mind of man, but reach beyond the end of time itself; whereas all the variety of inventions, that philosophy so much boasts of, as whilst they were in season they were devised for the service of the body, so they make us busy, and pride ourselves about things, that within a short time will not (so much as upon its score) at all concern us.

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* See examples of this, in my notes about Sensation and Sensible Qualities.

SECTION III.

I EXPECT you ſhould here urge on your friend's behalf, that the ſtudy of phyſicks has one prerogative, (above that of divinity,) which, as it is otherwiſe a great excellency, ſo does much add to the delightfulneſs of it. I mean, the certainty, and clearneſs, and thence reſulting ſatisfactorineſs of our knowledge of phyſical, in compariſon of any we can have of theological matters, whoſe being dark and uncertain, the nature of the things themſelves, and the numerous controverſies of differing ſects about them, ſufficiently manifeſt.

BUT upon this ſubject, divers things are to be conſidered.

FOR firſt, as to the fundamental and neceſſary articles of religion, I do not admit the allegation, but take thoſe articles to be both evident, and capable of a moral demoſtration. And if there be any articles of religion, for which a rational and cogent proof cannot be brought, I ſhall for that very reaſon conclude, that ſuch articles are not abſolutely neceſſary to be believed; ſince it ſeems no way reaſonable to imagine, that God having been pleaſed to ſend not only his prophets and his apoſtles, but his only ſon into the world, to promulgate to mankind the Chriſtian religion, and both to cauſe it to be conſigned to writing, that it may be known, and to alter the courſe of nature by numerous miracles, that it might be believed; it ſeems not reaſonable, I ſay, to imagine, that he ſhould not propoſe thoſe truths, which he in ſo wonderful and ſo ſolemn a manner recommended, with at leaſt ſo much clearneſs, as that ſtudious and well-diſpoſed readers may certainly underſtand ſuch, as are neceſſary for them believe.

2. THOUGH I will not here engage myſelf in a diſquiſition of the ſeveral kinds, or, if you pleaſe, degrees, of demonſtration (which yet is a ſubject, that I judge far more conſiderable than cultivated,) yet I muſt tell you, that as a moral certainty (ſuch as we may attain about the fundamentals of religion) is enough in many caſes for a wiſe man, and even a philoſopher to acquieſce in; ſo that phyſical certainty, which is pretended for the truths demonſtrated by naturaliſts, is, even where it is rightfully claimed, but an inferior kind or degree of certainty, as moral certainty alſo is. For even phyſical demonſtrations can beget but a phyſical certainty, (that is, a certainty upon ſuppoſition, that the principles of phyſick be true,) not a metaphyſical certainty, (wherein it is abſolutely impoſſible, that the thing believed ſhould be other than true.) For inſtance, all the phyſical demonſtrations of the antients about the cauſes of particular phænomena of bodies ſuppoſe, that *ex nihilo nihil fit*; and this may readily be admitted in a phyſical ſenſe, becauſe, according to the courſe of nature, no body can be produced out of nothing, but ſpeaking univerſally it may be falſe, as Chriſtians generally, and even the Carteſian naturaliſts, aſſerting the creation of the world, muſt believe, that, *de facto*, it is. And ſo whereas *Epicurus* does, I remember, prove, that a body once dead cannot be made alive again, by reaſon of the diſſipation and diſperſion of the atoms, it was, when alive, compoſed of; though all men will allow this aſſertion to be phyſically demonſtrable, yet the contrary may be true, if God's omnipotence intervenes, as all the philoſophers, that acknowledge the authority of the New Teſtament, where *Lazarus* and others are recorded to have been raiſed from the dead, muſt believe, that it actually did appear, and even all unprejudiced reaſoners muſt allow it to be poſſible, there being no contradiction implied in the nature of the thing. But now to affirm, that ſuch things, as are indeed contradictories, cannot be both true, or that *factum infectum reddi non poteſt*, are metaphyſical truths, which cannot poſſibly be other than true, and conſequently beget a metaphyſical and abſolute certainty. And your maſter *Carteſius* was ſo ſenſible of a dependance of phyſical demonſtrations upon metaphyſical truths, that he would not allow any certainty not only to them, but even to geometrical demonſtrations, until he had evinced, that there is a God, and that he cannot deceive men, that make uſe of their faculties aright.

To which I may add, that even in many things, that are looked upon as phyſical demonſtrations, there is really but a moral certainty. For when, for inſtance, *Des Cartes* and other modern philoſophers, take upon them to demonſtrate, that there are divers comets, that are not meteors, becauſe they have a parallax leſſer than that of the moon, and are of ſuch a bigneſs, and ſome of them move in ſuch a line, &c. it is plain, that divers of theſe learned men had never the opportunity to obſerve a comet in their lives, but take theſe circumſtances upon the credit of thoſe aſtronomers, that had ſuch opportunities. And though the inferences, as ſuch, may have a demonſtrable certainty; yet the premiſſes they are drawn from having but an hiſtorical one, the preſumed phyſico-mathematical demonſtration can produce in a wary mind but a moral certainty, and not the greateſt neither of that kind, that is poſſible to be attained; as he will not ſcruple to acknowledge, that knows by experience, how much more difficult it is, than moſt men imagine, to make obſervations about ſuch nice ſubjects, with the exactneſs, that is requiſite for the building of an undoubted theory upon them. And there are I know not how many things in phyſicks, that men preſume they believe upon phyſical and cogent arguments, wherein they really have but a moral aſſurance; which is a truth heeded by ſo few, that I have been invited to take the more particular notice of them in other papers, written purpoſely to ſhow the doubtfulneſs and incompleatneſs of natural philoſophy; of which diſcourſe, ſince you may command a ſight, I ſhall not ſcruple to refer you thither for the reaſons of my affirming here, that the moſt even of the modern virtuoſi are wont to fancy more of clearneſs and certainty in their phyſical theories, than a critical

tical examiner will find. Only, that you may not look upon this as a put off, rather than a reference, I will here touch upon a couple of ſubjects, which men are wont to believe to be, and which indeed ought to be, the moſt thoroughly underſtood; I mean the nature of body in general, and the nature of ſenſation.

And for the firſt of theſe, ſince we can turn our ſelves no way, but we are every where environed, and inceſſantly touched by corporeal ſubſtances, one would think, that ſo familiar an object, that does ſo aſſiduouſly, and ſo many ways affect our ſenſes, and for the knowledge of which we need not enquire into the diſtinct nature of particular bodies, nor the properties of any one of them, ſhould be very perfectly known unto us. And yet the notion of body in general, or what it is, that makes a thing to be a corporeal ſubſtance, and diſcriminates it from all other things, has been very hotly diſputed of, even among the modern philoſophers, *& adhuc ſub judice lis eſt*. And though your favourite *Des-Cartes*, in making the nature of a body to conſiſt in extenſion every way, has a notion of it, which it is more eaſy to find fault with, than to ſubſtitute a better; yet I fear, it will appear to be attended, not only with this inconvenience, that God cannot, within the compaſs of this world, wherein if any body vaniſh into nothing, the place or ſpace left behind it, muſt have the three dimenſions, and ſo be a true body, annihilate the leaſt particle of matter, at leaſt without, at the ſame inſtant and place, creating as much (which agrees very ill with that neceſſary and continual dependance, which he aſſerts matter itſelf to have on God for its very being;) but with ſuch other inconveniences, that ſome friends of yours, otherwiſe very inclinable to the Carteſian philoſophy, know not how to acquieſce in it: and yet I need not tell you, how fundamental a notion the deviſer of it aſſerts it to be. Neither do I ſee, how this notion of a corporeal ſubſtance will any more than any of the formerly received definitions of it, extricate us out of the difficulties of that no leſs perplexed, than famous controverſy, *de compoſitione continui*. And though ſome ingenious men, who perhaps perceive better than others, how intricate it is, have of late endeavoured to ſhew, that men need not be ſolicitous to determine this controverſy, it not being rightly propoſed by the ſchool-men, that have ſtarted it; and though I perhaps think, that natural philoſophy may be daily advanced without the deciſion of it, becauſe there is a multitude of conſiderable things to be diſcovered and performed in nature, without ſo much as dreaming of this controverſy; yet ſtill, as I would propoſe the queſtion, the difficulties, till removed, will ſpread a thick night over the notion of body in general. For either a corporeal and extended ſubſtance is (either really or mentally) diviſible into parts endowed with extenſion, and each of theſe parts is diviſible alſo into other corporeal parts, leſſer and leſſer, *in infinitum*; or elſe this ſubdiviſion muſt ſtop ſomewhere, (for there is no mean between the two members of the diſtinction;) and in either caſe the opinion pitched upon will be liable to those inconveniences, not to ſay abſurdities, that are rationally urged againſt it by the maintainers of the oppoſite; the objections on both ſides being ſo ſtrong, that ſome of the more candid, even of the modern metaphyſicians, after having tired themſelves and their readers with arguing *Pro* and *Con*, have confeſſed the objections on both ſides to be inſoluble.

But though we do not clearly underſtand the nature of body in general; yet ſure we cannot but be perfectly acquainted with what paſſes within ourſelves in reference to the particular bodies we daily ſee, and hear, and ſmell, and taſte, and touch. But alas, though we know but little, ſave by the informations of our ſenſes; yet we know very little of the manner, by which our ſenſes informs us. And to avoid prolixity, I will at preſent ſuppoſe with you, that the ingenious *Des-Cartes* and his followers have given the faireſt account of ſenſation, that is yet extant. Now, according to him, a man's body being but a well organized ſtatue, that, which is truly called ſenſation, is not performed by the organ, but by the mind, which perceives the motion produced in the organ; (for which reaſon he will not allow brutes to have ſenſe properly ſo called;) ſo that if you aſk a Carteſian, how it comes to paſs, that the ſoul of man, which he juſtly aſſerts to be an immaterial ſubſtance, comes to be wrought upon, and that in ſuch various manners, by thoſe external bodies, that are the objects of our ſenſes, he will tell you, that by their impreſſions on the ſenſories, they variouſly move the fibres or threads of the nerves, wherewith thoſe parts are endowed, and by which the motion is propagated to that little kernel in the brain, called by many writers the Conarion, where theſe differing motions being perceived by the there reſiding ſoul, become ſenſations, becauſe of the intimate union, and, as it were, permiſtion (as *Carteſius* himſelf expreſſes it) of the ſoul with the body.

But now, Sir, give me leave to take notice, that this union of an incorporeal, with a corporeal ſubſtance, (and that without a medium) is a thing ſo unexampled in nature, and ſo difficult to comprehend, that I ſomewhat queſtion, whether the profound ſecrets of theology, not to ſay the adorable myſtery itſelf of the incarnation, be more abſtruſe than this. For how can I conceive, that a ſubſtance purely immaterial, ſhould be united without a phyſical medium, (for in this caſe there can be none,) with the body, which cannot poſſibly lay hold on it, and which it can pervade, and fly away from at pleaſure, as *Des Cartes* muſt confeſs the ſoul actually does in death. And it is almoſt as difficult to conceive, how any part of the body, without excepting the animal ſpirits, or the Conarion, (for theſe are as truly corporeal, as other parts of the human ſtatue,) can make impreſſions upon a ſubſtance perfectly incorporeal, and which is not immediately affected by the motions of any other parts, beſides the *Genus Nervoſum*. Nor is it a ſmall difficulty to a mere naturaliſt (who, as ſuch, does not in phyſical matters take notice of revelations about angels,) to conceive, how a finite ſpirit can either move, or, which is is much the ſame thing, regulate and determine the motion of a body. But that, which I would on this occaſion invite you to conſider, is, that ſuppoſing the ſoul does in the brain perceive the differing motions communicated to the outward ſenſes; yet this, however it may give ſome account of ſenſation in general, will not at all ſhew us a ſatisfactory reaſon of particular and diſtinct ſenſations. For if I demand, why, for inſtance, when I look upon a bell, that is ringing, ſuch a motion or impreſſion in the Conarion produces in the mind that peculiar ſort of perception, ſeeing, and not hearing; and another motion, though coming from the ſame bell, at the ſame time, produces that quite differing ſort of perception, that we call ſound, but not viſion; what can be anſwered, but that it was the good pleaſure of the author of human nature to have it ſo? And if the queſtion be aſked about the differing objects of any one particular ſenſe; as, why the great plenty of unperturbed light, that is reflected from ſnow, milk, &c. does produce a ſenſation of whiteneſs, rather than redneſs or yellowneſs? or why the ſmell of caſtor, or aſſa fœtida, produces in moſt perſons that, which they call a ſtink, rather than a perfume? (eſpecially ſince we know ſome hyſterical women, that think it not only a wholſome, but a pleaſing ſmell.) And if alſo you further aſk, why melody and ſweet things do generally delight us? and diſcords and bitter things do generally diſpleaſe us? Nay, why a little more than enough of ſome objects, that produce pleaſure, will produce pain? (as may be exemplified in a cold hand, as it happens to be held out at a juſt, or at too near a diſtance from the fire:) if, I ſay, theſe, and a thouſand other queſtions of the like kind, be aſked, the anſwer will be but the general one, that is already given, that ſuch is the nature of man. For to ſay, that moderate motions are agreeable to the nature of the ſenſory they are excited in, but violent and diſorderly ones (as jarring ſounds, and ſcorching heat) do put it into too violent a motion for its texture; will by no means ſatisfy. For beſides that this anſwer give no account of the variety of ſenſations of the ſame kind, as of differing colours, taſtes, &c. but reaches only to pleaſure and pain; even as to theſe, it will reach but a very little way; unleſs the givers of it can ſhow, how an immaterial ſubſtance ſhould be more harmed by the briſker motion of a body, than by the more languid.

And as you and your friend think, you may juſtly ſmile at the Ariſtotelians, for imagining, that they have given a tolerable account of the qualities of bodies, when they have told us, that they ſpring from certain ſubſtantial forms, though when they are aſked particular queſtions about theſe incomprehenſible forms, they do in effect but tell us in general, that they have ſuch and ſuch faculties, or effects, becauſe nature, or the author of nature, endowed them therewith; ſo, I hope, you will give me leave to think, that it may keep us from boaſting of the clearneſs and certainty of our knowledge, about the operations of ſenſible objects, whilſt, as the Ariſtotelians cannot particularly ſhow, how their qualities are produced, ſo we cannot particularly explicate, how they are perceived; the principal thing, that we can ſay, being in ſubſtance this, that our ſenſations depend upon ſuch an union or permiſtion of the ſoul and body, as we can give no example of in all nature, nor no more diſtinct account of, than that it pleaſed God ſo to couple them together. But I beg your pardon for having detained you ſo long upon one ſubject, though perhaps it will not prove time miſ-ſpent, if it have made you take notice, that in ſpight of the clearneſs and certainty, for which your friend ſo much prefers phyſicks before theology, we are yet to ſeek, (I ſay yet, becauſe I know not what time may hereafter diſcover) both for the definition of a corporeal ſubſtance, and a ſatisfactory account of the manner of ſenſation: though without the true notion of a body we cannot underſtand, that object of phyſicks in general, and without knowing the nature of ſenſation, we cannot know that, from whence we derive almoſt all that we know of any body in particular.

If after all this your friend ſhall ſay, that *Des-Cartes's* account of body, and other things in phyſicks, being the beſt, that men can give, if they be not ſatisfactory, it muſt be imputed to human nature not to the Carteſian doctrine, I ſhall not ſtay to diſpute, how far the allegation is true; eſpecially ſince, though it be admitted, it will not prejudice my diſcourſe. For, whatſoever the cauſe of the imperfection of our knowledge about phyſical matters be, that there is an imperfection in that knowledge is manifeſt; and that ought to be enough to keep us from being puffed up by ſuch an imperfect knowledge, and from undervaluing upon its account the ſtudy of thoſe myſteries of divinity, which, by reaſon of the nobleneſs and remoteneſs of the objects, may much better than the nature of corporeal things, (which we ſee, and feel, and continually converſe with,) have their obſcurity attributed to the weakneſs of our human underſtandings. And if it be a neceſſary imperfection of human nature, that, whilſt we remain in this mortal condition, the ſoul being confined to the dark priſon of the body, is capable (as even *Ariſtotle* ſomewhere confeſſes) but of a dim knowledge; ſo much the greater value we ought to have for Chriſtian religion, ſince, by its means (and by no other without it) we may attain a condition, wherein, as our nature will otherwiſe be highly bleſſed and advanced; ſo our faculties will be elevated and enlarged, and probably made thereby capable of attaining degrees and kinds of knowledge, to which we are here but ſtrangers. In favour of which I will not urge the received opinion of divines, that before the fall (which yet is a leſs noble condition than is reſerved for us in heaven,) *Adam's* knowledge was ſuch, that he was able at firſt ſight of them, to give each of the beaſts a name expreſſive of its nature; becauſe that, in ſpight of ſome ſkill (which my curioſity for divinity, not philoſophy, gave me)

in the holy tongue, I could never find, that the Hebrew names of animals, mentioned in the beginning of *Geneſis*, argued a (much) clearer inſight into their natures, than did the names of the ſame or ſome other animals in Greek, or other languages: wherefore, as I ſaid, I will not urge *Adam*'s knowledge in paradiſe for that of the ſaints in heaven, though the notice he took of *Eve* at his firſt ſeeing of her, (if it were not conveyed to him by ſecret revelation) may be far more probably urged, than his naming of the beaſts: but I will rather mind you, that the proto-martyr's ſight
Acts vii. 56. was ſtrengthened ſo, as to "ſee the heavens "opened, and Jeſus ſtanding at the right hand "of God;" and when the prophet had pray-
2 Kings vi. 17. ed, that his ſervant's eyes might be opened, he immediately ſaw the mountain, where they were, all covered with chariots and horſemen, which, though mentioned to be of fire, were altogether inviſible to him before. To which, as a higher argument, I ſhall only add a couple of paſſages of ſcripture, which ſeem to allow us even vaſt expectations as to the knowledge our glorified nature may be advanced to. The one is that, which St. *Paul* ſays to the *Co-*
1 Cor. xiii. 12. *rinthians*, "for now we ſee through a darkly, "but then face to face: now I know in part, "but then ſhall I know even as alſo I am "known." And the other, where Chriſt's fa-
1 Joh. iii. 2. vourite-diſciple tells believers, "Beloved, now "we are the ſons of God, and it doth not yet "appear what we ſhall be; but we know, "that when he ſhall appear, we ſhall be like "him for we ſhall ſee him as he is."

WHAT has hitherto been diſcourſed, contains the firſt conſideration, that I told you might be propoſed about the certainty aſcribed to the knowledge we are ſaid to have of natural things; but this is not all I have to repreſent to you on this ſubject. For I conſider further, that it is not only by the certainty we have of them, that the knowledge of things is endeared to us, but alſo by the worthineſs of the object, the number of thoſe, that are unacquainted with it, the remoteneſs of it from common apprehenſions, the difficulty of acquiring it without peculiar advantages, the uſefulneſs of it when attained, and other particulars, which it is not here neceſſary to enumerate. I preſume, you doubt not but your friend does very much prefer the knowledge he has of the myſteries of nature (at many of which we have as yet but ingenious conjectures) to the knowledge of one, that underſtands the elements of arithmetick, though he be demonſtratively ſure of the truth of moſt of his rules and operations. And queſtionleſs *Copernicus* received a much higher ſatisfaction in his notion about the ſtability of the ſun, and the motion of the earth, though it were not ſo clear, but that *Tycho*, *Ricciolus*, and other eminent aſtromomers have rejected it, than in the knowledge of divers of the theorems about the ſphere, that have been demonſtrated by *Euclid*, *Theodoſius*, and other geometricans. Our diſcovering, that ſome comets are not, as the ſchools would have them, ſublunary meteors, but celeſtial bodies, and the conjectural theory, which is all, that hitherto we have been able to attain of them, do much better pleaſe both your friend, and you, and me, than the more certain knowledge we have of the time of the riſing and ſetting of the fixed ſtars. And the eſtimates we can make, by the help of parallaxes, of the heights of thoſe comets, and of ſome of the planets, though they are uncertain enough, (as may appear by the vaſtly different diſtances, that are aſſigned to thoſe bodies by eminent aſtronomers;) yet theſe uncertain meaſures of ſuch elevated and celeſtial lights do far more pleaſe us, than that we can by the help of a geometrical quadrant, or ſome ſuch inſtrument, take with far greater certainty the height of a tower or a ſteeple. And ſo a mathematician, when he probably conjectures at the compaſs of the terreſtrial globe, and divides, though but unaccurately, its ſurface, firſt, into proportions of ſea and land, and then into regions of ſuch extents and bounds, and, in a word, ſkilfully plays the coſmographer; thinks himſelf much more nobly and pleaſantly employed, than when, being reduced to play the ſurveyor, he does, with far more certainty, meaſure how many acres a field contains, and ſet out, with what hedges and ditches it is bounded. Now, that the knowledge of God, and of thoſe myſteries of theology, that are ignored by far the greateſt part of mankind, has more ſublime and excellent objects, and is unattained to by much the greateſt part even of learned men, and neverthele ſs is of unvaluable importance, and of no leſs advantage towards the purifying and improving of us here, and the making us perfect and happy hereafter, the paſt diſcourſe has very much miſcarried, if it have not evinced. Wherefore, as to be admitted into the privy-council of ſome great monarch, and thereby be enabled to give a probable gueſs at thoſe thoughts and deſigns of his, that govern kingdoms, and make the fates of nations, is judged preferable to that clearer knowledge, that a notary can have of the dying thoughts and intentions of an ordinary perſon, whoſe will he makes: and as the knowledge of a ſkilful phyſician, whoſe art is yet conjectural, is preferable to that of a cutler, that makes his diſſecting knives, though this man can more certainly perform what he deſigns in his own profeſſion, than the phyſician can in his: and (in fine) as the ſkill of a jeweller, that is converſant about diamonds, rubies, ſapphires, and ſome other ſorts of ſmall ſtones, which being, for the moſt part, brought us out of the *Indies*, we muſt take many things about them upon report, is, becauſe of the nobleneſs of the object, preferred to that of a maſon, that deals in whole quarries of common ſtones, and may be ſure upon his own experience of divers things concerning them, which as to jewels we are allowed to know but upon tradition: ſo a more dim and imperfect knowledge of God, and the myſteries of religion, may be more deſireable, and upon that account more delightful, than a clearer knowledge of thoſe inferior truths, that phyſicks are wont to teach.

I must now mention one particular more, which may well be added to those, that peculiarly endear physicks to the divine, that is studious of them. For as he contemplates the works of nature not barely for themselves, but to be the better qualified and excited to admire and praise the author of nature; so his contemplations are delightful to him, not barely as they afford a pleasing exercise to his reason, but as they procure him a more welcome approbation from his conscience, these distinct satisfactions being not at all inconsistent. And questionless, though *Esau* did at length miss of his aim, yet, while he was hunting venison for the good old patriarch, that desired it of him, besides the pleasure he was used to take in pursuing the dear he chased, he took a great one,
Gen. xxxvii. in considering, that now he hunted to please his father, and in order to obtain of him an inestimable blessing. So, when *David* employed his skilful hand and voice, in praising god with vocal and instrumental musick, he received in one act a double satisfaction, by exercising his skill and his devotion; and was no less pleased with those melodious sounds, as they were hymns, than as they were songs. And this example prompts me to add, that as the devout student of nature we were speaking of, does intentionally refer the knowledge he seeks of the creatures to the glory of the creator; so in his discoveries, that, which most contents him, is, that the wonders he observes in nature, heighten that admiration he would fain raise to a less disproportion to the wisdom of God; and furnish him with a nobler holocaust for those sacrifices of praise, he is justly ambitious to offer up to the deity. And as there is no doubt to be made, but that, when
Amos vi. 5. *David* invented (as the scripture intimates, that he did) new instruments of musick, there was nothing in that invention, that pleased him so much, as that they could assist him to praise God the more melodiously; so the pious student of nature finds nothing more welcome in the discoveries he makes of her wonders, than the rises and helps they may afford him, the more worthily to celebrate and glorify the divine attributes adumbrated in the creatures. And as a huntsman, or a fowler, if he meets with some strange bird or beast, or other natural rarity, thinks himself much the more fortunate, if it happen to be near the court, where he may have the king to present it to, than if he were to keep it but for himself, or some of his companions; so our devout naturalist has his discoveries of nature's wonders endeared to him, by having the deity to present them to, in the veneration they excite in the finder, and which they enable him to engage others to join in.

SECTION IV.

BUT I confess, Sir, I much fear, that that, which makes your friend have such detracting thoughts of theology, is a certain secret pride, grounded upon a conceit, that the attainments of natural philosophers are of so noble a kind, and argue so transcendent an excellency of parts in the attainer, that he may justly undervalue all other learning, without excepting theology itself.

YOU will not, I suppose, expect, that a person, who has written so much in the praise of physicks, and laboured so much for a little skill in it, should now here endeavour to depreciate that so useful part of philosophy. But I do not conceive, that it will be at all injurious to it, to prefer the knowledge of supernatural to that of mere natural things, and to think, that the truths, which God indiscriminately exposes to the whole race of mankind, and to the bad, as well as to the good, are inferior to those mysterious ones, whose disclosure he reckons among his peculiar favours, and whose contemplation employs the curiosity, and, in some points, exacts the wonder of the very angels. That I may therefore repress a little the overweaning opinion your friend has of his physical attainments, give me leave to represent a few particulars conducive to that purpose.

AND first, as for the nobleness of the truths taught by theology and physicks, those of the former sort have manifestly the advantage, being not only conversant about far nobler objects, but discovering things, that human reason of itself can by no means reach unto; as has been sufficiently declared in the foregoing part of this letter.

NEXT, we may consider, that, whatever may be said to excuse pride (if there were any) in *Moschus* the Phœnician, who is affirmed to have first invented the atomical hypothesis, and in *Democritus* and *Leucippus*, (for *Epicurus* scarce deserves to be named with them) that highly advanced that philosophy; and in Monsieur *Des Cartes*, who either improved, or at least much innovated the corpuscularian hypothesis: whatever, I say, may be alledged on the behalf of these men's pride; I see no great reason, why it should be allowed in such as your friend; who, though ingenious men, are neither inventors, nor eminent promoters of the philosophy they would be admired for, but content themselves to learn what others have taught, or, at least, to make some little further application of the principles, that others have established, and the discoveries they have made. And whereas your friend is not a little proud of being able to confute several errors of *Aristotle*, and the ancients, it were not amiss if he considered, that many of the chief truths, that overthrow those errors, were the productions of time and chance, and not of his daring ratiocinations: for there needs no great wit to disprove those, that maintain the uninhabitableness of the torrid zone, or deny the antipodes, since navigators have found many parts of the former well peopled, and sailing round the earth, have found men living in countries diametrically opposite to ours. Nor will it warrant a man's pride, that he believes not the moon to be the only planet, that shines with a borrowed light, or the galaxy to be a meteor; since that now the telescope shews us, that *Venus* has her full and wain like the moon, and that the milky way is made up of a vast multitude of little stars, inconspicuous to the naked eye.

eye. And indeed of thoſe other diſcoveries, that overthrow the aſtronomy of the ancients, and much of their philoſophy about the celeſtial bodies, few or none have any cauſe to boaſt, but the excellent *Galileus*, who pretends to have been the inventor of the teleſcope: for that inſtrument once diſcovered, to be able to reject the ſeptenary number of the planets, by the detection of the four Satellites of *Jupiter*, or talk of the mountains and valleys in the moon, requires not much more excellency in your friend, than it would to deſcry in a ſhip, where the naked eye could diſcern but the body of the veſſel (to deſcry, I ſay) by the help of a proſpective glaſs, the maſts, and ſails, and deck, and perceive a boat towed at her ſtern: though indeed, *Galileo* himſelf had no great cauſe to boaſt of the invention, though we are much obliged to him for the improvement of the teleſcope, ſince no leſs a maſter of dioptricks than *Des Cartes* does acknowledge with other writers, that perſpective-glaſſes were not firſt found out by mathematicians or philoſophers, but caſually by one *Metius*, a Dutch ſpectacle-maker. On which occaſion I ſhall mind you, that to hide pride from man, divers others of the chief diſcoveries, that have been made in phyſicks, have been the productions, not of philoſophy, but chance, by which gun-powder, glaſs, and, for aught we know, the verticity of the load-ſtone, (to which we owe both the *Indies*) came to be found in theſe later ages; as (more recently) the milky veſſels of the myſentery, the new receptacle of the chyle, and that other ſort of veſſels, which moſt men call the lymphæ-ducts, were lighted on but by chance, according to the ingenuous confeſſion of the diſcoverers themſelves.

We may further conſider, that thoſe very things, which are juſtly alledged in the praiſe of the corpuſcularian philoſophy it ſelf, ought to leſſen the pride of thoſe, that but make uſe of it. For that hypotheſis, ſuppoſing the whole univerſe (the ſoul of man excepted) to be but a great Automaton, or ſelf-moving engine, wherein all things are performed by the bare motion (or reſt) the ſize, the ſhape, and the ſituation, or texture of the parts of the univerſal matter it conſiſts of; all the phænomena reſult from thoſe few principles, ſingle or combined, (as the ſeveral tunes or chimes, that are rung on five bells,) and theſe fertile principles being already eſtabliſhed by the inventors and promoters of the particularian hypotheſis; all that ſuch perſons, as your friend, are wont farther to do, is but to inveſtigate, or gueſs by what kind of motions the three or four other principles are varied. So that the world being but, as it were, a great piece of clock-work, the naturaliſt, as ſuch, is but a mechanician; however the parts of the engine, he conſiders, be ſome of them much larger, and ſome of them much minuter, than thoſe of clocks or watches. And for an ordinary naturaliſt to deſpiſe thoſe, that ſtudy the myſteries of religion, as much inferior to phyſical truths, is no leſs unreaſonable, than it were for a watch-maker, becauſe he underſtands his own trade, to deſpiſe privy-counſellors, who are acquainted with the ſecrets of monarchs, and myſteries of ſtate; or than it were for a ſhip-carpenter, becauſe he underſtands more of the fabrick of the veſſel, to deſpiſe the admiral, that is acquainted with the ſecret deſigns of the prince, and employed about his moſt important affairs.

That great reſtorer of phyſicks, the illuſtrious *Verulam*, who has traced out a moſt uſeful way to make diſcoveries in the intellectual globe, as he calls it, confeſſes, that his work was (to ſpeak in his own terms) *partus temporis potius quàm ingenii*. And though I am not of his opinion, where he ſays in another place, that his way of philoſophizing does *exæquare ingenia*; yet I am apt to think, that the fertile principles of the mechanical philoſophy being once ſettled, the methods of enquiring and experimenting being found out, and the phyſico-mechanical inſtruments of working on nature's and art's productions being happily invented, the making of ſeveral leſſer improvements, eſpecially by rectifying of ſome almoſt obvious of ſupine errors of the ſchools, by the aſſiſtance of ſuch facilitating helps, may fall to the lot of perſons not endowed with any extraordinary ſagacity, or accuteneſs of parts. And though the inveſtigation, and clear eſtabliſhment of the true principles of philoſophy, and the deviſing the inſtruments of knowledge, be things, that may be allowed to be the proper work of ſublimer wits; yet, if a man be furniſhed with ſuch aſſiſtances, it is not every diſcourſe, that he makes, or thing, which he does by the help of them, that is difficult enough to raiſe him to that illuſtrious rank. And indeed, divers of the vulgar errors, as well as of ſcholars as other men, being mainly grounded upon the mere and often miſtaken authority of *Ariſtotle*, and perhaps ſome frivolous reaſons of his ſcholaſtic interpreters of ſuch precarious and ungrounded things, that to ruin them, does oftentimes require more of boldneſs than ſkill; it may perhaps be ſaid of your friend, in relation to his philoſophical ſucceſſes againſt ſuch vulgar errors, as I am ſpeaking of, what a Roman ſaid of *Alexander*'s triumph over the effeminate Aſiaticks, *Quod nihil aliud quàm bene auſus ſit vana contemnere*. And in ſome caſes it happens, that, when once a grand truth, or a happy way of experimenting has been found, divers phænomena of nature, that had been left unexplained, or were left mis-explained by the ſchools, did, in my opinion, require a far leſs ſtraining exerciſe of the mind to unriddle and explain them, than muſt have been requiſite to diſpel the darkneſs, that attended divers theological truths, that are now cleared up, and perhaps than I have myſelf, now and then, employed in ſome of thoſe attempts, to illuſtrate theological matters, that you may have met in ſome papers, that I have preſumed to write on ſuch ſubjects. And indeed the improvements, that ſuch virtuoſi, as your friend, are wont to make of the fertile theorems and hints, that have been preſented them by the founders, or prime benefactors of true natural philoſophy, are ſo poor and ſlender, and do ſo much oftener proceed from

from induſtry and chance, than they argue a tranſcendent ſagacity, or a ſublimity of reaſon, that, though ſuch perſons may have cauſe enough to be delighted with what they have done, yet they have none to be proud of it; and their performances may deſerve our thanks, and perhaps ſome of our praiſe, but reach not ſo high, as to merit our admiration; which is to be reſerved for thoſe, that have been either framers, or grand promoters, of true and comprehenſive hypotheſis, or (elſe) the authors of other noble and uſeful diſcoveries, many ways applicable.

IT will not perhaps be improper to add on this occaſion, that, as our knowledge is not very deep, not reaching with any certainty to the bottom of things, nor penetrating to their intimate or innermoſt natures; ſo its extent is not very large, not being able to give us, with any clearneſs and particularity, an account of the celeſtial and deeply ſubterraneal parts of the world, of which all the others make but a very ſmall (not to ſay contemptible) portion.

FOR, as to the very globe, that we inhabit, not to mention, how many plants, animals, and minerals, we are as yet wholly ignorant of, and how many others we are but ſlenderly acquainted with; I conſider, that the objects, about which our experiments and enquiries are converſant, do all belong to the ſuperficial parts of the terreſtrial globe, of which the earth, known to us, ſeems to be but as it were the cruſt or ſcurf. But what the internal part of this globe is made up of, is no leſs diſputable, than of what ſubſtance the remoteſt ſtars we can deſcry, conſiſt: for even among the modern philoſophers ſome think, the internal portion of the earth to be pure and elementary earth, which, ſay they, muſt be found there, or no where. Others imagine it to be fiery, and the receptacle, either of natural or helliſh flames. Others will have the body of the terreſtrial globe to be a great and ſolid magnet. And the Carteſians on the other ſide, (though they all admit ſtore of ſubterraneal loadſtones) teach, that the ſame globe was once a fixed ſtar, and that, though it have ſince degenerated into a planet, yet the internal part of it is ſtill of the ſame nature, that it was before; the change it has received proceeding only from having had its outward parts quite covered over with thick ſpots (like thoſe to be often obſerved about the ſun,) by whoſe condenſation the firm earth we inhabit was formed. And the miſchief is, that each of theſe jarring opinions is almoſt as difficult to be demonſtratively proved falſe as true. For, whereas to the centre of the earth there is, according to the modeſteſt account of our late coſmographers, above three thouſand and five hundred miles; my enquiries among navigators and miners have not yet ſatisfied me, that men's curioſity has actually reached above one mile or two at moſt downwards, (and that not in above three or four places) either into the earth or into the ſea. So that as yet our experience has ſcarce grated any thing deep upon the huſk, if I may ſo ſpeak, without at all reaching the kernel of the terraqueous globe.

AND alas! what is this globe of ours, of which itſelf we know ſo little, in compariſon of thoſe vaſt and luminous globes, that we call the fixed ſtars, of which we know much leſs? For, though former aſtronomers have been pleaſed to give us, with a ſeeming accurateneſs, their diſtances and bigneſſes, as if they had had certain ways of meaſuring them; yet later and better mathematicians will, I know, allow me to doubt of what thoſe have delivered. For ſince it is confeſſed, that we can obſerve no parallax in the fixed ſtars, nor perhaps in the higheſt planets, men muſt be yet to ſeek for a method to meaſure the diſtance of thoſe bodies. And not only the Copernicans make it to be I know not how many hundred thouſands of miles greater than the Ptolomeans, and very much greater than even *Tycho*; but *Ricciolus* himſelf, though a great Anti-Copernican, makes the diſtance of the fixed ſtars vaſtly greater, than not only *Tycho*, but, if I miſ-remember not, than ſome of the Copernicans themſelves. Nor do I wonder at theſe ſo great diſcrepances, (though ſome amount, perhaps, to ſome millions of miles) when I conſider, that aſtronomers do not meaſure the diſtance of the fixed ſtars by their inſtruments, but accommodate it to their particular hypotheſis. And by this uncertainty of the remoteneſs of the fixed ſtars you will eaſily gather, that we are not very ſure of their bulk, no not ſo much as in reference to one another; ſince it remains doubtful, whether the differing ſizes, they appear to us to be of, proceed from a real inequality of bulk, or only from an inequality of diſtance, or partly from one of thoſe cauſes, and partly from the other.

BUT it is not my deſign to take notice of thoſe things, which the famous diſputes among the modern aſtronomers manifeſt to be dubious. For I conſider, that there are divers things relating to the ſtars, which are ſo remote from our knowledge, that the cauſes of them are not ſo much as diſputed of, or enquired into, ſuch as may be among others, why the number of the ſtars is neither greater nor leſſer than it is? why ſo many of thoſe celeſtial lights are ſo placed, as not to be viſible to our naked eyes, nor even when they are helped by ordinary teleſcopes? (which extraordinary good ones have aſſured me of.) Why among the familiarly viſible ſtars, there are ſo many in ſome parts of the ſky, and ſo few in others? why their ſizes are ſo differing, and yet not more differing? why they are not more orderly placed, ſo as to make up conſtellations of regular or handſome figures (of which the triangle is, perhaps, the ſingle example) but ſeem to be ſcattered in the ſky as it were by chance, and have as confuſed configurations, as the drops, that fall upon one's hat in a ſhower of rain? To which divers other queſtions might be added, as about the ſtars, ſo about the interſtellar part of heaven, which ſeveral of the modern Epicureans would have to be empty, ſave where the beams of light (and perhaps ſome other celeſtial effluvia) paſs through it; and the Carteſians, on the contrary, think to be full of an æthereal matter, which ſome, that are otherwise favourers of their philosophy, confess they are reduced to take up but as an hypothesis. So that our knowledge is much short of what many think, not only if it be considered intensively, but extensively, (as a School-man would express it.) For there being so great a disproportion between the heavens and the earth, that some moderns think the earth to be little better than a point in comparison even of the orb of the sun; and the Cartesians, with other Copernicans, think the great orb itself, (which is equal to what the Ptolomeans called the sun's orb) to be but a point in respect of the firmament; and all our astronomers agree, that, at least, the earth is but a physical point in comparison of the starry heaven: Of how little extent must our knowledge be, which leaves us ignorant of so many things, touching the vast bodies, that are above us, and penetrates so little a way even into the earth, that is beneath us, that it seems confined to but a small share of the superficial part of a physical point! of which consideration the natural result will be, that, though what we call our knowledge, may be allowed to pass for a high gratification to our minds, it ought not to puff them up; and what we know of the system, and the nature of things corporeal, is not so perfect and satisfactory, as to justify our despising the discoveries of spiritual things.

One of the former parts of this letter may furnish me with one thing more, to evince thy excellencies and prerogatives of the knowledge of the mysteries of religion; and that one thing is such, that I hope I shall need to add nothing more, because it is not possible to add any thing higher; and that is, that the pre-eminence above other knowledge adjudged to that of divine truths by a judge above all exception, and above all comparison, namely, by God himself.

This having been but lately shown, I shall not now repeat it, but rather apply what hath been there evinced, by representing, that if he, who determines in favour of divine truths, were such an one, as was less acquainted, than our over-weening naturalists, with the secrets of their idolized physicks; or if he were, though an intelligent, yet (like an angel) a bare contemplator of what we call the works of nature, without having any interest in their productions, your friend's not acquiescing in his estimate of things might have, though not a fair excuse, yet a stronger temptation.

But when he, by whose direction we prefer the higher truths revealed in the scripture, before those, which reason alone teaches us, concerning those comparitively mean subjects, things corporeal, is the same God, that not only understands the whole universe, and all its parts, far more perfectly, than a watch-maker can understand one of his own watches, (in which he can give an account only of the contrivance, and not of the cause of the spring, nor the nature of the gold, steel, and other bodies his watch consists of,) but did make both this great Automaton, the world, and man in it: we have no colour to imagine, that he should either be ignorant of, or injuriously disparage his own workmanship, or impose upon his favourite creature, man, in directing him what sort of knowledge he ought most to covet and prize. So that since it is he, who framed the world, and all those things in it we most admire, that would have us prefer the knowledge he has vouchsafed us in his word, before that, which he has allowed us of his works; sure it is very unreasonable and unkind, to make the excellencies of the workmanship a disparagement to the author, and the effects of his wisdom a motive against acquiescing in the decisions of his judgment; as if, because he is to be admired for his visible productions, he were not to be believed, when he tells us, that there are discoveries, that contain truths more valuable than those, which relate but to the objects, that he has exposed to all men's eyes.

SECTION V.

I DOUBT, I should be guilty of a most important omission, if I should here forget to consider one thing, which I fear has a main stroke in the partiality your friend expresseth in his preference of physicks to theology; and that is, that he supposes he shall, by the former, acquire a fame, both more certain and more durable, than can be hoped for from the latter.

And I acknowledge, not only with readiness, but with somewhat of gratulation of the felicity of this age, that there is scarce any sort of knowledge more in request, than that which natural philosophy pretends to teach; and that among the awakened and inquisitive part of mankind, as much reputation and esteem may be gained by an insight into the secrets of nature, as by being entrusted with those of princes, or dignified with the splendidest marks of their favour.

But though I readily confess thus much, and though perhaps I may be thought to have had, I know not by what fate, as great a share of that perfumed smoak, applause, as (at least) some of those, which among the writers, that are now alive, your friend seems most to envy for it; yet I shall not scruple to tell you, partly from observation of what has happened to others, and partly too upon some little experience of my own, that neither is it so easy, as your friend seems to believe it, to get by the study of nature a sure and lasting reputation, neither ought the expectation of it, in reason, make men undervalue the study of divinity. Nor would it here avail to object (by way of prevention) that the difficulties and impediments of acquiring and securing reputation lie as well in the way of divines as philosophers, since this objection has been already considered at the beginning of this second part of our present tract. Besides that the progress of our discourse will shew, that the naturalist, aspiring to fame, is liable to some inconveniences, which are either not at all, or not near equally incident to the divine. Wherefore, without staying to take any further notice of this preventive allegation, I shall proceed to make good the first part of the assertion, that preceded it; which that I may the more

more fully do, give me leave (after having premised, that a man muſt either be a writer, or forbear to print what he knows;) to propoſe to you the following conſiderations.

And firſt, if your *Phyſeophilus* ſhould think to ſecure a great reputation, by forbearing to couch any of his thoughts or experiments in writing, he may thereby find himſelf not a little miſtaken. For if once he have gained a repute (upon what account ſoever) of knowing ſome things, that may be uſeful to others, or of which ſtudious men are wont to be very deſirous, he will not avoid the viſits and queſtions of the curious. Or, if he ſhould affect a ſolitude, and be content to hide himſelf, that he may hide the things he knows; yet he will not eſcape the ſolicitations, that will be made him by letters. And if theſe ways of tempting him to diſcloſe himſelf, prevail not at all with him to do ſo, he will provoke the perſons, that have employed them; who finding themſelves diſobliged, by being defeated of their deſires, if not alſo their expectations, will for the moſt part endeavour to revenge themſelves on him, by giving him the character of an uncourteous and ill-natured perſon; and will endeavour, perhaps, ſucceſsfully enough, to decry his parts, by ſuggeſting, that his affected concealments proceed but from a conſcientiouſneſs, that the things he is preſumed to poſſeſs, are but ſuch, as, if they ſhould begin to be known, would ceaſe to be valued.

You will ſay (perchance,) that ſo much reſervedneſs is a fault: nor ſhall I diſpute it with you, whether it be or not; but if he be open and communicative in diſcourſe to thoſe ſtrangers, that come to pump him, ſuch is the diſingenuous temper of too too many, that he will be in great danger of having his notions or experiments arrogated by thoſe, to whom he imparts them, or at leaſt, by others, to whom thoſe may (though perchance deſignleſsly) happen to diſcourſe of them. And then, if either *Phyſeophilus*, or any of his friends, that know him to be author of what is thus uſurped, ſhould mention him as ſuch, the uſurpers and their friends would preſently become his enemies; and, to ſecure their own reputation, will be ſolicitous to leſſen and blemiſh his. And if you ſhould now tell me, that your friend might here take a middle way, as that, which in moſt moſt caſes is thought to be the beſt, by diſcourſing at ſuch a rate of his diſcoveries, as may ſomewhat gratify thoſe, that have a curioſity to learn them, and yet not ſpeak ſo clearly, as diveſt himſelf of his propriety in them; I ſhould reply, that neither is this expedient a ſure one, nor free from inconveniencies. For moſt men are ſo ſelf-opinionated, that they will eaſily believe themſelves maſters of things, if they do but half underſtand them. And however, though the perſons, to whom the diſcourſe was immediately made, ſhould not have too great an opinion of themſelves, no more than too great a ſagacity; yet they may eaſily, by repeating what they heard and obſerved, give ſome more piercing wit a hint ſufficient to enable him to make out the whole notion, or the diſcovery, which he will then without ſcruple, and without almoſt any poſſibility of being diſproved, aſſume for his own. But if it happen, (as it often will in extemporaneous diſcourſe) that a philoſopher be not rightly underſtood, either becauſe he has not the leiſure, no more than a deſign, to explain himſelf fully, or becauſe the perſons he converſes with, bring not a competent capacity and attention; he then runs a greater danger than before. For the vanity moſt men take in being known to have converſed with eminent philoſophers, makes them very forward to repeat what they heard ſuch a famous wit ſay; and oftentimes being ſecure of not being contradicted, ignorantly to miſ-recite it, or wittingly to wreſt it in favour of the opinion they would countenance by it. So that, whereas by the formerly mentioned frankneſs of diſcourſe, he is only in danger to have the truths he diſcovered arrogated by others, this reſervedneſs expoſes him to have opinions and errors, that he never dreamed of, fathered on him. And when a man's opinions, or diſcoveries, come once to be publikly diſcourſed of, without being propoſed by himſelf, or ſome friend well inſtructed by him, he knows not what errors or extravagancies may be imputed to him (and that without a moral poſſibility left to moſt men to diſcern them,) by the miſtake of the weak, or the diſingenuity of the partial, or the artifices of the malicious. And even the greatneſs of a man's reputation does ſometimes give ſuch countenance to vain reports and ſurmiſes, as by degrees to ſhake, if not ruin it. As we ſee, that Frier *Bacon*, and *Trithemius*, and *Paracelſus*, who, for their times, were knowing, as well as famous men, had ſuch feats aſcribed to them, as by appearing fabulous to moſt of the judicious, have tempted many to think, that all the great things, that were ſaid of them, were ſo too.

These are ſome of the inconveniencies, that a naturaliſt may be liable to, if he forbear the communicating of his thoughts and diſcoveries himſelf: but if *Phyſeophilus* ſhould, to ſhun theſe, aſpire to fame by the uſual way of writing books, he may indeed avoid theſe, but perhaps, not without running into other inconveniencies and hazards, very little inferior to them.

First then, we may conſider, that whether a man writes in a ſyſtematical way, as they have done, who have publiſhed entire bodies of natural philoſophy, or methodical treatiſes of ſome conſiderable part of it; or whether he write in a more looſe and unconfined way, of any particular ſubject, that belongs to phyſicks; which ſoever, I ſay, of theſe two ways of writing books he ſhall make choice of, he will find it liable to inconvenience enough.

For if he write ſyſtematically, firſt, he will be obliged, that he may leave nothing neceſſary undelivered, to ſay divers things, that have been ſaid (perhaps many times) by others already, which cannot but be unpleaſant, not only to the reader, but (if he be ingenious) to to the writer. Next, there are ſo many things in nature, whereof we know little or nothing, and ſo many more, of which we do not know enough, that our ſyſtematical writer, though we ſhould grant him to be very learned, muſt needs, either leave divers things, that belong to his theme, untreated of, or diſcourſe of them ſlightly, and oftentimes (in likelihood) erroneouſly. So that in this kind of books there is always much ſaid, that the reader did know, and commonly not a little, that the writer does not know. And to this, I muſt add, in the third place, that natural philoſophy, being ſo vaſt and pregnant a ſubject, that (eſpecially in ſo inquiſitive an age as this) almoſt every day diſcovers ſome new thing or other about it, it is ſcarce poſſible for a method, that is adapted but to what is already known, to continue long the moſt proper; as the ſame clothes will not long fit a child, whoſe age will make him quickly out-grow them. And therefore ſucceeding writers will have a fair pretence to compile new ſyſtems, that may be more adequate to philoſophy, improved ſince the publication of the former. And though there were little of new to be added, and it were more eaſy to alter, than to mend the method of our ſuppoſed author; yet novelty itſelf is a thing ſo pleaſing and inviting to the generality of men, that it often recommends things, that have nothing elſe to recommend them; and we may apply to a great many other things, what, I remember, a famous courtier of my acquaintance uſed to ſay of miſtreſſes, that another was preferable to a better, (the better being but the ſame.)

But now, if, declining the ſyſtematical way, one ſhould chooſe the other of writing looſe tracts and diſcourſes, he may indeed avoid ſome of the lately mentioned inconveniencies, but will ſcarce avoid the being plundered by ſyſtematical writers: for theſe will be apt to cull out thoſe things, that they like beſt, and inſert them in their methodical books, (perhaps much curtailed, or otherwiſe injured in the repeating,) and will place them, not as their own author did, where they may beſt confirm or adorn his diſcourſe, and be illuſtrated or upheld by it; but where it may beſt ſerve the turn of the compiler: and theſe methodical books promiſe ſo much more compendious a way, than others, to the attainment of the ſciences they treat of, that though really for the moſt part they prove greater helps to the memory, than the underſtanding; yet moſt readers being, for want of judgment, or of patience, of another mind, they are willing to take it for granted, that in former writers, if there have been any thing conſiderable, it has been all carefully extracted, as well as orderly digeſted by the later compilers: and though I take this to be a very erroneous and prejudicial conceit, yet it obtains ſo much, that as goldſmiths, that only give ſhape and luſtre to gold, are far more eſteemed, and in a better condition, than miners, who find the ore in the bowels of the earth, and with great pains and induſtry dig it up, and refine it into metal; ſo thoſe, that with great ſtudy and toil, ſucceſsfully penetrate into the hidden receſſes of nature, and diſcover latent truths, are uſually leſs regarded, or taken notice of, by the generality of men, than thoſe, who by plauſible methods, and a neat ſtile, reduce the truths, that others have found out, into ſyſtems of a taking order, and a convenient bulk.

I conſider in the ſecond place, that as the method of the books one writes, ſo the bulk of them may prove prejudicial to the naturaliſt, that aſpires to fame: for if he write large books, it is odds but that he will write in them many things unaccurate, if not impertinent, or that he will be obliged to repeat many things, that others have ſaid before; and if he write but ſmall tracts, as is the cuſtom of the judicioufeſt authors, who have no mind to publiſh but what is new and conſiderable, as their excellency will make them to be the ſooner diſperſed, ſo the ſmallneſs of the bulk will endanger them to be quickly loſt, as experience ſhews us of divers excellent little tracts, which, though publiſhed not many years ago, are already out of print, (as they ſpeak) and not to be met with, ſave by chance, in ſtationers ſhops. So that theſe writings (which deſerve a better fate) come, after a while, either to be loſt (which is the caſe of divers,) or to have their memory preſerved only in the larger volume of ſome compiler, whoſe induſtry is only preferable to his judgment; it being obſervable, that (by I know not what unlucky fate) very few (for I do not ſay none) that addict themſelves to make collections out of others, have the judgment to cull out the choiceſt things in them; and the ſmall tracts, we are ſpeaking of, being preſerved but in ſuch a quoter or abridger, will run a very great danger of being conveyed to poſterity but under ſuch a repreſentation, as it pleaſes the compiler.

And this (that I may proceed to my third conſideration) may make the naturaliſt's fame very uncertain, not only becauſe of the want of judgment, that (as I newly ſaid) is too often obſervable in compilers, whereby they frequently leave far better things than they take, but for the want of ſkill to underſtand the author they cite and epitomize, or candor to do him right. For ſometimes men's phyſical opinions, and ſeveral paſſages of their writings, are ſo miſrepreſented by miſtake or deſign, eſpecially if thoſe, that recite their opinions be not of them, that men are made to teach or deliver things quite differing from their ſenſe, and perhaps quite contrary to it; of which I myſelf have had ſome unwelcome experience, a learned writer pretending, I know not how often, that I aſſerted an opinion, about which I did expreſly ἐπέχειν. And another noted writer having (not out of deſign, but unacquaintedneſs with mechanicks, and the ſubject I writ of,) given me commendations for having, by a new experiment, proved a thing, the quite contrary whereof I intended thereby to evince, and am not alone miſtaken, if I did not do it. Other naturaliſts I have met with, whoſe writings compilers have traduced out of hatred to their perſons, or their religion; as if truth could in nothing be a friend to one, that is the traducer's traducer's enemy; or as if a man, that falls into an error in religion, could not light upon a good notion in philosophy, in spite of all the truths we owe to *Aristotle*, *Epicurus*, and the other heathen philosophers. Nay, some there are, that will set themselves to decry a man's writings, not because they are directly his enemies, but because he is esteemed by theirs; as you may remember an instance in a servant of yours, who had divers things written against him upon this very account. Nor is it only by the citations of professed adversaries, or opponents, that a worthy writer's reputation may be prejudiced, since it is not unfrequently so by those, that mention him with an encomium, and seem disposed to honour him. For I have observed it to be the trick of certain writers, to name an author with much compliment, only for some one or few of the least considerable things they borrow of him; by which artifice they endeavour to conceal their being plagiaries of more and better; which yet is more excusable than the practice of some, who proceed to that pitch of disingenuity, that they will rail at an author, to whom indeed they owe too much, that they may not be thought to be beholden to him.

But (4.) I must add, that besides these dangers, that a naturalist's reputation with posterity may run through the ignorance or perverseness of men, it is liable to divers other hazards, from the very nature both of men, of opinions, and of things.

For, as men's geniusses and inclinations are naturally various in reference to studies, one man passionately affecting one sort of them, and another being fond of quite differing ones; so those inclinations are oftentimes variously and generally determined by external and accidental causes. As when some great monarch happens to be a great patron, or a despiser, and perhaps adversary of this or that kind of learning; and when some one man has gained much applause for this or that kind of study; imitation or emulation oftentimes makes many others addict themselves to it. Thus though *Rome*, under the consuls, was inconsiderable for learning, yet the reputation of *Cicero*, and favour of *Augustus*, brought learning into request there; where the small countenance it met with amongst most of the succeeding emperors, kept it far inferior to what it had been among the Greeks about *Alexander*'s age. And the age of the same *Augustus* was ennobled with store of poets, not only by the countenance, which he and *Mæcenas* afforded them, but probably also by the examples they gave to, and the emulation they excited in one another. And after the decay of the Roman empire, in the fourth century, natural philosophy and the mathematicks being very little valued, and less understood, by reason that men's studies were by the genius of those ages applied to other subjects, every hundred years scarce produced one improver, (not to say one eminent cultivator) either of mathematicks or of physicks: by which you may see, how little certainty there is, that, because a man is skilled in natural philosophy, and that science is now in request, his reputation shall be as great as now, when perhaps the science itself will be grown out of repute.

But besides the contingencies, that may happen to a naturalist's fame upon this account, that the science he cultivates is, as well as others, subject to wanes and eclipses in the general esteem of men; there is another uncertainty arising from the vicissitudes, that are to be met with in the estimates men make of differing hypotheses, sects, and ways of philosophizing about the same science, and particularly about natural philosophy. For during those learned times, when physicks first and most flourished among the Grecians, *Democritus*, *Leucippus*, *Epicurus*, *Anaxagoras*, *Plato*, and almost all the naturalists, that preceded *Aristotle*, were Corpuscularians, endeavouring, though not all by the same way, to give an account of the phænomena of nature, and even of qualities themselves, by the bigness, shape, motion, &c. of corpuscles, or the minutest active parts of matter: whereas *Aristotle*, having attempted to deduce the phænomena from the four first qualities, the four elements, and somefew other barren hypotheses, ascribing what could not be explicated by them, (and consequently far the greatest part of nature's phænomena) to substantial forms and occult qualities; (principles, that are readily named, but scarce so much as pretended to be understood,) and having upon these slight and narrow principles reduced physicks into a kind of system, which the judicious modesty of the Corpuscularians had made them backward to do; the reputation, that his great pupil *Alexander*, as well as his learning gave him; the easiness of the way he proposed to the attainment of natural philosophy; the good luck his writings had to survive those of *Democritus*, and almost all the rest of the Corpuscularians, when *Charles* the Great began to establish learning in *Europe*: these, I say, and some other lucky accidents, that concurred, did for about seven or eight hundred years together, make the Corpuscularian philosophy not only be justled, but even exploded out of the schools by the Peripatetick; which in our times is, by very many, upon the revival of the Corpuscularian philosophy, rejected, and, by more than a few, derided as precarious, unintelligible, and useless. And to give an instance in a particular thing, (which, though formerly named, deserves to be again mentioned to our present purpose,) *Aristotle* himself somewhere confesses, (not to say brags) that the Greek philosophers, his predecessors, did unanimously teach, that the world was (I say not created, but) made, and yet he, almost by his single authority, and the subtile arguments (as some have been pleased to think them,) that he employed, (though divers of them were borrowed of *Ocellus Lucanus*,) was able for many ages to introduce into the schools of philosophers that irreligious and ill-grounded opinion of the eternity of the world, which afterwards the Christian doctrine made men begin to question, and which now, both that and right reason have persuaded most men to reject.

And

And this invites me to consider farther, that the present success of the opinions, that your *Physeophilus* befriends, ought not to make him so sure, as he thinks he is, that the same opinions will be always in the same, or a greater vogue, and have the same advantages, in point of general esteem, that they now have, over their corrivals. For, opinions seem to have their fatal seasons and vicissitudes, as well as other things; as may appear, not only by the examples of it newly given, but also by the hypothesis of the earth's motion, which having been in great request before *Pythogaras*, (who yet is commonly thought the inventor of it,) had its reputation much encreased by the suffrage of the famous sect of the Pythagoreans, (whom *Aristotle* himself takes notice of as the patrons of that opinion;) and yet afterwards for near 2000 years it was laughed at, as not only false, but ridiculous. After all which time, this so long antiquated opinion being revived by *Copernicus*, has in a little time made so great a progress among the modern astronomers and philosophers, that if it go on to prevail at the same rate, the motion of the earth will be acknowledged by all its mathematical inhabitants. But though it be often the fate of an oppressed truth, to have at length a resurrection, yet it is not always its peculiar privilege; for obsolete errors are sometimes revived, as well as discredited truths: so that the general disrepute of an opinion in one age will not give us an absolute security, that it will not be in as general request in another, in which it may perhaps, not only revive, but reign.

Nor is it only in the credit of men's opinions about philosophical matters, that we may observe an inconstancy and vicissitude, but in the very way and method of philosophizing; for *Democritus*, *Plato*, *Pythagoras*, and others, who were of the more sincere and ingenious cultivators of physicks among the Greeks, exercised themselves chiefly either in making particular experiments and observations, as *Democritus* did in his manifold dissections of animals; or else applied the mathematicks to the explicating of a particular phænomenon of nature, as may appear (not to mention what *Hero* teaches in his Pneumaticks,) by the accounts, *Democritus*, *Plato*, and others, give of fire and other elements, from the figure and motion of the corpuscles they consist of. And although this way of philosophizing were so much in request before *Aristotle*, that (albeit he unluckily brought in another, yet) there are manifest and considerable footsteps of it to be met with in some of his writings, (and particularly in his books of animals, and his mechanical questions;) yet the scholastick followers of *Aristotle* did, for many ages, neglect the way of philosophizing of the antients, and (to the great prejudice of learning) introduced every where, instead of it, a quite contrary way of writing. For, not only they laid aside the mathematicks, (of which they were for the most part very ignorant,) but instead of giving us intelligible and explicite (if not accurate) accounts of particular subjects, grounded upon a distinct and heedful consideration of them, they contented themselves with hotly disputing, in general, certain unnecessary, or at least unimportant questions about the objects of physicks, about *Materia Prima*, substantial forms, privation, place, generation, corruption, and other such general things, with which when they had quite tired themselves and their readers, they usually remained utter strangers to the particular productions of that nature, about which they had so much wrangled, and were not able to give a man so much true and useful information about particular bodies, as even the meanest mechanicks, such as mine-diggers, butchers, smiths, and even dairy-maids, could do. Which made their philosophy appear so imperfect and useless, not only to the generality of men, but to the more elevated and philosophical wits, that our great *Verulam* attempted with much skill and industry, (and not without some indignation) to restore the more modest and useful way practised by the antients, of enquiring into particular bodies, without hastening to make systems, into the request it formerly had; wherein the admirable industry of two of our *London* physicians, *Gilbert* and *Harvey*, has not a little assisted him. And I need not tell you, that since him, *Des Cartes*, *Gassendus*, and others, having taken in the application of geometrical theorems, for the explication of physical problems; he, and they, and other restorers of natural philosophy, have brought the experimental and mathematical way of enquiring into nature, into at least as high and growing an esteem, as ever it possessed when it was most in vogue among the naturalists, that preceded *Aristotle*.

To the considerations I have hitherto deduced, which (perhaps) might alone suffice for my purpose, I shall yet subjoin one, that I take to be of greater weight than any of them, for the manifesting, how difficult it is to be sure, that the physical opinions, which at present procure a champion or promoter of them veneration, shall be still in request. For besides that inconstant fate of applauded opinions, which may be imputed to the inconstancy of men, there is a greater danger, that threatens the aspirer's reputation from the very nature of things: for the most general principles of all, viz. the figure, bigness, motion, and other mechanical affections of the small parts of matter, being (as your friend believes) sufficiently and clearly established already; he must expect to raise his reputation from subordinate hypotheses and theories; and in these I shall not scruple to say, that it is extremely difficult, even for those, that are more exercised than he in framing them, and in making of experiments to have so reaching and attentive a prospect of all things fit to be known, as not to be liable to have their doctrine made doubtful, or disproved by something, that he did not discover, or that aftertimes may. This, I doubt not, but you would easily be prevailed with to allow, if I had leisure and conveniency to transmit to you my sceptical naturalist. And without having recourse

course to that tract, it may possibly suffice, that we consider, that one of the conditions of a good * hypothesis is, that it fairly comport not only with all other truths, but with all other phænomena of nature, as well as those it is framed to explicate. For this being granted, (which cannot be denied,) he, that establishes a theory, which he expects shall be acquiesced in by all succeeding times, and make him famous in them, must not only have a care, that none of the phænomena of nature, that are already taken notice of, do contradict his hypothesis at the present, but that no phænomena, that may be hereafter discovered, shall do it for the future. And I very much question, whether *Physeophilus* do know, or, upon no greater a number and variety of experiments than most men build upon, can know, how incompleat the history of nature we yet have, is, and how difficult it is to build an accurate hypothesis upon an incompleat history of the phænomena it is to be fitted to; especially considering, that (as I was saying) many things may be discovered in after-times by industry or chance, which are not now so much as dreamed of, and which may yet overthrow doctrines speciously enough accommodated to the observations, that have been hitherto made.

Those ancient philosophers, that thought the torrid zone to be uninhabitable, did not establish their opinion upon wild reasonings; and as it continued uncontrouled for many ages, so perhaps it would have always done, if the discoveries made by modern navigations had not manifested it to be erroneous. The solidity of the celestial orbs was, for divers centuries above 1000 years, the general opinion of astronomers and philosophers; and yet in the last age, and in ours, the free trajection, that has been observed in the motion of some comets, from one of the supposed orbs to another, and the intricate motions in the planet *Mars*, (observed by *Kepler* and others, to be sometimes nearer, as well as sometimes remoter from the earth than is the sun;) these, I say, and other phenomena undiscovered by the ancients, have made even *Tycho*, as well as most of the recent astromoners, exchange the too long received opinion of solid orbs for the more warrantable belief of a fluid æther. And though the celestial part of the world, by reason of its remoteness from us, be the most unlikely of any other to afford us the means of over-throwing old theories by new discoveries; yet even in that we may take notice of divers instances to our present purpose, though I shall here name but this one, viz. That, after the Ptolemaick number and order of the planets had past uncontradicted for very many ages; and even the Tychonians and Copernicans, (however they did, by their differing hypotheses, dissent from the Ptolemaick system (as to the order) did (yet) acquiesce in it as to the number of the planets; by the happy discoveries, made by *Galilæo* of the Satellites of *Jupiter*, and by the excellent *Hugenius*, of the new planet about *Saturn*, (which I think I had the luck to be the first, that observed and shewed disbelievers of it in *England)* the astronomers of all persuasions are brought to add to the old septenary number of the planets, and take in five others, that their predecessors did not dream of. That the chyle prepared in the stomach passed through the mesaraick veins to the liver, and so to the heart, was for many ages the unanimous opinion, not only of physicians, but anatomists, whose numerous dissections did not tempt them to question it; and yet, since the casual, though lucky, discoveries made of the milky vessels in the thorax by the dextrous *Pecquet*, those, that have had with you and me the curiosity to make the requisite experiments, are generally convinced, that, at least, a good part of the chyle goes from the stomach to the heart, without passing through the mesaraick veins, or coming at all to the liver.

It were easy to multiply instances of this kind, but I rather chuse to add, that it is not only about the qualities, and other attributes of things, but about their causes also, that new, and oftentimes accidental discoveries may destroy the credit of long and generally approved opinions. That quick-lime exceedingly heats the water, that is poured on to quench it, on the account of *Antiperistasis*, has been very long and universally received by the school-philosophers, where it is the grand and usual argument, urged to establish *Antiperistasis* †; and yet I presume you have taken notice, that this proof is made wholly ineffectual in the judgment of many of the virtuosi, by some contrary experiments of mine, and particularly that of exciting in quick-lime full as great an effervescence by the affusion of hot water instead of cold. So it has been generally believed, that in the congelation of water, that liquor is condensed into a narrower room; whereas our late experiments ‖ have satisfied most of the curious, that ice is water expanded, or, if you please, that ice takes up more room than the water did, whilst it remained unfrozen. And whereas the notion of nature's abhorrence of a vacuum has not only, ever since *Aristotle*'s time, made a great noise in the schools, but seems to be confirmable by a multitude of phænomena; the experiments of *Torricellius* and some of ‡ ours, evidencing, that the air has a great weight and a strong spring, have, I think, persuaded almost all, that have impartially considered them, that, whether there be or be not such a thing, as they call *fuga vacui*, yet suction, and the ascension of water in pumps, and those other phænomena, that are generally ascribed to it, may be very well explicated without it, and are, indeed, caused by the weight of the atmosphere, and the elastical power of the air.

And this puts me in mind to take notice, that even practical inventions, where one would think the matter of fact to be evident, may, by un-

* See the requisites of a good hypothesis.
† See this subject handled at large in an appendix to the author's *Examen of Antiperistasis*.
‖ In the history of cold.
‡ Now published in the book of new physico-mechanical experiments.

undreamed-of discoveries be brought to lose the general reputation they had for compleatness in their kind. For to endear the invention of sucking pumps, and of syphons, it has been generally presumed, that by means of either of these, water and any other liquor may, *ob fugam vacui*, be raised to what height one pleases; and accordingly ways have been proposed by famous authors, to convey water from one side of an high mountain to the other: whereas, first, the unexpected disappointments, that were met with by some pump-makers, and afterwards experiments purposely made, sufficiently evince, that neither a pump nor a siphon will raise water to above 35 foot, or thereabouts, nor quick-silver to so many inches.

And as to the invention of weather-glasses, which has been so much, and justly applauded and used, as it has been generally received for the truest standard of the heat and cold of the weather; so it seems to be liable to no suspicion of deceiving us: for not only it is evident, that in winter, when the air is very cold, the water rises much higher than in summer, and other seasons, when it it not so; but if you but apply your warm hand to the bubble at the top, the water will be visibly depressed by the rarified air, which upon the removal of the hand returning to its former coldness, the water will forthwith as manifestly ascend again. * And yet by finding, that, as the atmosphere has a considerable weight, so this weight is not always the same, but varies much, and that, as far as I can yet discover, uncertainly enough; I have had the luck to satisfy many of the curious, that these open thermometers are not to be safely relied on, since in them the liquor is made to rise and fall, not only, as men have hithertherto supposed, by the cold and heat of the ambient air, but (as I have shewn by divers new experiments) according to the varying gravity of the atmosphere; which variation has not only a sensible, but a very considerable influence upon the weather-glass. To these instances I shall annex only one more, from which we may learn, that notwithstanding a very heedful survey of all, that at present a man can take notice of, or well suspect, that he ought to take into his consideration, the case may be such, that having devised an instrument, he may use it many years with good success; and yet, unless he were able to live very many more, he shall not be sure to outlive the danger of finding the same instrument (though to sense as well conditioned as ever) fallacious: as he, that first applied a magnetick needle to the finding of the meridian line, might very probably conclude, that his needle pointing directly N. and S. or declining from it just two or three, or some other determinate number of degrees, he had discovered a certain and ready way, without the help of sun or stars, or astronomical instruments, to describe a meridian line, and if he lived but an ordinary number of years after his observation, he might probably have found his instrument not deceitful; which yet it may now be, the magnetick needle, not only declining in many places from the true points of N. and S. but (as later discoveries inform us) varying in tract of time its declination in the self same place.

The considerations hither to proposed might easily enough be increased by more of the same tendency, especially if I thought fit to borrow from a discourse (of mine) purposely written about the partiality and uncertainty of fame; but instead of adding to their number, I should think myself obliged to excuse my having already mentioned so many, and insisted so much upon them, if I did not vehemently suspect, that in your *Physeophilus*, (as well as in many other modern naturalists) scarce any thing does more contribute to an undervaluation of the study of divinity, than, that being eagerly ambitious of a certain, as well as a posthume fame, he is confident, that physiology will help to it; and therefore, the design of his discourse made me think it expedient to spend some time to manifest, "that "it is far less easy than he thinks, to be as "sure, that he shall have the praises of future "ages, as that (though he have them) he shall "not hear them."

The past considerations have, I presume, convinced you, that it is no such easy matter for a naturalist to acquire a great reputation, and be sure it will prove a lasting one. Wherefore, that I may also confirm the second part of what formerly I proposed, I now proceed to show, that, though the case were otherwise, yet he would have no reason to slight the study of divinity.

1. For, in the first place, nothing hinders, but that a man, who values and enquires into the mysteries of religion, may attain to an eminent degree in the knowledge of those of nature. For frequently men of great parts may successfully apply themselves to more than one study; and few of them have their thoughts and hours so much engrossed by that one subject or employment, but that, if they have great inclinations, as well as fitness for the study of nature, they will find time, not only to cultivate it, but to excel in it. You need not be told, that *Copernicus*, to whom our late philosophers owe so much, was a churchman; that his champion *Lansbergius* was a minister, and that *Gassendus* himself was a doctor of divinity. Among the Jesuits you know, that *Clavius*, and divers others, have as prosperously addicted themselves to mathematicks as divinity. And as to physicks, not only *Scheiner*, *Aquilonius*, *Kircher*, *Schottus*, *Zucchius*, and others, have very laudably cultivated the optical and some other parts of philosophy; but *Ricciolus* himself, the learned compiler of that voluminous and judicious work of the *Almagestum novum*, wherein he has inserted divers accurate observations of his own, is not only a divine, but a professor of divinity. And without going out of our own country, I could, if I durst for fear of offending the modesty of those I should name, or injuring the merit of those I should omit; I could, I say, if it

* See a Tract on this Subject, premised by the Author to his Book of Cold.

were not for this, among our English ecclesiasticks name you divers, who though they apply themselves so much to the study of the scripture, as to be not only solid divines, but excellent preachers, have yet been so happily conversant with nature, that, if they had lived in the learned times of the Greeks, they would have rivalled, if not eclipsed, some of them, *Pythagoras* and *Euclid*; others of them, *Anaxagoras* and *Epicurus*; and some of them, even *Archimedes* and *Democritus* themselves.

AND certainly, provided there be curiosity and industry enough employed in the study of nature, it is not necessary, that the knowledge of nature should be the ultimate end of that study; a fondness of the object being required only in order to the engaging the mind to such a serious application, as a higher aim may sufficiently invite us to; and will rather promote than discourage. *David* became no less skilful in musick, than those, that were addicted to it only to please themselves in it; though we may reasonably suppose, that so pious an author of psalms and instruments aspired to an excellency in that delightful science, that he might apply and prefer it to the service of the temple, and promote the celebration of God's praises with it. And as experience has manifested, that the heathen philosophers, that courted moral virtue for herself, did not raise it to that pitch, to which it was advanced by the heroick practises of those true Christians, that in the highest exercise of virtue had a religious aim at the pleasing and enjoying of God; so I see not, why natural knowledge must be more prosperously cultivated by those selfish naturalists, that aim but at the pleasing of themselves in the attainment of that knowledge, than those religious naturalists, who are invited to attention and industry, not only by the pleasantness of the knowledge it self, but by a higher and more engaging consideration; namely, that by the discoveries they make in the book of nature, both themselves and others may be excited and qualified the better to admire and praise the author, whose goodness does so well match the wisdom they celebrate, that he declares in his word, that "those, that "honour him, he will honour."

Amos vi. 5.

1 Sam. ii. 30.

AND as a man, that is not in love with a fair lady, but has only a respect for her, may have as true and perfect, though not as discomposing an idea of her face, as the most passionate Inamorato; so I see not, why a religious and inquisitive contemplator of nature may not be liable to give a good account of her, without preferring her so far to all other objects of his study, as to make her his mistress, and perhaps too his idol.

II. AND now I proceed to consider in the second place, that matters of divinity may, as well as those of philosophy, afford a reputation to him, that discovers, or illustrates them. For though the fundamental articles of Christian religion be, as I have formerly declared, little less evident than important; yet there are many other points in divinity, and passages in the scripture, which (for reasons, that I have elsewhere mentioned) are exceeding hard to be cleared, and do not only pose ordinary readers, and the common sort of scholars, but will sufficiently exercise the abilities of a great wit, and give him opportunity enough to manifest, that he is one. For divers of the points I speak of, are much benighted upon the score of the sublimity of the things they treat of; such as are the nature, attributes, and decrees of God, which cannot be easy to the dim understandings of us, that are but men: and many other particulars, that are not abstruse in their own nature, are yet made obscure to us by our ignorance, (or at least imperfect knowledge,) of the disused languages, wherein they are delivered, and the great remoteness of the ages when, and the countries where, the things recorded were done or said. So that oftentimes a man may need and show as great learning and judgment to dispel the darkness, wherein time has involved things, as that, which nature has cast on them: and in effect we see, that St. *Augustin*, St. *Hierom*, *Origen*, and others of the fathers, have acquired no less a reputation, than *Empedocles*, *Anaxagoras*, or *Zeno*; and *Grotius*, *Salmasius*, Mr. *Mede*, Dr. *Hamond*, and some other critical expounders of difficult texts of scripture, have thereby got as much credit, as *Fracastorius* by his book *De Sympathia & Antipathia*; *Levinus Lemnius* by his *De occultis rerum Miraculis*; or *Cardanus* (and his adversary *Scaliger*) by what they writ *De Subtilitate*; or even *Fernelius* himself by his book *De Abditis rerum Causis*. And it will contribute to the credit, which theological discoveries and illustrations may procure a man, that the importance of the subjects, and the earnestness, wherewith men are wont to busy themselves about them, some upon the score of piety, and others upon that of interest, some to learn truths, and others to defend what they have long or publickly taught for truth, does make greater numbers of men take notice of such matters, and concern themselves far more about them, than about almost any other things, and especially far more, than about matters purely philosophical, which but few are wont to think themselves fit to judge of, and concerned to trouble themselves about. And accordingly we see, that the writings of *Socinus Calvin*, *Bellarmine*, *Padre Paulo*, *Arminius*, *&c.* are more famous, and more studied, than those of *Telesius*, *Campanella*, *Severinus Danus*, *Magnenus*, and divers other innovators in natural philosophy. And *Erastus*, though a very learned physician, is much less famous for all his elaborate disputations against *Paracelsus*, than for the little tract against particular forms of church-government. And I presume you have taken notice, as well as I, that there are scarce any five new controversies in all physicks, that are known to, and hotly contended for by so many, as are the five articles of the Remonstrants.

III. My second consideration being thus dispatched, it remains, that I tell you in the third place, that supposing, but not granting, that to prosecute the study of divinity, one must of necessity neglect the acquist of reputation; yet this inconvenience itself ought not to deter us from the duty they would dissuade. For in all deliberations, wherein any thing is proposed to be quitted or declined, to obey or please God; methinks, we may fitly apply that of the prophet to the Jewish king, who being persuaded (to express his concern for God's glory) to decline the assistance of an idolatrous army of *Israelites*, and objecting, that by complying with the advice given him, he should lose a sum of money, amounting to no less than the hire of a potent army; received from the prophet this brisk, but rational answer, 2 Chron. xxv. 9. "The Lord is able to give thee far more than "this." The apostle *Paul*, who had been traduced, reviled, buffetted, scourged, imprisoned, ship-wrecked, and stoned for his zeal to propagate the truths, whose study I plead for; after he had once had a glimpse of that great recompence of reward, that is reserved for us in heaven, scruples not to pronounce, that he finds upon casting up the account (for he uses the arithmetical λογίζομαι) Rom. viii. 18. "that the sufferings of this present time are "not worthy to be compared with the glory, that "is to be revealed in us." And if all, that the persecuted Christians of his time could suffer Luke xxiii. 15. were not suitable (for so I remember the same Greek word to signify elsewhere) or proportionable to that glory; it will sure far outweigh what we can now forego or decline for it; the loss of an advantage, and much more the bare missing of it, being usually but a negative affliction, in comparison of the actual sufferance of evil. Christ did not only tell his disciples, that he, who should give the least of his followers so much as a cup of cold water upon the score of their relation to him, should not be unrewarded; but when the same persons asked him, what should be done to them, who had left all to follow him, he presently allots them thrones, as much over-valuing that all they had lost, as an ordinary recompence may exceed a cup of cold water. And indeed God's goodness is so great, and his treasure so unexhausted, that as he is forward to recompence even the least services, that can be done him, so he is able to give the greatest a proportionable reward. *Solomon* had an opportunity, such as never any mortal had, (that we know of,) either before or since, of satisfying his desires, whether of fame, or any other 1 Kings iii. 5. thing, that he could wish; "Ask what I shall "give thee," was the proffer made him by him, that could give all things worth receiving; and yet the wisdom even of *Solomon*'s choice, approved by God himself, consisted in declining the most ambitious things of this life for those things, that might the better qualify him to serve and please God. And to give you an example in a greater than *Solomon*, we may Phil. ii. 6. consider, that he, "who being in the form of "God, thought it not robbery to be equal "with God;" and who, by leaving heaven, did, to dwell on earth, quit more than any inhabitant of the earth can gain in heaven, and denied more to become capable of being tempted, than he did when he was tempted with an offer of all the kingdoms of the world, and the glory of them: this Saviour, I say, is said in scripture to have, "for the joy, that was set be- Heb. xii. 2. "fore him, endured the cross, and despised the "shame;" as if heaven had been a sufficient recompence for even his renouncing honours, and embracing torments.

He, that declines the acquist of the applause of men for the contemplation of the truths of God, does but forbear to gather that, whilst it is immature, which, by waiting God's time, he will more seasonably gather when it is full ripe, and wholesome, and sweet. That immarcescible crown, as St. *Peter* calls it, which the Gospel promises to them, "who, by patient con- Rom. ii. 7. "tinuance in well-doing, seek for glory and "honour," will make a rich amends for the declining of a fading wreath here upon earth, where reputation is oftentimes as undeservedly acquired, as lost; whereas in heaven, the very having celestial honours argues a title to them. And since it is our Saviour's reasoning, that his disciples ought to rejoice when their reputation is pursued by calumny, as well as their lives by persecution, "because their reward is great in Matth. v. 11, 12. "heaven," we may justly infer, that the grounded expectation of so illustrious a condition may bring us more content, even when it is not attended with a present applause, than this applause can give those, who want that comfortable expectation. So that, upon the whole matter, we have no reason to despond, or to complain of the study of theology, for but making us decline an empty and transitory fame for a solid and eternal glory.

The CONCLUSION.

By this time, Sir, I have said as much as I think fit (and therefore, I hope, more than upon your single account was necessary) to manifest, that *Physeophilus* had no just cause to undervalue the study of divinity nor our friend the doctor, for addicting himself to it. I hope you have not forgotten what I expressly enough declared at the beginning of this letter, that both your friend and you admitting the holy scriptures, I know myself thereby to be warranted to draw proofs from their authority. And if I need not remind you of this, perhaps I need not tell you by way of apology, that I am not so unacquainted with the laws of discoursing, but that, if I had been to argue with Atheists or Scepticks, I should have forborn to make use of divers of the arguments I have employed, as fetched from unconceded topicks, and substituted others for such, as yet, I think, it very allowable for me to urge, when I deal with a person, that, as your friend, does only undervalue the study of the scriptures, nor reject their authority. And if the prolixity I have been guilty of already, did forbid me to encrease it by apologies not absolutely necessary, I should perchance, rather think myself obliged to excuse the plainness of the stile of this discourse; which both upon the subject's score, and yours, may seem to challenge a richer dress. But the matter is very serious, and you are a philosopher, and when the things we treat of are highly important, I think,

truths

truths clearly made out to be the moſt perſuaſive pieces of oratory. And a diſcourſe of this nature is more likely to prove effectual on intelligent peruſers, by having the reaſons it preſents perſpicuouſly propoſed, and unprejudicedly entertained, than by their being pathetically urged, or curiouſly adorned. And I have the rather forborn expreſſions, that might ſeem more proper to move, than to convince; becauſe I foreſee, I may very ſhortly have occaſion to employ ſome of the former ſort in another letter to a friend of yours and mine, who will, I doubt, make you a ſharer in the trouble of reading it. But writing this for you, and *Phyſeophilus*, I was far more ſolicitous to give the arguments I employ a good than a bright gloſs. For even when we would excite devotion, if it be in rational men, the moſt effectual pieces of oratory are thoſe, which like burning-glaſſes inflame, by nothing but numerous and united beams of light. If this letter prove ſo happy as to give you any ſatisfaction, it will thereby bring me a great one. For prizing you as I do, I cannot but wiſh to ſee you eſteem thoſe things now, which I am confident we ſhall always have cauſe to eſteem; and then moſt, when the light of glory ſhall have made us better judges of the true worth of things. And it would extreamly trouble me to ſee you a diſeſteemer of thoſe divine things, which as long as a man undervalues, the poſſeſſion of heaven itſelf would not make him happy. And therefore, if the bleſſing of him, whoſe glory is aimed at in it, make the ſucceſs of this paper anſwerable to the wiſhes, the importance of the ſubject will make the ſervice done you by it, ſuitable to the deſires of,

SIR,

Your moſt faithful,

moſt affectionate,

and moſt humble ſervant.

ABOUT

ABOUT THE

EXCELLENCY

AND

GROUNDS

OF THE

MECHANICAL HYPOTHESIS,

SOME CONSIDERATIONS,

Occaſionally propoſed to a FRIEND.

The Publisher's ADVERTISEMENT.

THE following paper having been but occaſionally and haſtily penned, long after what the author had written (by way of dialogue) about the requiſites of a good hypotheſis, it was intended, that if it came forth at all, it ſhould do ſo as an appendix to that diſcourſe; becauſe, though one part of it does little more than name ſome of the heads treated of in the dialogue, yet, according to the exigency of the occaſion, the other part contains ſeveral things either pretermitted, or but more lightly touched on in this diſcourſe. But, although the author's deſign were to reſerve theſe thoughts, as a kind of paralipomena to his dialogue; yet, ſince he is not willing to let that, at leaſt quickly, come abroad, and theſe are fallen into my hands; I will make bold, with his good leave, to annex them to the foregoing treatiſe, not only to compleat the bulk of the book, but becauſe of ſome affinity between them, ſince both aim at manifeſting the excellency of the ſtudies they would recommend. And perhaps it will not be unwelcome to ſome of the curious to find, that our noble author in the ſame book, wherein he prefers the ſtudy of divine things to that of natural ones, does himſelf prefer the mechanical principles before all other hypotheſes about natural things; they being in their own nature ſo accommodate, to make conſidering men underſtand, rather than diſpute of, the effects of nature.

OF THE EXCELLENCY and GROUNDS OF THE CORPUSCULAR or MECHANICAL PHILOSOPHY.

THE importance of the queſtion, you propoſe, would oblige me to refer you to "the dialogue about a good "hypotheſis," and ſome other papers of that kind, where you may find my thoughts about the advantages of the mechanical hypotheſis ſomewhat amply ſet down, and diſcourſed of. But, ſince your deſires confine me to deliver in few words, not what I believe reſolvedly, but what I think may be probably ſaid for the preference or the pre-eminence of the corpuſcular philoſophy above *Ariſtotle*'s, or that of the chemiſts, you muſt be content to receive from me, without any preamble, or exact method, or ample diſcourſes, or any other thing, that may coſt many words, a ſuccinct mention of ſome of the chief advantages of the hypotheſis we incline to. And I the rather comply, on this occaſion, with your curioſity, becauſe I have often obſerved you to be alarmed and diſquieted, when you hear of any book, that pretends to uphold, or repair the decaying philoſophy of the ſchools, or ſome bold chymiſt, that arrogates to thoſe of his ſect the title of philoſophers, and pretends to build wholly upon experience, to which he would have all other naturaliſts thought ſtrangers. That therefore you may not be ſo tempted to deſpond, by the confidence or reputation of thoſe writers, that do ſome of them applaud, and others cenſure, what, I fear, they do not underſtand, (as when the Peripateticks cry up ſubſtantial forms, and the chemiſts, mechanical explications) of nature's phænomena, I will propoſe ſome conſiderations, that, I hope, will not only keep you kind to the philoſophy you have embraced, but perhaps, (by ſome conſiderations, which you have not yet met with,) make you think it probable, that the new attempts you hear of from time to time, will not overthrow the corpuſcularian philoſophy, but either be foiled by it, or found reconcilable to it.

BUT when I ſpeak of the corpuſcular or mechanical philoſophy, I am far from meaning with the Epicureans, that atoms, meeting together by chance in an infinite vacuum, are able of themſelves to produce the world, and all its phænomena; nor with ſome modern philoſophers, that, ſuppoſing God to have put into the whole maſs of matter ſuch an invariable quantity of motion, he needed do no more to make the world, the material parts being able by their own unguided motions, to caſt themſelves into ſuch a ſyſtem (as we call by that name:) but I plead only for ſuch a philoſophy, as reaches but to things purely corporeal, and diſtinguiſhing between the firſt original of things, and the ſubſequent courſe of nature, teaches, concerning the former, not only that God gave motion to matter, but that in the beginning he ſo guided the various motions of the parts of it, as to contrive them into the world he deſigned they ſhould compoſe, (furniſhed with the ſeminal principles and ſtructures, or models of living creatures,) and eſtabliſhed thoſe rules of motion, and that order amongſt things corporeal, which we are wont to call the laws of nature. And having told this as to the former, it may be allowed as to the latter to teach, that the univerſe being once framed by God, and the laws of motion being ſettled and all upheld by his inceſſant concourſe and general providence, the phænomena of the world thus conſtituted are phyſically produced by the mechanical affections of the parts of matter, and what they operate upon one another according to mechanical laws. And now having ſhewn what kind of corpuſcular philoſophy it is, that I ſpeak of, I proceed to the particulars, that I thought the moſt proper to recommend it.

I. THE firſt thing, that I ſhall mention to this purpoſe, is the intelligibleneſs or clearneſs of mechanical principles and explications. I need not tell you, that among the Peripateticks, the diſputes are many and intricate about matter, privation, ſubſtantial forms, and their eduction, &c. And the chemiſts are ſufficiently puzzled, (as I have elſewhere ſhewn,) to give ſuch definitions and accounts of their hypoſtatical principles, as are reconcileable to one another, and even to ſome obvious phænomena. And much more dark and intricate are their doctrines about the Archeus, Aſtral Beings, Gas, Blaſs, and other odd notions, which perhaps have in part occaſioned the darkneſs and ambiguity of their expreſſions, that could not be very clear, when their conceptions were far from being ſo. And if the principles of the Ariſtotelians and Spagyriſts are thus obſcure, it is not to be expected, the explications, that are made by the help only of ſuch principles ſhould be clear. And indeed many of them are either ſo general and ſlight, or otherwiſe ſo unſatisfactory, that granting their principles, it is very hard to underſtand or admit their applications of them to particular phænomena. And even in ſome of the more ingenious and ſubtle of the peripatetick diſcourſes upon their ſuperficial and narrow theories, methinks, the authors have better plaid the part of painters than philoſophers, and have only had the

ſkill,

ſkill, like drawers of landſkips, to make men fancy they ſee caſtles and towns, and other ſtructures, that appear ſolid and magnificent, and to reach to a large extent, when the whole piece is ſuperficial, and made up of colours and art, and compriſed within a frame perhaps ſcarce a yard long. But to come now to the corpuſcular philoſophy, men do ſo eaſily underſtand one another's meaning, when they talk of local motion, reſt, bigneſs, ſhape, order, ſituation, and contexture of material ſubſtances; and theſe principles do afford ſuch clear accounts of thoſe things, that are rightly deduced from them only, that even thoſe Peripateticks or chymiſts, that maintain other principles, acquieſce in the explications made by theſe, when they can be had, and ſeek not any further, though perhaps the effect be ſo admirable, as would make it paſs for that of a hidden form, or occult quality. Thoſe very Ariſtotelians, that believe the celeſtial bodies to be moved by intelligences, have no recourſe to any peculiar agency of theirs to account for eclipſes. And we laugh at thoſe Eaſt-Indians, that to this day go out in multitudes, with ſome inſtruments, that may relieve the diſtreſſed luminary, whoſe loſs of light they fancy to proceed from ſome fainting fit, out of which it muſt be rouzed. For no intelligent man, whether chemiſt or Peripatetic, flies to his peculiar principles, after he is informed, that the moon is eclipſed by the interpoſition of the earth betwixt her and it, and the ſun by that of the moon betwixt him and the earth. And when we ſee the image of a man caſt into the air by a concave ſpherical looking-glaſs, though moſt men are amazed at it, and ſome ſuſpect it to be no leſs than an effect of witchcraft, yet he, that is ſkilled enough in catoptricks, will, without conſulting *Ariſtotle*, or *Paracelſus*, or flying to hypoſtatical principles and ſubſtantial forms, be ſatiſfied, that the phænomenon is produced by the beams of light reflected, and thereby made convergent according to optical, and conſequently mathematical laws.

BUT I muſt not now repeat what I elſewhere ſay, to ſhew, that the corpuſcular principles have been declined by philoſophers of different ſects, not becauſe they think not our explications clear, if not much more ſo, than their own; but becauſe they imagine, that the applications of them can be made but to few things, and conſequently are inſufficient.

II. IN the next place I obſerve, that there cannot be fewer principles than the two grand ones of mechanical philoſophy, matter and motion. For, matter alone, unleſs it be moved, is altogether unactive; and whilſt all the parts of the body continue in one ſtate without any motion at all, that body will not exerciſe any action, nor ſuffer any alteration itſelf, though it may perhaps modify the action of other bodies, that move againſt it.

III. NOR can we conceive any principles more primary, than matter and motion. For, either both of them were immediately created by God, or, (to add that for their ſakes, that would have matter to be unproduced,) if matter be eternal, motion muſt either be produced by ſome immaterial ſupernatural agent, or it muſt immediately flow by way of emanation from the nature of the matter it appertains to.

IV. NEITHER can there be any phyſical principles more ſimple than matter and motion; neither of them being reſoluble into any things, whereof it may be truly, or ſo much as tolerably ſaid to be compounded.

V. THE next thing I ſhall name to recommend the corpuſcular principle, is their great comprehenſiveneſs. I conſider then, that the genuine and neceſſary effect of the ſufficiently ſtrong motion of one part of matter againſt another, is, either to drive it on in its intire bulk, or elſe to break or divide it into particles of determinate motion, figure, ſize, poſture, reſt, order or texture. The two firſt of theſe, for inſtance, are each of them capable of numerous varieties. For the figure of a portion of matter may either be one of the five regular figures treated of by geometricians, or ſome determinate ſpecies of ſolid figures, as that of a cone, cylinder, &c. or irregular, though not perhaps anonymous, as the grains of ſand, hoops, feathers, branches, forks, files, &c. And as the figure, ſo the motion of one of theſe particles may be exceedingly diverſified, not only by the determination to this or that part of the world, but by ſeveral other things, as particularly by the almoſt infinitely varying degrees of celerity, by the manner of its progreſſion with, or without rotation, and other modifying circumſtances; and more yet, by the line, wherein it moves, as (beſides ſtreight) circular, elliptical, parabolical, hyperbolical, ſpiral, and I know not how many others. For as later geometricians have ſhewn, that thoſe crooked lines may be compounded of ſeveral motions, (that is, traced by a body, whoſe motion is mixed of, and reſults from, two or more ſimpler motions,) ſo how many more curves may, or rather may not be made by new compoſitions and decompoſitions of motion, is no eaſy taſk to determine.

Now, ſince a ſingle particle of matter, by virtue of two only of the mechanical affections, that belong to it, be diverſifiable ſo many ways; how vaſt a number of variations may we ſuppoſe capable of being produced by the compoſitions and decompoſitions of myriads of ſingle inviſible corpuſcles, that may be contained and contexed in one ſmall body, and each of them be embued with more than two or three of the fertile catholick ptinciples abovementioned? Eſpecially ſince the aggregate of thoſe corpuſcles may be farther diverſified by the texture reſulting from their convention into a body, which, as ſo made up, has its own bigneſs, and ſhape, and pores, (perhaps very many and various) and has alſo many capacities of acting and ſuffering upon the ſcore of the place it holds among other bodies in a world conſtituted as ours is: ſo that, when I conſider the almoſt innumerable diverſifications, that compoſitions and decompoſitions may make of a ſmall number, not perhaps exceeding twenty of diſtinct things, I am apt to look upon thoſe, who

who think the mechanical principles may ſerve indeed to give an account of the phænomena of this or that particular part of natural philoſophy, as ſtaticks, hydroſtaticks, the theory of the planetary motions, &c. but can never be applied to all the phænomena of things corporeal; I am apt, I ſay, to look upon thoſe, otherwiſe learned men, as I would do upon him, that ſhould affirm, that by putting together the letters of the alphabet, one may indeed make up all the words to be found in one book, as in *Euclid*, or *Virgil*; or in one language, as Latin, or Engliſh; but that they can by no means ſuffice to ſupply words to all the books of a great library, much leſs to all the languages in the world.

AND whereas there is another ſort of philoſophers, that, obſerving the great efficacy of the bigneſs, and ſhape, and ſituation, and motion, and connexion in engines, are willing to allow, that thoſe mechanical principles may have a great ſtroke in the operations of bodies of a ſenſible bulk, and manifeſt mechaniſm, and therefore may be uſefully employed in accounting for the effects and phænomena of ſuch bodies, who yet will not admit, that theſe principles can be applied to the hidden tranſactions, that paſs among the minute particles of bodies; and therefore think it neceſſary to refer theſe to what they call nature, ſubſtantial forms, real qualities, and the like unmechanical principles and agents.

BUT this is not neceſſary; for both the mechanical affections of matter are to be found, and the laws of motion take place, not only in the great maſſes, and the middle ſized lumps, but in the ſmalleſt fragments of matter; and a leſſer portion of it being as well a body as a greater, muſt, as neceſſarily as it, have its determinate bulk and figure: and he, that looks upon ſand in a good microſcope, will eaſily perceive, that each minute grain of it has as well its own ſize and ſhape, as a rock or mountain. And when we let fall a great ſtone and a pebble from the top of a high building, we find not, but that the latter as well as the former moves conformably to the laws of acceleration in heavy bodies deſcending. And the rules of motion are obſerved, not only in cannon bullets, but in ſmall ſhot; and the one ſtrikes down a bird according to the ſame laws, that the other batters down a wall. And though nature (or rather its divine author) be wont to work with much finer materials, and employ more curious contrivances than art, (whence the ſtructure even of the rareſt watch is incomparably inferior to that of a human body;) yet an artiſt himſelf, according to the quantity of the matter he employs, the exigency of the deſign he undertakes, and the bigneſs and ſhape of the inſtruments he makes uſe of, is able to make pieces of work of the ſame nature or kind of extremely differing bulk, where yet the like, though not equal art and contrivance, and oftentimes motion too, may be obſerved: as a ſmith, who with a hammer, and other large inſtruments, can, out of maſſes of iron, forge great bars or wedges, and make thoſe ſtrong and heavy chains, that were employed to load malefactors, and even to ſecure ſtreets and gates, may, with leſſer inſtruments, make ſmaller nails and filings, almoſt as minute as duſt; and may yet, with finer tools, make links of a ſtrange ſlenderneſs and lightneſs, inſo much, that good authors tell us of a chain of divers links, that was faſtened to a flea, and could be moved by it; and if I miſremember not, I ſaw ſomething like this, beſides other inſtances, that I beheld with pleaſure, of the littleneſs, that art can give to ſuch pieces of work, as are uſually made of a conſiderable bigneſs. And therefore to ſay, that though in natural bodies, whoſe bulk is manifeſt and their ſtructure viſible, the mechanical principles may be uſefully admitted, that are not to be extended to ſuch portions of matter, whoſe parts and texture are inviſible; may perhaps look to ſome, as if a man ſhould allow, that the laws of mechaniſm may take place in a town-clock, but cannot in a pocket-watch; or, (to give you an inſtance, mixed of natural and artificial,) as if, becauſe the terraqueous globe is a vaſt magnetical body of ſeven or eight thouſand miles in diameter, one ſhould affirm, that magnetical laws are not to be expected to be of force in a ſpherical piece of loadſtone, that is not perhaps an inch long: and yet experience ſhews us, that notwithſtanding the ineſtimable diſproportion betwixt theſe two globes, the terrella, as well as the earth, hath its poles, æquator, and meridians, and in divers other magnetical properties, emulates the terreſtrial globe.

THEY, that, to ſolve the phænomena of nature, have recourſe to agents, which, though they involve no ſelf-repugnancy in their very notions, as many of the judicious think ſubſtantial forms and real qualities to do, yet are ſuch, that we conceive not, how they operate to bring effects to paſs: theſe, I ſay, when they tell us of ſuch indeterminate agents, as the ſoul of the world, the univerſal ſpirit, the plaſtic power, and the like; though they may in certain caſes tell us ſome things, yet they tell us nothing, that will ſatisfy the curioſity of an inquiſitive perſon, who ſeeks not ſo much to know, what is the general agent, that produces a phænomenon, as, by what means, and after what manner, the phænomenon is produced. The famous *Sennertus*, and ſome other learned phyſicians, tell us of diſeaſes, which proceed from incantation; but ſure it is but a ſlight account, that a ſober phyſician, that comes to viſit a patient reported to be bewitched, receives of the ſtrange ſymptoms he meets with, and would have an account of, if he be coldly anſwered, that it is a witch, or the devil, that produces them; and he will never ſit down with ſo ſhort an account, if he can by any means reduce thoſe extravagant ſymptoms to any more known and ſtated diſeaſes, as epilepſies, convulſions, hyſterical fits, &c. and, if he cannot, he will confeſs his knowledge of this diſtemper to come far ſhort of what might be expected and attained in other diſeaſes, wherein he thinks himſelf bound to ſearch into the nature of the morbific matter, and will not be ſatisfied, till he can, probably at leaſt, deduce from that, and the ſtructure

ture of an human body, and other concurring phyſical cauſes, the phænomena of the malady. And it would be but little ſatisfaction to one, that deſires to underſtand the cauſes of what occurs to obſervation in a watch, and how it comes to point at, and ſtrike the hours, to be told, that it was ſuch a watch-maker that ſo contrived it; or to him, that would know the true cauſe of an eccho, to be anſwered, that it is a man, a vault, or a wood, that makes it.

AND now at length I come to conſider that, which I obſerve the moſt to alienate other ſects from the mechanical philoſophy; namely, that they think it pretends to have principles ſo univerſal and ſo mathematical, that no other phyſical hypotheſis can comport with it, or be tolerated by it.

BUT this I look upon as an eaſy, indeed, but an important miſtake; becauſe by this very thing, that the mechanical principles are ſo univerſal, and therefore applicable to ſo many things, they are rather fitted to include, than neceſſitated to exclude, any other hypotheſis, that is founded in nature, as far as it is ſo. And ſuch hypotheſes, if prudently conſidered by a ſkilful and moderate perſon, who is rather diſpoſed to unite ſects than multiply them, will be found, as far as they have truth in them, to be either legitimately (though perhaps not immediately) deducible from the mechanical principles, or fairly reconcilable to them. For, ſuch hypotheſes will probably attempt to account for the phænomena of nature, either by the help of a determinate number of material ingredients, ſuch as the *tria prima* of the chemiſts, by participation whereof other bodies obtain their qualities; or elſe by introducing ſome general agents, as the Platonic ſoul of the world, or the univerſal ſpirit, aſſerted by ſome ſpagyriſts; or by both theſe ways together.

Now, to diſpatch firſt thoſe, that I named in the ſecond place; I conſider, that the chief thing, that inquiſitive naturaliſts ſhould look after in the explicating of difficult phænomena, is not ſo much what the agent is or does, as, what changes are made in the patient, to bring it to exhibit the phænomena, that are propoſed; and by what means, and after what manner, thoſe changes are effected. So that the mechanical philoſopher being ſatisfied, that one part of matter can act upon another but by virtue of local motion, or the effects and conſequences of local motion, he conſiders, that as if the propoſed agent be not intelligible and phyſical, it can never phyſically explain the phænomena; ſo, if it be intelligible and phyſical, it will be reducible to matter, and ſome or other of thoſe only catholick affections of matter, already often mentioned. And the indefinite diviſibility of matter, the wonderful efficacy of motion, and the almoſt infinite variety of coalitions and ſtructures, that may be made of minute and inſenſible corpuſcles, being duly weighed, I ſee not, why a philoſopher ſhould think it impoſſible, to make out, by their help, the mechanical poſſibility of any corporeal agent, how ſubtil, or diffuſed, or active ſoever it be, that can be ſolidly proved to be really exiſtent in nature, by what name ſoever it be called or diſguiſed. And though the Carteſians be mechanical philoſophers, yet, according to them, their *Materia Subtilis*, which the very name declares to be a corporeal ſubſtance, is, for aught I know, little (if it be at all) leſs diffuſed through the univerſe, or leſs active in it than the univerſal ſpirit of ſome ſpagyriſts, not to ſay, the *Anima Mundi* of the Platoniſts. But this upon the by; after which I proceed, and ſhall venture to add, that whatever be the phyſical agent, whether it be inanimate or living, purely corporeal, or united to an intellectual ſubſtance, the above-mentioned changes, that are wrought in the body, that is made to exhibit the phænomena, may be effected by the ſame or the like means, or after the ſame or the like manner; as for inſtance, if corn be reduced to meal, the materials and ſhape of the milſtones, and their peculiar motion and adaptation, will be much of the ſame kind; and (though they ſhould not, yet) to be ſure the grains of corn will ſuffer a various contrition and comminution in their paſſage to the form of meal; whether the corn be ground by a water-mill, or a wind-mill, or a horſe-mill, or a hand-mill; that is, by a mill, whoſe ſtones are turned by inanimate, by brute, or by rational agents. And, if an angel himſelf ſhould work a real change in the nature of a body, it is ſcarce conceivable to us men, how he could do it without the aſſiſtance of local motion; ſince, if nothing were diſplaced, or otherwiſe moved than before, (the like happening alſo to all external bodies to which it related,) it is hardly conceivable, how it ſhould be in itſelf other, than juſt what it was before.

BUT to come now to the other ſort of hypotheſis formerly mentioned; if the chemiſts, or others, that would deduce a compleat natural philoſophy from ſalt, ſulphur, and mercury, or any other ſet number of ingredients of things, would well conſider, what they undertake, they might eaſily diſcover, that the material parts of bodies, as ſuch, can reach but to a ſmall part of the phænomena of nature, whilſt theſe ingredients are conſidered but as quieſcent things, and therefore they would find themſelves neceſſitated to ſuppoſe them to be active; and that things purely corporeal cannot be but by means of local motion, and the effects, that may reſult from that, accompanying variouſly ſhaped, ſized, and aggregated parts of matter: ſo that the chemiſt and other materialiſts, if I may ſo call them, muſt (as indeed they are wont to do) leave the greateſt part of the phænomena of the univerſe unexplicated by the help of the ingredients (be they fewer or more than three) of bodies, without taking in the mechanical, and more comprehenſive affections of matter, eſpecially local motion. I willingly grant, that ſalt, ſulphur, and mercury, or ſome ſubſtances analogous to them, are to be obtained by the action of the fire, from a very great many diſſipable bodies here below; nor would I deny, that in explicating divers of the phænomena of ſuch bodies, it may be of uſe to a ſkilful naturaliſt to know and conſider, that this or that ingredient, as ſulphur, for inſtance, does abound in the body

body propofed, whence it may be probably argued, that the qualities, that ufually accompany that principle, when predominant, may be alfo, upon its fcore, found in the body, that fo plentifully partakes of it. But not to mention, what I have elfewhere fhewn, that there are many phænomena, to whofe explication this knowledge will contribute very little or nothing at all; I fhall only here obferve, that, though chemical explications be fometimes the moft obvious and ready, yet they are not the moft fundamental and fatisfactory: for, the chemical ingredient itfelf, whether fulphur or any other, muft owe its nature and other qualities to the union of infenfible particles in a convenient fize, fhape, motion or reft, and contexture; all which are but mechanical affections of convening corpufcles. And this may be illuftrated by what happens in artificial fire-works. For, though in moft of thofe many differing forts that are made, either for the ufe of war, or for recreation, gunpowder be a main ingredient, and divers of the phænomena may be derived from the greater or leffer meafure, wherein the compofitions partake of it; yet, befides that there may be fire-works made without gun-powder, (as appears by thofe made of old by the Greeks and Romans,) gunpowder itfelf owes its aptnefs to be fired and exploded to the mechanical contexture of more fimple portions of matter, nitre, charcoal, and fulphur; and fulphur itfelf, though it be by many chemifts miftaken for an hypoftatical principle, owes its inflammability to the convention of yet more fimple and primary corpufcles; fince chemifts confefs, that it has an inflammable ingredient, and experience fhews, that it very much abounds with an acid and uninflammable falt, and is not quite devoid of terreftreity. I know it may be here alledged, that the productions of chemical analyfes are fimple bodies, and upon that account irrefoluble. But, that divers fubftances, which chemifts are pleafed to call the falts, or fulphurs, or mercuries of the bodies, that afforded them, are not fimple and homogeneous, has elfewhere been fufficiently proved; nor is their not being eafily diffipable, or refoluble, a clear proof of their not being made up of more primitive portions of matter. For, compounded, and even decompounded bodies, may be as difficultly refoluble, as moft of thofe, that chemifts obtain by what they call their analyfis by the fire; witnefs common green glafs, which is far more durable and irrefoluble than many of thofe, that pafs for hypoftatical fubftances. And we fee, that fome amels will be feveral times even vitrified in the fire, without lofing their nature, or oftentimes fo much as their colour; and yet amel is manifeftly, not only a compounded, but a decompounded body, confifting of falt and powder of pebbles or fand, and calcined tin, and, if the amel be not white, ufually of fome tinging metal or mineral. But how indeftructible foever the chemical principles be fuppofed, divers of the operations afcribed to them will never be well made out, without the help of local motion, (and that diverfified too;) without which, we can little better give an account of the phænomena of many bodies, by knowing what ingredients compofe them, than we can explain the operations of a watch, by knowing of how many, and of what metals the balance, the wheels, the chain, and other parts are made; or than we can derive the operations of a wind-mill from the bare knowledge, that it is made up of wood, and ftone, and canvas, and iron. And here let me add, that it would not at all overthrow the Corpufcularian hypothefis, though either by more exquifite purifications, or by fome other operations, than the ufual analyfis of the fire, it fhould be made appear, that the material principles, or elements of mixed bodies, fhould not be the *tria prima* of the vulgar chemifts, but either fubftances of another nature, or elfe fewer, or more in number; as would be, if that were true, which fome fpagyrifts affirm, (but I could never find,) that from all forts of mixed bodies, five, and but five, differing fimilar fubftances can be feparated: or, as if it were true, that the Helmontians had fuch a refolving menftruum as the Alkaheft of their mafter; by which he affirms, that he could reduce ftones into falt of the fame weight with the mineral, and bring both that falt, and all other kind of mixed and tangible bodies, into infipid water. For, whatever be the number or qualities of the chemical principles, if they be really exiftent in nature, it may very poffibly be fhewn, that they may be made up of infenfible corpufcles of determinate bulks and fhapes; and by the various coalitions and contextures of fuch corpufcles, not only three or five, but many more material ingredients, may be compofed or made to refult. But, though the Alkaheftical reductions newly mentioned fhould be admitted, yet the mechanical principles might well be accommodated even to them. For the folidity, tafte, &c. of falt, may be fairly accounted for, by the ftiffnefs, fharpnefs, and other mechanical affections of the minute particles, whereof falts confift; and if, by a farther action of the alkaheft, the falt, or any other folid body, be reduced into infipid water, this alfo may be explicated by the fame principles, fuppofing a farther comminution of the parts, and fuch an attrition, as wears off the edges and points, that enabled them to ftrike brifkly the organ of tafte: for, as to fluidity and firmnefs, thofe mainly depend upon two of our grand principles, motion and reft. And I have elfewhere fhewn, by feveral proofs, that the agitation of reft, and the loofer contact, or clofer cohæfion, of the particles, is able to make the fame portion of matter, at one time a firm, and at another time a fluid body. So that, though the further fagacity and induftry of chemifts (which I would by no means difcourage) fhould be able to obtain from mixed bodies homogeneous fubftances, differing in number, or nature, or both, from their vulgar falt, fulphur, and mercury; yet the corpufcular philofophy is fo general and fertile, as to be fairly reconcilable to fuch a difcovery; and alfo fo ufeful, that thefe new material principles

ples will, as well as the old *tria prima*, ſtand in need of the more catholick principles of the Corpuſcularians, eſpecially local motion. And indeed, whatever elements or ingredients men have (that I know of) pitched upon, yet if they take not in the mechanical affections of matter, their principles have been ſo deficient, that I have uſually obſerved, that the materialiſts, without at all excepting the chemiſts, do not only, as I was ſaying, leave many things unexplained, to which their narrow principles will not extend; but, even in the particulars, they preſume to give an account of, they either content themſelves to aſſign ſuch common and indefinite cauſes, as are too general to ſignify much towards an inquiſitive man's ſatiſfaction; or if they venture to give particular cauſes, they aſſign precarious or falſe ones, and liable to be eaſily diſproved by circumſtances, or inſtances, whereto their doctrine will not agree, as I have often elſewhere had occaſion to ſhew. And yet the chemiſts need not be frighted from acknowledging the prerogative of the mechanical philoſophy, ſince that may be reconcileable with the truth of their own principles, as far as theſe agree with the phænomena they are applied to. For theſe more confined hypotheſes may be ſubordinated to thoſe more general and fertile principles, and there can be no incredient aſſigned, that has a real exiſtence in nature, that may not be derived either immediately, or by a row of decompoſitions, from the univerſal matter, modified by its mechanical affections. For if, with the ſame bricks, diverſly put together and ranged, ſeveral walls, houſes, furnaces, and other ſtructures, as vaults, bridges, pyramids, &c. may be built, merely by a various contrivement of parts of the ſame kind; how much more may great variety of ingredients be produced by, or, according to the inſtitution of nature, reſult from the various coalitions and contextures of corpuſcles, that need not be ſuppoſed, like bricks, all of the ſame, or near the ſame ſize and ſhape, but may have amongſt them, both of the one and the other, as great a variety as need be wiſhed for, and indeed a greater than can eaſily be ſo much as imagined? And the primary and minute concretions, that belong to theſe ingredients, may, without oppoſition from the mechanical philoſophy, be ſuppoſed to have their particles ſo minute and ſtrongly coherent, that nature of herſelf does ſcarce ever tear them aſunder; as we ſee, that mercury and gold may be ſucceſſively made to put on a multitude of diſguiſes, and yet ſo retain their nature, as to be reducible to their priſtine forms. And you know, I lately told you, that common glaſs and good amels, though both of them but factitious bodies, and not only mixed, but decompounded concretions, have yet their component parts ſo ſtrictly united by the ſkill of illiterate tradeſmen, as to maintain their union in the vitrifying violence of the fire. Nor do we find, that common glaſs will be wrought upon by aqua fortis, or aqua regis, though the former of them will diſſolve mercury, and the latter gold.

FROM the foregoing diſcourſe it may (probably at leaſt) reſult, that if, beſides rational ſouls, there are any immaterial ſubſtances (ſuch as the heavenly intelligences, and the ſubſtantial forms of the Ariſtotelians,) that regularly are to be numbered among natural agents, their way of working being unknown to us, they can but help to conſtitute and effect things, but will very little help us to conceive how things are effected; ſo that by whatever principles natural things be conſtituted, it is by the mechanical principles, that their phænomena muſt be clearly explicated. As for inſtance, though we ſhould grant the Ariſtotelians, that the planets are made of a quinteſſential matter, and moved by angels, or immaterial intelligences; yet, to explain the ſtations, progreſſions, and retrogradations, and other phænomena of the planets, we muſt have recourſe either to eccentricks, epicycles, &c. or to motions made in elliptical or other peculiar lines; and, in a word, to theories, wherein the motion and figure, ſituation, and other mathematical or mechanical affections of bodies are mainly employed. But if the principles propoſed be corporeal things, they will be then fairly reducible, or reconcilable, to the mechanical principles; theſe being ſo general and pregnant, that among things corporeal, there is nothing real, (and I meddle not with chimerical beings, ſuch as ſome of *Paracelſus*'s,) that may not be derived from, or be brought to, a ſubordination to ſuch comprehenſive principles. And when the chemiſts ſhall ſhew, that mixed bodies owe their qualities to the predominancy of this or that of their three grand ingredients, the Corpuſcularians will ſhew, that the very qualities of this, or that ingredient, flow from its peculiar texture, and the mechanical affections of the corpuſcles it is made up of. And to affirm, that, becauſe the furnaces of chemiſts afford a great number of uncommon productions and phænomena, there are bodies or operations amongſt things purely corporeal, that cannot be derived from, or reconciled to, the comprehenſive and pregnant principles of the mechanical philoſophy, is, as if, becauſe there are a great number and variety of anthems, hymns, pavins, threnodies, courants, gavots, branles, ſarabands, jigs, and other (grave and ſprightly) tunes to be met with in the books and practiſes of muſicians, one ſhould maintain, that there are in them a great many tunes, or at leaſt, notes, that have no dependence on the ſcale of muſick; or, as if, becauſe, beſides rhombuſſes, rhomboids, trapeziums, ſquares, pentagons, chiliagons, myriagons, and innumerable other polygons, regular, and irregular, one ſhould preſume to affirm, that there are among them ſome rectilinear figures, that are not reducible to triangles, or have affections, that will overthrow what *Euclid* has taught of triangles and polygons.

TO what has been ſaid I ſhall add but one thing more; that as, according to what I formerly

formerly intimated, mechanical principles and explications are for their clearneſs preferred, even by materialiſts themſelves, to others, in the caſes where they can be had; ſo, the ſagacity and induſtry of modern naturaliſts and mathematicians having happily applied them to ſeveral of thoſe difficult phænomena, (in hydoſtaticks, the practical part of opticks, gunnery, &c.) that before were, or might be referred to occult qualities; it is probable, that when this philoſophy is deeplier ſearched into, and farther improved, it will be found applicable to the ſolution of more and more of the phænomena of nature. And on this occaſion let me obſerve, that it is not always neceſſary, though it be always deſirable, that he, that propounds an hypotheſis in aſtronomy, chemiſtry, anatomy, or other part of phyſicks, be able *à priori*, to prove his hypotheſis to be true, or demonſtratively to ſhew, that the other hypotheſes propoſed about the ſame ſubject muſt be falſe. For as, if I miſtake not, *Plato* ſaid, that the world was God's epiſtle written to mankind, and might have added, conſonantly to another ſaying of his, it was written in mathematical letters: ſo, in the phyſical explications of the parts and ſyſtem of the world, methinks, there is ſomewhat like what happens, when men conjecturally frame ſeveral keys to enable us to underſtand a letter written in cyphers. For though one man by his ſagacity have found out the right key, it will be very difficult for him, either to prove otherwiſe than by trial, that this or that word is not ſuch, as it is gueſſed to be by others, according to their keys; or to evince, *à priori*, that their's are to be rejected, and his to be preferred; yet, if due trial being made, the key he propoſes, ſhall be found ſo agreeable to the characters of the letter, as to enable one to underſtand them, and make a coherent ſenſe of them, its ſuitableneſs to what it ſhould decypher, is, without either confutations, or extraneous poſitive proofs, ſufficient to make it be accepted as the right key of that cypher. And ſo, in phyſical hypotheſes, there are ſome, that, without noiſe, or falling foul upon others, peaceably obtain diſcerning men's approbation only by their fitneſs to ſolve the phænomena, for which they were deviſed, without croſſing any known obſervation or law of nature. And therefore, if the mechanical philoſophy go on to explicate things corporeal at the rate it has of late years proceeded at, it is ſcarce to be doubted, but that, in time, unprejudiced perſons will think it ſufficiently recommended by its conſiſtency with itſelf, and its applicableneſs to ſo many phenomena of nature.

Ὁ Θεὸς ἀεὶ γεωμετρεῖ.

A RECAPITULATION.

PERCEIVING, upon a review of the foregoing paper, that the difficulty and importance of the ſubject, has ſeduced me to ſpend many more words about it, than I at firſt deſigned; it will not now be amiſs to give you this ſhort ſummary of what came into my mind, to recommend to you the mechanical philoſophy, and obviate your fears of ſeeing it ſupplanted; having firſt premiſed once for all, that preſuppoſing the creation and general providence of God, I pretend to treat but of things corporeal, and do abſtract in this paper from immaterial Beings, (which otherwiſe I very willingly admit,) and all agents and operations miraculous or ſupernatural.

I. OF the principles of things corporeal, none can be more few, without being inſufficient, or more primary, than matter and motion.

II. THE natural and genuine effect of variouſly determined motion in portions of matter is, to divide it into parts of differing ſizes, and ſhapes, and to put them into different motions; and the conſequences, that flow from theſe, in a world framed as ours is, are, as to the ſeparate fragments, poſture, order, and ſituation, and, as to the conventions of many of them, peculiar compoſitions and contextures.

III. THE parts of matter endowed with theſe catholick affections are, by various aſſociations, reduced to natural bodies of ſeveral kinds, according to the plenty of the matter, and the various compoſitions and decompoſitions of the principles; which all ſuppoſe the common matter they diverſify: and theſe ſeveral kinds of bodies, by virtue of their motion, reſt, and other mechanical affections, which fit them to act on, and ſuffer from one another, become endowed with ſeveral kinds of qualities, (whereof ſome are called manifeſt, and ſome occult,) and thoſe, that act upon the peculiarly framed organs of ſenſe, whoſe perceptions, by the animadverſive faculty of the ſoul, are ſenſations.

IV. THESE principles, matter, motion, (to which reſt is related) bigneſs, ſhape, poſture, order, texture, being ſo ſimple, clear, and comprehenſive, are applicable to all the real phænomena of nature, which ſeem not explicable by any other not conſiſtent with ours. For, if recourſe be had to an immaterial principle or agent, it may be ſuch an one, as is not intelligible; and however it will not enable us to explain the phænomena, becauſe its way of working upon things material, would probably be more difficult to be phyſically made out, than a mechanical account of the phænomena. And, notwithſtanding the immateriality of a created agent, we cannot conceive, how it ſhould produce changes in a body, without the help of mechanical principles, eſpecially local motion; and accordingly we find not, that the reaſonable ſoul in man is able to produce what changes it pleaſes in the body, but is confined to ſuch, as it may produce by determining, or guiding the motions of the ſpirits, and other parts of the body, ſubſervient to voluntary motion.

V. AND if the agents, or active principles reſorted to, be not immaterial, but of a corporeal nature, they muſt either in effect be the ſame with the corporeal principles above-named; or, becauſe of the great univerſality and ſimplicity of ours, the new ones propoſed, muſt be leſs general than they, and conſequently capable of being ſubordinate, or reduced to ours, which by various compoſitions may afford matter to ſeveral hypotheſes, and by ſeveral coalitions afford minute concretions exceedingly numerous and durable, and conſequently fit to become the elementary ingredients of more compounded bodies, being in moſt trials ſimilar, and as it were the radical parts, which may, after ſeveral manners, be diverſified; as in Latin, the themes are by prepoſitions, terminations, &c. and in Hebrew, the roots by the hæemantic letters. So that the fear, that ſo much of a new phyſical hypotheſis, as is true, will overthrow, or make uſeleſs the mechanical principles, is, as if one ſhould fear, that there will be a language propoſed, that is diſcordant from, or not reducible to, the letters of the alphabet.

TRACTS.

CONTAINING

I. SUSPICIONS about ſome HIDDEN QUALITIES of the AIR; with an APPENDIX touching CELESTIAL MAGNETS, and ſome other PARTICULARS.

II. ANIMADVERSIONS upon Mr. HOBBES's PROBLEMATA DE VACUO.

III. A DISCOURSE of the CAUSE of ATTRACTION by SUCTION.

PREFACE.

AMONG other papers, that I deſigned to contribute towards the natural hiſtory of the air, I began ſome years ago to ſet down a collection of ſome new or leſs heeded obſervations and experiments relating to the cauſes and effects of changes in the air, which I referred to ſeveral heads, as to the air's heat, coldneſs, moiſture, dryneſs, diaphaneity, opacity, conſiſtence, ſeveral ſaltneſſes and other titles; the laſt of which was of the occult qualities of the air, ſuppoſing there be any ſuch. And though afterwards I was, by ſickneſs and other impediments, diverted from proceeding in that collection, and induced to lay aſide ſome of the obſervations I had provided, and employ in other treatiſes, ſuch as were proper to them; yet as to the title, that contained ſuſpicions about ſome hidden qualities of the air, the poſſibility, if not likelihood, that either the matters of fact, or the intimations delivered in them, might afford hints not uſeleſs to the ſagacious and inquiſitive, perſuaded me to let it eſcape the fate of its companions, though poſſibly, if I had more conſulted my own reputation, I ſhould leaſt of all have ſuffered this title to appear, there being none of the reſt, that was not leſs conjectural. But it being thought unfit, that any thing ſhould periſh, that related to ſo conſiderable and uncommon a ſubject, as that of this title, I was content to caſt the collected experiments into the following eſſay, for the reaſons expreſſed at the beginning and cloſe of the enſuing paper. Which, it was hoped, may be the better underſtood, and leſs liable to have its deſign miſtaken, by being uſhered in by this advertiſement about the occaſion of it.

OBSER-

OBSERVATIONS ABOUT THE GROWTH OF METALS in their ORE, Exposed to the AIR.

IT is altogether unnecessary to my present purpose, to examine, whether metals and minerals, as if they were a kind of subterraneal plants, do properly grow as vegetables do. For this enquiry belongs to another place, but not to this, where the reference made in the 468th page of the following paper does not oblige me to speak of the growth of metals in any other than a lax and popular sense, in which a metal may be said to grow, if a portion of matter being assigned, wherein as yet men can find either no metal, as gold or tin, or but such a quantity of it; this being exposed to the air, will after a time either afford some metal, where there appeared none before, or a greater proportion of metal than it had before.

OBSERVATIONS of this kind requiring length of time, as well as residence near places abounding with minerals, I have little or no opportunity to make any of them myself, at least with the wariness, that to me seems due to observations, that I think not easy to be well made. And therefore I must content myself to set down what I have been able to learn by conversing with mineralists and travellers, and to add some particulars, that I met with in authors of good credit.

OBSERVATIONS about the GROWTH of TIN.

AN ancient owner of mines, being asked by me, whether he could, otherwise than upon the conjectures of vulgar tradition, prove, that minerals grow, even after the veins have been dug? answered affirmatively; and being desired to let me know his proofs, he gave me these that follow.

HE told me, that not far from his house there was a tin-mine, which the old diggers affirmed to have been left off, some said eighty, some an hundred and twenty years ago, because they had by their washing and vanning separated all the ore from the rest of the earth, and yet of late years they found it so richly impregnated with metalline particles, that it was wrought over again with very good profit, and preferred to some other mines, that were actually wrought, and had never been so robbed. And when I objected, that probably this might proceed from the laziness and unskilfulness of workmen in those times, who left in the earth the tin, that was lately separated, and might then have been so; I was answered, that it was a known thing in the country, that in those times the mine-men were more careful and laborious, to separate the metalline part from the rest of the ore, than now they are.

HE also affirmed to me, that in his own time some tenants and neighbours of his (employed by him) having got all the ore they could out of a great quantity of stuff, dug out of a tin-mine, they laid the remains in great heaps, exposed to the air, and within twenty and thirty years after, found them so richly impregnated, that they wrought them over again with good benefit.

AND lastly, he assured me, that in a work of his own, wherein he had exercised his skill and experience, (which is said to be very great) to separate all the particles of the tin from the terrestrial substances, that were dug up with it out of the vein, he caused dams to be made to stop the earthy substance, which the stream washed away from the ore, giving passage to the water; after it had let fall this substance, which lying in heaps exposed to the air, within ten or twelve years, and sometimes much less, he examined this, or that heap, and found it to contain such store of metalline particles,

particles, as invited him to work it again, and do it with profit. And yet this gentleman was so dextrous at separating the metalline from the other parts of tin-ore, that I could (not without wonder) see, what small corpuscles he would, to satisfy my curiosity, sever from vast quantities (in proportion) of earthy and other mineral stuff.

RELATIONS agreeable to these I received from another very ingenious gentleman, that was conversant with tin-mines, and lived not far from more than one of them.

I was the more solicitous to procure an information about the growth of this metal, because the bulk of that, which is used in *Europe*, being found in *England*, I have met with little or no mention of the growth of it in outlandish writers.

OBSERVATIONS about the GROWTH of LEAD.

AS for the growth of lead in the ore exposed to the air, I remember, I enquired about it of a person of quality, who had a patent for divers leaden mines, that were supposed to contain silver, and wrought some of them himself at no small charge, yet not without profit; and, as I remember, he answered me, that the lead-ore, that had been wrought and laid in heaps, did, in tract of time, grow impregnated with metal again, and, as experience manifested, became worth working a second time. And indeed some mineralists deliver it as a general observation, that the growth and renascence of metals is more manifest in lead than in any other of them. *Fessularum mons in Hetruria*, says *Boccatius Certardus*, who delivers it as a most approved truth, *Florentiæ Civitati imminens, lapides plumbarios habet, qui, si excidantur, brevi temporis spatio novis incrementis instaurantur.* J. Gerhard. *in decade quæstionum, pag. m.* 22.

Tu subtilius ne quæras (says *Agricola*, speaking of the growth of mines in general) *sed tantummodo refer animum ad cuniculos, & considera, eos adeò interdum memoriâ hominum in angustum venisse, ut aliqua sui parte nullum aut admodum difficilem præbeant transitum, cùm eos satis latè agere soleant fossores, ne transituros impediant. In tales autem angustias sunt adducti propter accretionem materiæ, ex qua lapis est factus.*

BUT whether this increment of lead is observable in all mines of that metal, I was induced to doubt by the answer given me by a gentleman, whose house was seated near several lead-mines, and who was himself owner of one or two, which he yet causes to be wrought: for this gentleman, though a chemist, assured me, that in the country, where he lives, which is divided by the sea from that of the person above-mentioned, he never observed the lead-ore to encrease, either out of the veins or in them; but that in some places, whence ore had been dug thirty or forty, if not fifty years before, he perceived not on the sides of the passages, whence the ore had been dug, that any other had grown in its place, or that the passages, though narrow before, were sensibly straightened, much less blocked up.

AND indeed, if there were no other arguments in the case, the straightning of the ancient passages in process of time would not convince me. For, when I consider, that the soils, that abound with metals, do usually also abound with waters, which are commonly imbibed by the neighbouring earth; and when I consider too, that water is somewhat expanded by being turned into ice, and that this expansion is made, (as I have often tried) though slowly, yet with an exceeding great force, by which it often stretches or breaks the vessels that contain it: when I consider these things, I say, I am apt to supect, that sometimes the encreasing narrowness of the subterraneal passages in mines may proceed from this, that the soil, that invirons them, if they lie not deep, may have the water, imbibed by them, frozen in sharp winters. By which glaciation, the moistened portion of the soil must forcibly endeavour to expand itself, and actually do so in the parts contiguous to the passage, since there it finds no resistance: and though the expansion made in one year or two be but small, and therefore not observed; yet, in a succession of many winters, it may by degrees grow to be very considerable. But this suspicion I suggest not, that I would deny the growth of minerals, but to recommend this argument for it to further consideration. And yet I take this to be a better proof, than what is much relied on by some writers of metals, who urge, that in churches, and other magnificent buildings, that are leaded over, the metalline roofs, in a long tract of years, grow far more ponderous, insomuch, that oftentimes there is a necessity to remove them, and exchange them for brass ones. For though this plausible argument be urged by several writers, and among them by the learned *Jo. Gerhardus, pag. m.* 22; yet I fear they proceed upon a mistake. For having had some occasion to observe and enquire after this kind of lead, I soon suspected, that the increment of weight, (which sometimes may indeed be very great) was no clear proof of the real growth of the metal itself. For in that, which I had occasion to consider, the additional weight, as well as bulk, seemed to proceed from acetous or other saline corpuscles of the timber of those buildings, which by degrees exhaling and corroding, that side of the lead which they fastened on, turned it with themselves into a kind of Ceruse: which suspicion I shall briefly make probably by noting, 1. That I have found by trial purposely made, that woods afford an acid, though not merely acid, liquor, capable of corroding lead. 2. That it is known, that lead turned into Ceruse increases notably

notably in weight, some say, (for I had not opportunity to try it) above six or seven in the hundred. 3. That from the sheets of lead, that have very long covered churches, and the like buildings, there is often obtained by scraping a good proportion of white lead, which I have known much preferred, by an eminent artist, to common ceruse, when a white pigment was to be employed. And, by the way, men's finding this ceruse not on that side of the lead, that is exposed to the outward air, (where I scarce ever observed any) but on the inside, that regards the timber and other wooden work, may disabuse those, that fancied this ceruse to be a part of the lead calcined by the beams of the sun, that strike immediately upon the metal. And if to this it be added, that by distillation and otherwise I have found cause to suspect, that alabaster and white marble may emit spirituous parts, that will invade lead, it may be doubted, whether what *Galen* relates of the great intumescence of leaden bands or fastenings, wherewith the feet of statues were fastened to their pedestals, be a sure argument of the real growth of that metal in the air.

BUT I begin to digress, and seemingly to the prejudice of the particular scope of this paper; but yet not to that of one of the main scopes of all my physical writings, the disquisition and advancement of truth.

OBSERVATIONS about the GROWTH of IRON.

I Did not find in one of our chief mines of iron, that there was any notice taken of the growth of that metal; but in another place or two, some, that deal in iron-ore, informed me, that they believe it grows, and may be regenerated; and upon that account one of them set up a work, contiguous to some land of mine, to melt over again the remainder of ore, that had been already wrought (at a great distance from that place) and had for some ages lain in heaps exposed to the free air; but with what success this chargeable attempt has been made, I am not yet informed.

BUT of the growth of iron in the island of *Ilva* or *Elva*, in the Tyrrhene sea, not far from the coast of *Tuscany*, not only ancient authors, as *Pliny* and *Strabo*, take special notice, but modern mineralists of very good credit, as *Falopius* and *Cæsalpinus*, particularly attest the same thing; of whom the latter speaks

Lib. III. cap. 6.

thus: *Vena ferri copiosissima est in Italia, ob eam nobilitata, Ilva, Tyrreni maris insula, incredibili copiâ etiam nostris temporibus eam gignens: nam terra, quæ eruitur, dum vena effoditur, tota procedente tempore in venam convertitur.*

AND the experienced *Agricola* gives us the like account of a place in his country, *Germany*, *In Lygiis*, says he, *ad Sagam oppidum in pratis eruitur ferrum, fossis ad altitudinem bipedaneam actis. Id decennio renatum denuò foditur, non aliter ac Ilvæ ferrum.*

Agric. de Vet. & Nov. Met. lib. II. cap. 15.

THE learned *Johan. Gerhardus*, out of a book, which he calls *Conciones Metallicæ*; I suppose he means the High Dutch Sermons of *Mathesius*, (whose language I understand not) has this notable passage to our present purpose: *Relatum mihi est à metallico fossore, ad ferrarias, quæ non longè Ambergâ distant, terram inanem cum ferri minera erutam, quam vocant* den Gummer, *mixtam cum recrementis ferri, quæ appellatur* der Sinder, *congestam in cumulos, instar magni cujusdam valli, solibus pluviisque exponi, & decimo quinto anno denuò excoqui, eliquarique ferrum tantæ tenacitatis, ut solæ laminæ inde procudantur.*

J. Gerhard. Professor Tubingensis, Decad. Quæst. Physico-chymicarum, pag. m. 18.

OBSERVATIONS about the GROWTH of SILVER.

OF the growth, as is supposed, of silver in the form of trees or grass or other vegetables, I have met with some instances among mineralists, and I have elsewhere mentioned, that an acquaintance of mine shewed me a stone, wherein he affirmed the silver, I saw in it, to have increased since he had it. But for certain reasons, none of these relations seem to me very proper to my present purpose; in order to which, I shall therefore set down only one instance, which I lately met with in a French collection of voyages, published by a person of great curiosity and industry, from whose civility I received the book. For there, in an account given by a gentleman of his country of a late voyage he made to *Peru*, wherein he visited the famous silver-mines of *Potosi*, I found a passage, which speaks to this sense: *Le meilleur argent, &c.* i. e. The best silver in all the *Indies*, and the purest, is that of the mines of *Potosi*; the chief have been found in the mountain of *Aranzasse*: and (some lines being interposed) it is added, that they draw this metal even from the mineral earths, that were in times past thrown aside, when the ground was open, and the grooves and shafts, that are in the mountains, were made; it having been observed, that in these recrements, metal had been formed afresh since those times, which sufficiently shews the propensity of the soil to the production of this metal; yet it is true, that these impregnated earths yield not so much as the ordinary ore, which is found in veins betwixt the rocks.

Voyage du Sieur au Peru, pag. 15.

OBSERVATIONS about the GROWTH of GOLD.

As for the growth of gold, the enquiries I have yet made among travellers give me no great satisfaction about it, and though I have spoken with several, that have been at the coast of *Guinea*, and in *Congo*, and other parts of *Afric*, where much gold is to be had; yet I could not learn by them, that they, or any acquaintance of theirs among the natives, had seen any mines or veins of gold, (which yet divers authors affirm to be found in more than one kingdom of *Æthiopia*, and in some other African Countries.) And having afterwards met with a learned traveller, that had carefully visited the famous gold-mines of *Cremnitz* in *Hungary*, he answered me, that he did not learn from the miners, whether or no the ores of gold, &c. did really grow or were regenerated in tract of time, by being exposed to the air, or upon any other account; but the grand overseer, who was lord of part of the soil, told him, that he thought the whole mountain to abound with particles of gold, and therefore was wont, when the diggers had almost exhausted the vein, to cast in store of earth, and dig up other neighbouring places, which, being kept there as in a conservatory, would afterwards afford gold, as the mine did before.

Johan. Gerhardus in Decade Quæstionum, pag. m. 19.

And, if a late German professor of physick do not misinform us, his country affords an eminent instance of the growth or regeneration of gold. *Nam Corbachi*, says he, *quæ est civitas Westphaliæ, sub ditione comitis de Isenborg & Waldeck, aurum excoquitur ex cumulis congestis, ita ut singulis quadrienniis iterum elaboretur cumulus unus, semper se restaurante naturâ, &c.*

POSTSCRIPT.

Since the setting down of the foregoing observations, I casually met with a curious book of travels, lately made by the very ingenious Dr. *Edward Brown*, and finding in page 100, a couple of relations, that seem pertinently referable, the one to a passage above-cited out of *Agricola*, in the notes about the growth of lead, and the other to the present title about the growth of gold; I thought fit to annex them in the learned author's own words, viz.

"1. Some passages in this mine cut through the rock, and long disused, have grown up again: and I observed the sides of some, which had been formerly wide enough to carry their ore through, to approach each other, so as we passed with difficulty. This happens most in moist places; the passages unite not from the top to the bottom, but from one to another.

"2. The common yellow earth of the country near *Cremnitz*, especially of the hills towards the west, although not esteemed ore, affords some gold: and in one place, I saw a great part of an hill digged away, which hath been cast into the works, washed and wrought in the same manner as pounded ore, with considerable profit.

The fore-going observations about the growth of gold and other metals are not all, that I might, perhaps without being blamed or it, have referred to that title. But all my papers, wherein other observations of this kind were set down, are not now at hand, and divers other instances, that I have met with among writers, of the growth of metals, (taking that expression in the sense I formerly declared) do not seem to me so pertinent in this place, because the improving ores were not exposed, nor perchance accessible to the air. And even as to the instances, that I have now mentioned, till severer observations have been made, to determine, whether it be partly the contact or the operation of the air, or some internal disposition, analogous to a metalline seed or ferment, that causes this metalline increment, I dare not be positive; though I thought the interest of the air in this effect might make it pardonable, to add on this occasion to the history of nature some particulars, of which the cause conjecturally proposed may be probable enough to countenance a suspicion, till further experience have more clearly instructed us.

To what has been said of the growth of metals in the air, I add some instances of the growth of fossile salts, and of some other minerals: but, besides that these belong to the paper about the saltness of the air; what has been already said may suffice for the present occasion.

POSTSCRIPT.

After what I writ in the 446 page of the following discourse, having an opportunity to look again upon the marchasite there mentioned to have been hermetically sealed up after its surface had been freed from the grains of vitriolate salt, that adhered to it, I perceived, that notwithstanding the glass had been so closely stopped, yet there plainly appeared from the outside of the mass some grains of an efflorescence, whose colour between blue and green, argued it to be of a vitriolate nature. If this be seconded with other trials made with the like success, it may suggest new thoughts about the growth of metals and minerals, especially with reference to the air.

SUS-

SUSPICIONS

ABOUT

Some Hidden QUALITIES in the AIR.

BESIDES the four firſt qualities of the air, (heat, cold, dryneſs and moiſture) that are known even to the vulgar; and thoſe more unobvious, that philoſophers and chemiſts have diſcovered, ſuch as gravity, ſpringineſs, the power of refracting the beams of light, &c. I have often ſuſpected, that there may be in the air ſome yet more latent qualities or powers differing enough from all theſe, and principally due to the ſubſtantial parts or ingredients, whereof it conſiſts. And to this conjecture I have been led, partly (though not only, or perhaps chiefly) by conſidering the conſtitution of that air we live and breathe in, which, to avoid ambiguities, I elſewhere call Atmoſpherical air. For this is not, as many imagine, a ſimple and elementary body, but a confuſed aggregate of effluviums from ſuch differing bodies, that, though they all agree in conſtituting, by their minuteneſs and various motions, one great maſs of fluid matter, yet perhaps there is ſcarce a more heterogeneous body in the world.

AND as by air I underſtand not, (as the Peripateticks are wont to do) a mere elementary body; ſo, when I ſpeak of the qualities of the air, I would not be thought to mean ſuch naked and abſtracted beings (as the ſchools often tell us of,) but ſuch as they call qualities *in concreto*, namely, corpuſcles endued with qualities, or capable of producing them in the ſubjects they invade and abound in.

In a paper about ſubterraneal ſteams.

I have elſewhere ſhewn it to be highly probable, that, beſides thoſe vapours and exhalations, which by the heat of the ſun are elevated into the air, and there afford matter to ſome meteors, as clouds, rain, parhelions and rainbows, there are, at leaſt at ſome times, and in ſome places, ſtore of effluviums emitted from the ſubterraneal parts of the terreſtrial globe; and it is no leſs probable, (from what I have there and elſewhere delivered) that in the ſubterraneal regions there are many bodies, ſome fluid and ſome conſiſtent, which, though of an operative nature, and like, upon occaſion, to emit ſteams, ſeldom or never appear upon the ſurface of the earth, ſo that many of them have not ſo much as names aſſigned them even by the mineraliſts. Now, among this multitude and variety of bodies, that lie buried out of our ſight, who can tell, but that there may be ſome, if not many, of a nature very differing from thoſe we are hitherto familiarly acquainted with; and that, as divers wonderful and peculiar operations of the loadſtone, (though a mineral many ages ago famous among philoſophers and phyſicians) were not diſcovered till of later ages, wherein its nobler virtues have been diſcloſed; ſo there may be other ſubterraneous bodies, that are endowed with conſiderable powers, which, if they were known, be found very differing from thoſe of the foſſiles we are wont to deal with?

I alſo further conſider, that (as I have elſewhere endeavoured to make it probable) the ſun and planets (to ſay nothing of the fixed ſtars) may have influences here below diſtinct from their heat and light. On which ſuppoſition it ſeems not abſurd to me to ſuſpect, that the ſubtil, but corporeal, emanations even of theſe bodies may (ſometimes at leaſt) reach to our air, and mingle with thoſe of our globe in that great receptacle or rendeſvous of celeſtial and terreſtrial effluviums, the atmoſphere. And if this ſuſpicion be not groundleſs, the very ſmall knowledge we have of the ſtructure and conſtitution of globes, ſo many thouſands or hundreds of thouſands of miles remote from us, and the great ignorance we muſt be in of the nature of the particular bodies, that may be preſumed to be contained in thoſe globes, (as minerals and other bodies are in the earth) which in many things appear of kin to thoſe that we inhabit, (as with excellent teleſcopes I have often with attention and pleaſure obſerved, particularly in the moon) this great imperfection, I ſay, of our knowledge may keep it from being unreaſonable to imagine, that ſome, if not many, of thoſe bodies and their effluxions, may be of a nature quite differing from thoſe we take notice of here about us, and conſequently may operate after a very differing and peculiar manner.

And though the chief of the heteroclite effluviums, that endow the air with hidden qualities, may probably proceed from beneath the ſurface of the earth, and from the celeſtial bodies; yet I would not deny, but that, eſpecially at ſome times, and in ſome places, the air may derive multitudes of efficacious particles from its own operations, acting as a fluid ſubſtance upon that vaſt number and variety of bodies, that are immediately expoſed to it. For, though, by reaſon of its great thinneſs, and of its being in its uſual ſtate devoid both of taſte and ſmell, it ſeems wholly unfit to be a menſtruum; yet I am not ſure but it may have a diſſolving, or at leaſt a conſuming, power on many bodies, eſpecially ſuch as are peculiarly diſpoſed to admit its operations.

FOR

For I consider, that the air has a great advantage by the vast quantity of it, that may come to work in proportion to the bodies that are exposed to it: and I have long thought, that, in divers cases, the quantity of a menstruum may much more considerably compensate its want of strength, than chemists are commonly aware of, (as there may be occasion elsewhere to exemplify.) And there are liquors, which pass for insipid, (and are therefore thought to be altogether unfit to be solvents,) which, though they have their active parts too thinly dispersed to be able presently to make sensible impressions upon our organs of tasting, yet are not quite destitute of corpuscles fit to act as a solvent; especially if they have time enough to make with the other parts of the fluid such numerous and various motions, as must bring, now some of them, and then others, to hit against the body exposed to them. Which may be illustrated by the rust like to verdigrease, which we have observed in copper, that has been long exposed to the air, whose saline particles, little by little, do, in tract of time, fasten themselves in such numbers to the surface of the metal as to corrode it, and produce that efflorescence coloured like verdigrease which you know is a factitious body, wont to be made of the same metal, corroded by the sharp corpuscles of vinegar, or of the husks of grapes: besides, that by the power, which mercury has to dissolve gold and silver, it appears, that it is not always necessary for the making a fluid fit to be a dissolvent, that it it should affect the taste. And as to those bodies, on which the aerial menstruum can, though but slowly, work, the greatest quantity of it may bring it this advantage, that, whereas even the strongest menstruums, if they bear no great proportion in bulk to the bodies they are to work on, are easily glutted, and being unable to take up any more, are fain to leave the rest of the body undissolved, our aerial menstruum bears so vast a proportion to the bodies exposed to it, that when one portion of it has impregnated itself as much as it is able, there may still come fresh and fresh to work further on the remaining part of the exposed body.

Besides the saline and sulphureous particles, that, at least in some places, may (as I have elsewhere shewn) impregnate the air, and give it a greater affinity to chemical menstruums more strictly so called; I am not averse from thinking, that the air, merely as a fluid body, that consists of corpuscles of differing sizes and solidities restlesly and very variously moved, may upon the account of these corpuscles be still resolving, or preying upon the particles of the bodies, that are exposed to their action. For many of those aerial corpuscles, some hitting and some rubbing themselves every minute against those particles of exposed bodies, that chance to lie in their way, may well, by those numerous occursions and affrictions, strike off and carry along with them now some, and then others of those particles; as you see it happens in water, which, as soft and fluid as it is, wears out such hard and solid bodies as stones themselves, if it often enough meet them in its passage, according to the known saying,

Gutta cavat lapidem non vi, sed sæpe cadendo.

And though the aerial corpuscles be very minute, and the bodies exposed to them oftentimes large and seemingly solid; yet this needs not make you reject our supposition; because it is not upon the whole body at once, that, according to us, the aerial corpuscles endeavour to work, but upon the superficial particles, which may often be more minute than those corpuscles; as you will the more easily believe, if you first observe with a good microscope, how many extant particles may be met with on the surface of bodies, that to the naked eye seem very smooth, and even of those, that are polished by art with tripoli or puttee; and then consider, that one of these protuberances, being yet manifestly visible, may well be presumed to consist of a multitude of lesser particles, divers of which may very well be as minute as those aerial corpuscles, that successively hit against them, and endeavour to carry them along with themselves. And this may be illustrated by a familiar instance. For if you take a lump of loaf sugar, or even of a much solider and harder body, sal gemmæ, and cast it into common water, though this liquor is insipid, and the motions of its corpuscles but very languid; yet these corpuscles are capable to loosen and carry off the superficial particles of sugar or salt, that chance to lye in their way, and fresh corpuscles of water still succeeding to work upon the remaining particles of the exposed body, that stands in their way, the whole lump is, by and little, dissolved, and ceases to appear to the eye a thing distinct from the liquor.

Some things, that have occurred me, to have made me suspect, that it is not impossible, but that some bodies may receive a disposition to volatility, and consequently to pass into the air by the action either of the sun-beams, in the form of sun-beams, or of some substance, that once issued out of the sun, and reached unto the air. For there may be certain bodies for the most part in the form of liquors, which though they pass off from some peculiarly disposed bodies, may during their stay or contact produce in them a great and strange aptness to be volatized. In favour of which conjecture, I might here alledge both the effects, which the Paracelsians and Helmontians ascribe to the Alkahest, of volatizing even fixed and ponderous bodies barely by being often abstracted from them, and some other things, which I shall now leave unmentioned, because you may find them in my notes about Volatility and Fixity.

But whatever become of this conjecture, it is consonant to experience, that, either upon the above recited accounts, or also some others, those parts of the atmosphere, which, in a stricter sense, may be called the air, are, at least, in some places, so intermixed with particles of differing kinds, that among that great number of various sorts of them, it is very likely that there should be some of an uncommon and unobserved nature. And I could countenance what has been said by the wasting of odorous bodies, and especially camphire, and by representing, that I have observed some solid bodies actually cold, when their superficial

parts

parts were newly taken off, to emit, though invisibly, such copious steams into the air, as to grow continually and manifestly lighter upon the balance, so as to suffer a notable decrement of weight in a minute of an hour. But the mention I make of such things in another paper, dissuades me from insisting on them here, where it will be seasonable to resume the discourse, which the mention of the dissolving power, that may be guessed to be in the air, has for some pages interrupted, and to tell you, that those propounded, before I entered upon the digression, are the two main considerations *à priori* (as they speak) whereon I have grounded my surmise, which being proposed but as a suspicion, I presume it will not be expected, that the argument *à posteriori*, which I shall bring to countenance it, should be more than conjectures, much less, that they should be demonstrations. And therefore I shall venture to lay before you some few phænomena, which seem to be at least as probably referable to some latent quality in the air, as to any other cause I yet know. Upon which score such phænomena may be allowed to be pleaded in favour of our suspicion, until some other certain cause of them shall be satisfactorily assigned.

HAVING premised thus much to keep you from looking for stronger proofs than I think my task obliges me to give; the first phænomenon, I shall propose, shall be the appearing or growth of some salts in certain bodies, which we observed to afford them either not at all, or at least nothing near in such plenty, or so soon, unless they be exposed to the air. Of such a phænomenon as this, that is not so much as mentioned by vulgar philosophers, and very rarely, if at all, to be met with in the laboratories of chymists, you will not, I suppose, wonder, that I do not present you many examples, and some few I am able to name. For I remember, that suspecting a solid marchasite, hard as stone, to be fit to be made an instance for my purpose, I caused it to be broken, that the internal more shining parts might be exposed to the air; but, though this were done in a room, where a good fire was usually kept, so that the marchasite was not only sheltered from the rain, but kept in a dry air, yet after a while I discovered upon the glistering parts an efflorescence of a vitriolate nature.

AND afterwards meeting with a ponderous and dark coloured mineral, and which, at the first breaking, discovered to the eye no appearance of any salt, nor so much as any shining marchasitical particles, we found nevertheless, that a good quantity of these hard and heavy bodies, being kept exposed to the air, even in a room, that preserved them from the rain, though probably they had lain many ages entire in the hill, wherein they were found under ground; yet in not many months, by the operation of the air upon them, they were, in great part, crumbled to powder exceeding rich in copperas. Nay, I remember, that having for curiosity sake, laid up some of these stones in a room, where I constantly kept fire, and in the drawer of a cabinet, which I did not often take out to give them fresh air, some, if not most of them, were notwithstanding covered with a copious efflorescence, which by its conspicuous colour between blue and green, by its taste, and by its fitness to make in a trice an inky mixture with infusion of galls, sufficiently manifested itself to be vitriol; whose growth by the help of the contact of the air is the more considerable, because it is not a meer acid salt, but abounds in sulphureous and combustible parts, which I have divers times been able, by methods elsewhere mentioned, actually to separate or obtain from common vitriol without the addition of any combustible body, and sometimes without any additament at all. It was also uncommon, that our blackish minerals required no longer time, nor no rain, to make them afford their vitriolate efflorescences: for I remember, I kept many of those marchasites, both glittering ones and others, of which they make and sell great quantities of vitriol at *Deptford*, without perceiving in them a change, that came any thing near to what I have recited. And I observed those, whose trade it is to make vitriol, to be often obliged to let their vitriol-stones, as they call them, lie half a year, or even eighteen months, or two years exposed, not only to the open air, but to the rain and sun, to be able to obtain from them their vitriolate parts.

THAT also the earth or ore of allum, being robbed of its salt, will in tract of time recover it by being exposed to the air, we are assured by the experienced *Agricola*, where, having delivered the way of making allum, he subjoins this advertisement: *Terra aluminosa, quæ in castellis diluta, postquam effluxit, superfuit egesta & coacervata quotidie, rursus magis & magis fit aluminosa, non aliter atque terra, ex qua halinitrum fuit confectum, suo succo plenior fit; quare denuo in castella conjicitur, & aquæ affusæ ea percolantur.*

I have likewise observed, as you also perchance have done, that some kind of lime in old walls and moist places has gained in length of time a copious efflorescence, very much of nitrous nature; as I was convinced by having obtained salt-petre from it by barely dissolving it in common water, and evaporating the filtrated solution: and, that in calcined vitriol, whose saline parts have been driven away by the violence of the fire, particles of fresh salt may be found, after it has lain a competent time in the air, I shall before long have occasion to inform you.

BUT in the mean time, (to deal ingenuously with you,) I shall confess to you, that though these and the like observations have satisfied learned men, without having been called in question, and consequently have, at least, probability enough to ground our suspicion upon; yet I, that am more concerned for the discovery of a truth than the reputation of a paradox, propose the argument drawn from the foregoing observations, but as a probationer. For it yet seems to me somewhat doubtful, whether the salts, that appear in the forementioned cases, are really produced by the operation of the air working as an agent, or also concurring as an ingre-

ingredient; or whether these saline substances be not the production of some internal thing, that is analagous to a seminal principle, which makes in these bodies a kind of maturation of some parts, which being once ripened, and perhaps assisted by the moisture of the air, disclose themselves in the form of saline concretions; as in the feculent or tartareous parts of many wines, there will in tract of time be generated or produced store of corpuscles of a saline nature, that produce the acid taste we find in tartar, especially, that of rhenish wine. It may also be suspected, that the formerly mentioned salts found in marchasites, in nitrous and aluminous earths, &c. are made by the saline particles of the like nature, that among multitudes of other kinds swim in the air, and are attracted by the congenerous particles, that yet remain in the terrestrial bodies, that are, as it were, the wombs of such minerals, (as I have elsewhere shewn, that the spirit of nitre will, with fixed nitre and some other alkalies, compose salt-petre;) or else, that these aerial salts, if I may so call them, assisted by the moisture of the air, do soften and open, and almost corrode or dissolve the more terrestrial substance of these wombs, and thereby sollicit out and somewhat extricate the latent saline particles, and, by their union with them, compose those emerging bodies, that resemble vitriol, allum, &c.

But not only to suggest these scruples, as if I had a mind they should but trouble you, and keep you irresolute, I shall propound something towards the removal of them; namely, that a convenient quantity of nitrous earth, or that other of those substances, which you would examine, be kept in a close vessel to which the air has not access, for at least as long time as has been observed to be sufficient to impregnate the like substance, or rather a portion of the same parcel, that was chosen to be included: for if the body, that was kept close, have either gained no salt all, or very much less in proportion to its bulk than that, which was kept exposed, we may thence estimate, what is to be ascribed to the air in the production of nitre or other saline concretions. And, because I have observed none of these bodies, that would so soon, and so manifestly even to the eye, disclose a saline substance, as the blackish vitriol-ore, I lately told you I kept in a drawer of my cabinet; I judged, that a very fit subject, wherewith to try, what maturation, or time, when the air was secluded, would perform towards the deciding of our difficulty: and accordingly having taken some fragments of it, which we had carefully freed from the adhering vitriolate efflorescence, by whose plenty we are assured, that it was very well disposed to be wrought on by the air, we put of these fragments of differing sizes into two conveniently shaped glasses, which being hermetically sealed were ordered to be carried away, and kept in fixed places; by which means it was expected, that, even without opening the glasses, we should be able easily to see by the changed colour of the superficial parts, whether any vitriolate efflorescence were produced; but, through the negligence or mistake of those, to whom the care was recommended, the experiment was never brought to an issue; and though I afterwards got more of the mineral, and made a second trial of the same, I have not yet been informed of the event.

But, Sir, though, until the success of some trial be known, I dare not too confidently pronounce about the production or regeneration of salts in bodies, that have been robbed of them, and ascribe it wholly to the air; yet, when I consider the several and great effects of the air upon divers other bodies, I think it not rash to conjecture, in the mean time, that the operations of the air may have a considerable share in these phænomena, and so that there may be latent qualities in the air, in the sense I declared above, where I told you, that when I speak of these qualities, I look upon them *in concreto*, (as they phrase it,) together with the substances or corporeal effluvia they reside in: and of these aerial qualities, taken in this sense, I shall now proceed to mention some other instances.

The difficulty we find of keeping flame and fire alive, though but for a little time, without air, makes me sometimes prone to suspect, that there may be dispersed through the rest of the atmosphere some odd substance, either of a solar, or astral, or some other exotic nature, on whose account the air is so necessary to the subsistence of flame; which necessity I have found to be greater, and less dependent upon the manifest attributes of the air, than naturalists seem to have observed. For I have found by trials purposely made, that a small flame of a lamp, though fed perhaps with a subtil thin oyl, would in a large capacious glass-receiver expire, for want of air, in a far less time than one would believe. And it will not much lessen the difficulty to alledge, that either the gross fuliginous smoak did in a close vessel stifle the flame, or, that the pressure of the air is requisite to impel up the aliment into the wieck: for, to obviate these objections, I have in a larger receiver imployed a very small wieck with such rectified spirit of wine, as would in the free air burn totally away; and yet, when a very small lamp, furnished (as I was saying) with a very slender wieck, was made to burn, and, filled with this liquor, was put lighted into a large receiver, that little flame, though it emitted no visible smoak at all, would usually expire within about one minute of an hour, and, not seldom, in a less time; and this, though the wieck was not so much as singed by the flame: nor indeed is a wieck necessary for the experiment, since highly rectified spirit of wine will in the free air flame away well without it. And indeed it seems to deserve our wonder, what that should be in the air, which enabling it to keep flame alive, does yet, by being consumed or depraved, so suddenly render the air unfit to make flame subsist. And it seems by the sudden wasting or spoiling of this fine subject, whatever it be, that the bulk of it is but very small in proportion to the air it impregnates with

with its virtue. For after the extinction of the flame, the air in the receiver was not visibly altered, and, for aught I could perceive by the ways of judging I had then at hand, the air retained either all, or at least far the greatest part of its elasticity, which I take to be its most genuine and distinguishing property.

AND this undestroyed springiness of the air seems to make the necessity of fresh air to the life of hot animals, (few of which, as far as I can guess after many trials, would be able to live two minutes of an hour, if they were totally and all at once deprived of air,) suggest a great suspicion of some vital substance, if I may so call it, diffused through the air, whether it be a volatile nitre, or (rather) some yet anonimous substance, sydereal or subterraneal, but not improbable of kin to that, which I lately noted to be so necessary to the maintenance of other flames.

I know not, whether you will think it pertinent to our present discourse, that I observe to you, that by keeping putrifying bodies in glasses, which by *Hermes* his seal were secured from the contact of the external air, I have not been able to produce any insect, or other living creature, though sometimes I have kept animal substances, and even blood so included, for many months, and one or two of them for a longer time; and though all these substances had a manifest change made in their consistence whilst they remained sealed up.

ON this occasion I shall add an odd observation, that I met with in a little dissertation *de admirandis Hungariæ aquis*, written by an anonymous, but ingenious nobleman of that country, where, speaking of the native salt, that abounds in their regions, he says, that in the chief mine (by them called *Desiensis*) of *Transylvania*, there was, a few years before he writ, a great oak, like a huge beam, dug out of the middle of the salt; but, though it was so hard, that it would not easily be wrought upon by iron tools, yet being exposed to the air out of the mine, it became so rotten, as he expresses it, that in four days it was easy to be broken, and crumbled between one's fingers. And of that corruptive or dissolutive power of air near those mines, the same author mentions other instances.

HAVING found an antimonial preparation to procure vomits, in a case where I did not at all expect it, I was afterwards curious to enquire of some physicians and chymists, that were of my acquaintance, whether they had not taken notice, that Antimonium Diaphoreticum, which, as its name imports, is wont to work by sweat or transpiration, would not become vomitive, if it were not kept from the air? To which one physician, that was a learned man, assured me, it would, as he had found by particular trials: and the like answer has been given me by more than one. And I find, that the experienced *Zwelfer* himself does somewhere give a caution against letting the air have access to these antimonial medicines, lest it should render them, as he says it will, in tract of time, not only emetic, but disposed to produce heart-burnings, (as they call them,) faintings, and other bad symptoms. And I learned by enquiry, from a very ingenious doctor of physic, that, having carefully prepared Antimonium Diaphoreticum, he gave many doses whilst it was fresh and kept stopped in a glass, (without finding, that in any patient it procured so much as one vomit,) but having kept a parcel of the self-same remedy for a pretty while in a glass only covered loosely with a paper, the medicine, vitiated by the air, proved emetic (strongly enough) to those, who neither by constitution, or foulness of stomach, or on any other discernible account, were more than others, that had taken it disposed to vomit. By which observations, and from what I formerly told you of the salt-petre obtainable from quick-lime, a man partial to the air would be made forward to tell you, that this looks, as if either there were in the air a substance disposed to be assimilated by all kinds of bodies, or that the air is so vast and rich a rendezvouz of innumerable seminal corpuscles, and other analogous particles, that almost any body long exposed to it may there meet with particles of kin to it, and fit to repair its wrongs and losses, and restore it to its natural condition. But without taking any further notice of this odd surmize, I will proceed to mention two or three other phænomena of nature, that seem to favour the suspicion, that there may be secret qualities in the air, in reference to some bodies.

THE ingenious Monsieur *de Rocheford*, in the handsome account he gives of the apple, or fruit of the tree junipa, whose juice is employed by the Indians to black their skins, that they may look the more terrible to their enemies, observes, that, though the stain, or, as he speaks, the tincture of this fruit cannot be washed out with soap, yet, within nine or ten days, it will vanish of itself; which would make one suspect, that there may be in the air some secret powerful substance, that makes it a menstruum of more efficacy than soap itself to obliterate stains I remember, I have seen this fruit, but not whilst it was succulent enough to have a trial made with it; which I was therefore troubled at, because the author does not clearly express, whether this disappearing of the tincture happens indifferently to the bodies it chances to stain, or only is observed on the skins of men. For, as in the former case, it will afford an instance pertinent to our present purpose; so in the latter I should suspect, that the vanishing of the tincture may be due, not so much to the operation of the air upon it, as to the sweat and exhalations of a human body, which abounding with volatile salt, may either destroy or carry off with them, the coloured particles they meet with in their passages.

I have sometimes, not altogether without wonder, observed the excellency of the better sort of Damasco-steel, (for I speak not of all that goes under that name,) in comparison of ordinary steel. And, besides what I have elsewhere taken notice of concerning it, there is one phænomenon, which though I am not sure

ſure it belongs to the latent qualities of the air, yet, becauſe it may well do ſo, and I am unwilling it ſhould be loſt, I will here tell you, that having enquired of an eminent and experienced artificer, (whom I long ſince employed in ſome difficult experiments,) about the properties of Damaſco-ſteel, this honeſt and ſober man averred to me, that when he made inſtruments of it, and gave them the true temper, which is ſomewhat differing from that of other ſteel, he generally obſerved, that though, when raſors or other inſtruments made of it were newly forged, they would be ſometimes no whit better, and ſometimes leſs good, than thoſe made of other ſteel; yet when they had been kept a year or two or three in the air, though nothing elſe were done to improve them, they would be found much to ſurpaſs other inſtruments of the ſame kind, and what themſelves were before; in ſo much, that ſome of them have been laid aſide at firſt, as no way anſwering the great expectation conceived of them, which after two or three years were found to ſurpaſs it; of which alſo I am now making a trial. I have ſeveral times made a ſubſtance, that conſiſts chiefly of a metalline body, and is of a texture cloſe enough to lie for many hours undiſſolved in a corroſive menſtruum; and yet this ſubſtance, that was fixed enough to endure the being melted by the fire without loſing its colour, would, when I had purpoſely expoſed it to the air, be diſcoloured in a very ſhort time, and have its ſuperficial parts turned almoſt black.

AND this brings into my mind that very pretty obſervation, that has been newly made in *Italy* by an ingenious man, who took notice, that, if after the opening of a vein, the blood be kept till it be concreted, and have excluded the ſuperficial ſerum, though the lower part be uſually of a dark and blackiſh colour, in compariſon of the ſuperficial parts, and therefore be counted far more feculent; yet, if the lump, or clot of blood be broken, and the internal, and dark coloured parts of the blood be expoſed to the air, it will after a time (for it is not ſaid how long) be wrought on by the contact of the air, that the ſuperficial part of the blood will appear as florid, as the lately mentioned upper part (ſuppoſed to be, as it were, the flower of the blood,) did ſeem before. And this obſervation I found to hold in the blood of ſome beaſts, whereon I tried it, in which I found it to ſucceed in much fewer minutes, than the Italian virtuoſo's experiment on human blood would make me expect.

ON the other ſide I have often prepared a ſubſtance, whoſe effect appears quite contrary to this. For, though this factitious concrete, whilſt kept to the fire, or very carefully preſerved from the air, be of a red colour, almoſt like the common opacous bloodſtone of the ſhops; yet, if I broke it, and left the lumps, or fragments of it, a little while in the air, it would in a ſhort time (ſometimes perhaps, not amounting to a quarter of an hour) it would, I ſay, have its ſuperficial part turned of a dark colour, very little, and ſometimes ſcarce at all, ſhort of blackneſs.

A very inquiſitive perſon of my acquaintance, having occaſion to make, by diſtillation, a medicine of his own deviſing, chanced to obſerve this odd property in it, that, at that time of the year, if it were kept ſtopped, it would be coagulated almoſt like oil of anniſeeds in cold weather; yet, if the ſtopple were taken out, and ſo acceſs were for a while given to the air, it would turn to a liquor, and the veſſel being again ſtopped, it would, though more ſlowly, recoagulate. The hints, that I gueſſed, might be given by ſuch a phænomenon, making me deſirous to know ſomething of it more than barely by relation, I expreſſed rather a curioſity than diffidence about it; and the maker of it telling me, he thought, he had in a ſmall vial about a ſpoonful of this medicine left in a neighbouring chamber, I deſired his leave to conſider it myſelf, which requeſt being preſently complied with, I found it, when he brought it into the room which I ſtayed in, not liquid but conſiſtent, though of but a ſlight and ſoft contexture. And having taken out the cork, and ſet the vial in a window, which (if I well remember) was open, though the ſeaſon, which was winter, was cold, yet in a little time, that I ſtayed talking with the chemiſt, I found, that the ſo lately coagulated ſubſtance was almoſt all become fluid. And another time, when the ſeaſon was leſs cold, having occaſion to be where the vial was kept well ſtopped, and caſting my eyes on it, I perceived the included ſubſtance to be coagulated much like oil of anniſeeds. And this ſubſtance having, as the maker aſſured me, nothing at all of mineral in it, nor any chemical ſalt, it conſiſting only of two ſimple bodies, the one of a vegetable, and the other of an animal ſubſtance, diſtilled together, I ſcarce doubt but you will think with me, that theſe contrary operations of the air, which ſeems to have a power in ſome circumſtances to coagulate ſuch a body, and yet to diſſolve and make it fluid, when freſh and freſh parts are allowed acceſs to it, may deſerve to be further reflected on, in reference (among other things) to the opportune operations, the inſpired air may have on the conſiſtence and motion of the circulating blood, and to the diſcharge of the fuliginous recrements to be ſeparated from the blood in its paſſage through the lungs.

THERE are two other phænomena, that ſeemed favourable to our ſuſpicion, that there are anonymous ſubſtances and qualities in the air, which ought not to be altogether prætermitted on this occaſion; though, becauſe to ſpeak fully of them would require far more time than I can now ſpare, I ſhall ſpeak of them but ſuccinctly.

THE latter of theſe two phænomena is the growth or appearing production of metals or minerals dug out of the earth, and expoſed to the air. And this, though it be the laſt of the two, I mention firſt, becauſe it ſeems expedient, leſt it ſhould prove too long an an interruption to our discourse, to postpone the observations, and annex them to the end of this paper; only intimating to you now, that the caution I formerly interposed about the regeneration of salts in nitrous, and other earths, may, for greater security, be applied, *mutatis mutandis*, to that production of metalline and mineral bodies we are speaking of.

THE other of the two phænomena, I lately promised to mention, is afforded me by those various and odd diseases, that at some times, and in some places, happen to invade, and destroy numbers of beasts, sometimes of one particular kind, and sometimes of another. Of this we have many instances in the books of approved authors, both physicians and others; and I have myself observed some notable examples of it. But yet I should not mention it as a ground of suspicion, that there may be, in some times and places, unknown effluvia and powers in the air, but, that I distinguish these from those diseases of animals, that proceed, as the rot in sheep often does, from the exorbitancy of the seasons, the immoderateness of cold, heat, or any other manifest quality in the air. And you will easily perceive, that some of these examples probably argue, that the subterraneal parts do sometimes (especially after earthquakes, or unusual cleavings of the ground) send up into the air peculiar kinds of venomous exhalations, that produce new and mortal diseases in animals of such a species, and not in those of another, and in this or that particular place, and not elsewhere: of which we have an eminent instance in that odd plague or murrain of the year 1514, which, *Fernelius* tells us, invaded none but cats. And even in animals of the same species, sometimes one sort have been incomparably more obnoxious to the plague than another; as *Dionysius Halicarnasseus* mentions a plague, that attacked none but maids; whereas, the pestilence, that raged in the time of *Gentilis* a famed physician, killed few women, and scarce any but lusty men. And so *Boterus* mentions a great plague, that assualted almost only the younger sort of persons, few past thirty years of age being attacked by it: which last observation has been also made by several later physicians. To which may be added, what learned men of that faculty have noted at several times concerning plagues, that particularly invaded those of this or that nation, though confusedly mingled with other people; as *Cardan* speaks of a plague at *Basil*, with which only the Switzers, and not the Italians, French, or Germans, were infected. And *Johannes Utenhovius* takes notice of a cruel plague at *Copenhagen*, which, though it raged among the Danes, spared both the English, Dutch, and Germans, though they freely entered infected houses, and were not careful to shun the sick. In reciting of which instances I would not be understood, as if I imputed these effects merely to noxious subterraneal fumes; for I am far from denying, that the peculiar constitutions of men's bodies are likely to have a great interest in them; but yet it seems less probable, that the pestilent venom diffused through the air should owe its enormous and fatal efficacy to the excess of the manifest qualities of the air, than to the peculiar nature of the pestilential poison sent up into the air from underground, which when it is by dilution or dissipation enervated, or by its progress past beyond the air we breathe in, or rendered ineffectual by subterraneal, or other corpuscles, of a contrary quality, the plague, which it, as a con-cause, produced, either quite ceases, or degenerates into somewhat else. But I have not time to countenance this conjecture, much less to consider, whether some of those diseases, that are wont to be called new, which either did begin to appear, or at least to be rife, within these two or three centuries, as the *Sudor Anglicus* in the fifteenth century, the scurvy, and the *Morbus Hungaricus*, the *Lues Moraviæ*, *Novus Morbus Luneburgensis*, and some others, in the last century of all, may be in part caused by the exotic steams this discourse treats of. But this consideration I willingly resign to physicians.

AND now, if the two forementioned suspicions, the one about subterraneal, the other about sidereal, effluviums, shall prove to be well grounded, they may lead us to other suspicions and further thoughts about things of no mean consequence; three of which I shall venture to make mention of in this place.

I. FOR we may hence be awakened to consider, whether divers changes of temperature and constitution in the air, not only as to manifest qualities, but as to the more latent ones, may not sometimes in part, if not chiefly, be derived from the paucity or plenty, and peculiar nature of one or both of these sorts of effluviums. And in particular, we find in the most approved writers such strange phænomena to have several times happened in great plagues and contagious diseases, fomented and communicated, nay (as many eminent physicians believed) begun, by some latent pestiferous, or other malignant, diathesis or constitution of the air, as have obliged many of the learnedest of them to have recourse to the immediate operation of the angels, or of the power and wrath of God himself, or at least to some unaccountable influence of the stars; none of the solutions of which difficulties seem preferable to what may be gathered from our conjecture; since of physical agents, of which we know nothing so much, as that they are to us invisible and probably of a heteroclite nature, it need be no great wonder, that the operation should also be abstruse, and the effects uncommon. And on this occasion it may be considered, that there are clearer inducements to perswade us, that another quality of the atmosphere, its gravity, may be altered by unseen effluviums, ascending from the subterraneous regions of our globe; and we have often perceived by the mercurial baroscope the weight of the air to be notably encreased, when we could not perceive in the air, nor surface of earth, any cause, to which we could ascribe so notable a change. And this gives me a rise to add, that I have sometimes allowed myself to doubt,

doubt, whether even the ſun itſelf may not now and then alter the gravity of the atmoſphere otherwiſe than by its beams of heat. And I remember, I deſired ſome virtuoſi of my acquaintance to aſſiſt me in the enquiry, whether any of the ſpots, that appear about the ſun, may not, upon their ſudden diſſolution, have ſome of their diſcuſſed and diſperſed matter thrown off, as far as to our atmoſphere, and, that copiouſly enough to produce ſome ſenſibly alterations in it, at leaſt as to gravity.

II. Another thing, that our two forementioned ſuſpicions, if allowed of, will ſuggeſt, is, that it may not ſeem altogether improbable, that ſome bodies, we are converſant with, may have a peculiar diſpoſition and fitneſs to be wrought on by, or to be aſſociated with, ſome of thoſe exotic effluvia, that are emitted by unknown bodies lodged under ground, or that proceed from this or that planet. For what we call ſympathies and antipathies depending indeed on the peculiar textures and other modifications of the bodies, between whom theſe friendſhips and hoſtilities are ſaid to be exerciſed, I ſee not, why it ſhould be impoſſible, that there be a cognation betwixt a body of a congruous or convenient texture, (eſpecially as to the ſhape and ſize of its pores,) and the effluviums of any other body, whether ſubterraneal or ſidereal. We ſee, that convex burning-glaſſes, by virtue of their figure and the diſpoſition of their pores, are fitted to be pervaded by the beams of light and to refract them, and thereby to kindle combuſtible matter; and the ſame beams of the ſun will impart a lucidneſs to the Bolonian ſtone. And as for ſubterraneal bodies, I elſewhere * mention two minerals, which being prepared, (as I there intimate,) the ſteams of the one, aſcending without adventitious heat, and wandering through the air, will not ſenſibly work on other bodies; but if they meet with that, which we prepared, they will immediately have an operation on it, whoſe effect will be both manifeſt and laſting.

III. I now paſs on to the other thing, that the two formerly mentioned ſuſpicions may ſuggeſt, which is, that if they be granted to be well founded, we may be allowed to conſider, whether among the bodies we are acquainted with here below, there may not be found ſome, that may be receptacles, if not alſo attractives, of the ſidereal, and other exotic effluviums, that rove up and down in our air.

Some of the myſterious writers about the philoſophers-ſtone ſpeak great things of the excellency of what they call their philoſophical magnet, which, they ſeem to ſay, attracts and (in their phraſe) corporifies the univerſal ſpirit, or (as ſome ſpeak) the ſpirit of the world. But theſe things being abſtruſities, which the writers of them profeſſed to be written for, and to be underſtood only by the ſons of art; I, who freely acknowledged I cannot clearly apprehend them, ſhall leave them in their own worth as I found them, and only, for brevity ſake, make uſe of the received word of a magnet, which I may do in my own ſenſe, without avowing the received doctrine of attraction. For by ſuch a magnet as I here propoſe to ſpeak of, I mean not a body, that can properly attract our foreign effluviums; but ſuch an one, as is fitted to detain and join with them, when by virtue of the various motions, that belong to the air as a fluid, they happened to accoſt the magnet. Which may be illuſtrated by the known way of making oil of tartar (as the chemiſts call it) *per deliquium*. For, though the ſpagyriſts and others ſuppoſe, that the fiery ſalts draw to it the aqueous vapours, yet indeed it does but arreſt, and embody with ſuch of thoſe, that wander through the air, as chance in their paſſage to accoſt it.

And, without receding from the Corpuſcularian principles, we may allow ſome of the bodies, we ſpeak of, a greater reſemblance to magnets, than what I have been mentioning. For not only ſuch a magnet may upon the bare account of adheſion by juxta-poſition, or contact, detain the effluviums, that would glide along it, but theſe may be the more firmly arreſted by a kind of precipitating faculty, that the magnet may have in reference to ſuch effluviums; which, if I had time, I could illuſtrate by ſome inſtances; nay, I dare not deny it to be poſſible, but, that in ſome circumſtances of time, or place, one of our magnets may, as it were, fetch in ſuch ſteams, as would indeed paſs near it, but would not otherwiſe come to touch it. On which occaſion I remember, I have in certain caſes been able to make ſome bodies, not all of them electrical, attract (as they ſpeak) without being excited by rubbing, &c. far leſs light bodies, than the effluviums we are ſpeaking of.

But this it may ſuffice to have glanced at, it not being here my purpoſe to meddle with the myſtical theories of the chemiſts; but rather to intimate, that, without adopting, or rejecting them, one may diſcourſe like a naturaliſt about magnets of celeſtial, and other emanations, that appear not to have been conſidered, not to ſay thought of, either by the ſcholaſtick, or even the mechanical philoſophers.

* See the Experiment in the diſcourſe of the Determinate Nature of Effluviums.

OF
CELESTIAL AND AERIAL
MAGNETS.

IF now, upon what I have granted in the close of the past discourse, you should urge the question further, and press me to declare, whether, as I think it no impossible thing, that nature should make, so I think it no unpracticable or hopeless thing, that men should find, or art should prepare, useful magnets of the exotic effluviums of the lower region of the earth, or the upper of the world: it would much distress me to give any other answer, than that I think it extremely difficult, and not absolutely impossible; and therefore I would not discourage any curious or industrious man from attempting to satisfy himself by experiments, because even a seemingly slight discovery in a thing of this nature may be of no small use in the investigation of the nature of the air, especially in some particular places, and of the correspondency, which, by the intervention of the air, the superficial part of the terrestrial globe may have both with the subterraneal regions of the earth, and the celestial ones of the universe. Some of the things I have tried or seen relating to this discovery, I must, for certain reasons, leave here unmentioned; and only advertise you, that several bodies, which experience has assured us do imbibe or retain something from the air, as some calcined minerals, some marchasites, some salts, as well factitious as natural, &c. may be fit to be often exposed to it, and then weighed again, and further diligently examined, whether that, which makes the increment of weight, be a mere imbibed moisture, or also somewhat else; and likewise, whether it be separable from the body or not, or however have endowed it with any considerably quality; and if you chance to meet with a good magnet, you may then vary experiments with it, by exposing it long to the air in regions differing much in climate, or soil, or both, by exposing it by day only, or by night, at several seasons of the year, in several temperatures of the air, at several considerable aspects of the stars and planets, by making it more or less frequently part with what it has gained from the air; and in short, by having regard to variety of circumstances, which your curiosity and sagacity may suggest. For, by thus diversifying the experiment many ways, you may perhaps, by one or other of them, make some unexpected and yet important discovery of what effluviums the air, in particular places and times, abounds with, or wants, and perchance too, of some correspondency between the terrestrial and etherial globes of the world.

I shall neither be surprized, nor quarrel with you, if you tell me, that these are extravagant thoughts; but if I had been fortunate in preserving all, that trial, observation, or other productions of some curiosity, I once had for such enquiries, procured me, you would not, perhaps, think me so very extravagant. But though I must not here make any further mention of them, and shall only take notice of one body, namely vitriol, whether crude, or unripe, and (as chemists speak) embrionated, or spagyrically prepared; yet some phænomena of these vitriolate substances may for the present, I hope, somewhat moderate your censure for my putting you upon observations, that I fear you yourself will judge unpromising, and less favourable persons, than you would think phantastical. And to let you see by a pregnant instance, that the air may not only have a notable operation upon vitriol, and that, after a strong fire could work no farther on it, but that this operation was considerably diversified by circumstances; I shall begin what I have to alledge, with what the experienced *Zwelfer* occasionally observed, and relates to usher in a caution about a chemical preparation of vitriol: For, having informed his reader, that the colcothar, that is made by a strong distillation, is not corrosive, he denies, that (to use his own words) *statim à distillatione sal ex eodem, affusâ aquâ, elici queat*; *sed tum prius*, (continues he,) *ubi aliquandiu aeri expositum fuerit*; *tunc enim sal præbet quandoque candidum, quandoque purpureum, aspectu pulcherrimum, quod aliquando in copia acquisivi, & penes me asservo, quandoque etiam nitrosum.*

WHICH testimony of this candid spagyrist has much the more weight with me, because I find, what he affirms of the saltlesness of newly and strongly calcined vitriol to be very agreeable to some of my experiments about colcothar of blue (venereal) vitriol; which salt or mineral (I mean vitriol) is so odd a concrete, that I have thought fit more than once to recommend the making experiments about it to several curious persons, that had better opportunity to continue them than I, whose residence was not so fixed. And I remember, that one of these, a person industrious and versed in chemical operations, gave me this account, that not only he had differing kinds of salts from colcothar exposed to the air for many months, and robbed at convenient times of what it had acquired, but that in tract of time he found it so altered, that he obtained from it a pretty quantity of true running mercury.

AND now, to reſume and conclude what I was ſaying about colcothar, there are two or three things I would propoſe to be obſerved by you, or any virtuoſo, that would aſſiſt me in theſe trials about this odd *calcinatum*, (for to call it *terra damnata* were to injure it.)

THE firſt is, to take notice of ſome circumſtances, that moſt obſervers would overlook; ſuch as (beſides the nature of the ſoil) the temperature of the air, the month of the year, and the winds, the weight of the atmoſphere, the ſpots of the ſun, if any be, the moon's age, and her place in the Zodiac, and the principal aſpects of the planets, and the other chief ſtars. For, though it be a boldneſs to affirm, that any, or perhaps all of theſe together, will have any intereſt in the production of the ſalt or other ſubſtance, to be made or diſcloſed in the colcothar; yet in things new and exorbitant, it may be ſometimes raſh and peremptory to deny, even ſuch things as cannot, without raſhneſs, be poſitively aſſerted; and in our caſe, the ſmall trouble of taking notice of circumſtances will be richly paid by the leaſt diſcovery made in things ſo abſtruſe and conſiderable. And as we cannot yet knowingly pronounce, ſo much as negatively, whether the libration of the moon, and the motion of the ſun (and perhaps of ſome of the other planets) about their own centers, and conſequently their obverting ſeveral parts of their bodies to us, may have an operation upon our atmoſphere; ſo, for aught I know, there may be in thoſe vaſt internal parts of the earth, whoſe thin cruſt only has been here and there dug into by men, conſiderable maſſes of matter, that may have periodical revolutions, or accenſions, or euſtations, or fermentations, or, in ſhort, ſome other notable commotions, whoſe effluvia and effects may have operations, yet unobſerved, on the atmoſphere, and on ſome particular bodies expoſed to it; though theſe periods may be perhaps either altogether irregular, or have ſome kind of regularity differing from what one would expect. As we ſee, that the ſea has thoſe grand intumeſcencies, we call ſpring tides, not every day, nor at any conſtant day of the month or week, but about the full and new moon; and theſe ſpring tides are moſt notably heightened, not every month, but twice a year, at or about the vernal and autumnal equinoxes; which obſervations have not been near ſo ancient and known, as the daily ebbing and flowing of the ſea. The Eteſians of the ancients I ſhall not now inſiſt on, nor the obſervations, that I think I elſewhere mentioned of the elder inhabitants of the *Caribbee-iſlands*, who, when the Europeans firſt reſorted thither, were wont to have hurricanes but once in ſeven years; afterwards they were moleſted with them but once in three years; and of late they are troubled with them almoſt every year. And a phyſician, that lived there, told me, that he had ſcarce ever obſerved them to come but within the compaſs of two months joining to one another. In which inſtances, and divers others, that may be noted of what changes happened to great quantities of matter, nature ſeems to affect ſomething of periodical, but not in a way, that appears too regular.

ONE may add on this occaſion, that memorable paſſage related by the learned *Varenius** of thoſe hot ſprings in *Germany*, that he calls *Thermæ Piperinæ*, of which he affirms in more than one place, that they have this peculiarity, that they annually begin and ceaſe to flow at certain times; the former about the third of *May*, and the latter near the middle of *September*, at which time they are wont to reſt till the following ſpring. But though, for aught I know, our geographer's obſervation will hold in hot ſpring; yet it muſt not be extended to all, at leaſt, if we admit that, which is related by the accurate *Johannes de Laet*, (I ſuppoſe out of *Ximenes*, or the famous conqueror of *Mexico*, *Cortes*,) who tells us, that in the Mexican province, *Xilotepec fons celebratur, qui quatuor continuis annis ſcaturit, deinde quatuor ſequentibus deficit, & rurſus ad priorem modum erumpit, &, quod mirabile, pluviis diebus, parciùs, quum ſudum eſt tempus & aridum, copioſiùs, exuberat.* *Amer, lib. V. cap. 7.*

BUT this is not a place to enlarge upon the grounds of my ſuſpecting, there may be ſome periodical motions and commotions within the terreſtrial globe; what has been mentioned being only to invite you to take notice of circumſtances in your obſervations of colcothar, ſome of which may, with the more ſhew of probability, be kept expoſed for a long time, becauſe that bars of windows, and other erected irons, I have found to acquire, in tract of time, from the effluvia of the earth, a ſettled magnetiſm.

THE other main thing I would recommend, is, that notice be taken not only of the kind of vitriol, the colcothar is made of; (for I generally uſed blue Dantzick vitriol) as martial vitriol, Hungarian vitriol, Roman vitriol, &c. to which I have, for curioſity, added vitriol made by ourſelves of the ſolution of the more ſaline parts of marchaſites in water, without the uſual additament of iron, or copper; but alſo, to what degree the calcination is made, and how far the calcined matter is freed from the ſalt by water. For theſe circumſtances, at leaſt in ſome places, may be of moment, and perhaps may afford us good hints of the conſtitution of the atmoſphere in particular parts, as well as of the beſt preparation of colcothar for detaining the exotic effluviums. And I would the rather have experiments tried again in other places with colcothar not calcined to the utmoſt, nor yet ſo exquiſitely edulcorated, but that ſome ſaline particles ſhould be left in it for future encreaſe; becauſe I have more than once purpoſely tried in vain, that the *Caput Mortuum* of blue vitriol, whereof the oil and other parts had been driven off with a violent and laſting fire, would not, when freſh, impart any ſaltneſs to the water; nor do I think, that out of ſome ounces purpoſely edulcorated I obtained one grain of ſalt. And this ſaltleſs colcothar being expoſed, ſome by me, and ſome by a friend, that had conveniency in another place not far off, to the air, ſome for many weeks and ſome for divers months, we did not find it to have manifeſtly encreaſed in weight, or to have acquired any ſenſible ſaltneſs,

* *Varenius, lib.* 1. *Geograph. Univerſ. Thermæ omnes ferè quas novimus ſine ceſſatione fluunt, exceptis Piperinis Germaniæ, &c.*

neſs, which, ſuppoſing the vitriol to have nothing extraordinary, gave me the ſtronger ſuſpicion of ſome peculiarity in the air of that part of *London*, where the trials had been made, at leaſt, during thoſe times, wherein we made them; becauſe not only former experience, made here in *England*, had aſſured me, that ſome colcothars will gain no deſpicable acceſſion of weight by being expoſed to the air; but accidentally complaining of my lately mentioned diſappointment to an ingenious traveller, that had, in divers countries, been curious to examine their vitriols, he aſſured me, that, though he uſually dulcified his colcothar very well, yet within four or five weeks he found it conſiderably impregnated by the air it was expoſed to.

IT remains, that I add one intimation more about vitriol, which is, that I have found it to have ſo great a correſpondency with the air, that it would not be amiſs to try, not only colcothar of differing vitriols (whether barely made the common way, or without any metalline addition to the vitriol ſtones or ore,) but other preparations of vitriol too, ſuch as expoſing vitriol, only calcined to whiteneſs by the ſun-beams, or further to an higher colour by a gentle heat, or throughly calcined, and then impregnated with a little of its own oil. For ſuch vitriolate ſubſtances as theſe the air may work upon, nay even liquid preparations of vitriol may be peculiarly affected by the air, and thereby perhaps be uſeful to diſcover the preſent conſtitution, or foretel ſome approaching changes of it. Of the uſe of which conjecture, namely the peculiar action of the air on ſome vitriolate liquors, I remember I ſhewed ſome virtuoſi a new inſtance in an experiment, whereof this was the ſum:

[I elſewhere mention a compoſition, that I deviſed, to make with ſublimate, copper, and ſpirit of ſalt, a liquor of a green exceeding lovely. But in the deſcription of it I mentioned not (having no need to do it there) a circumſtance as odd, as the liquor it ſelf was grateful. For the air has ſo much intereſt in the production of this green, that when you have made the ſolution of the copper and mercury with the ſpirit of ſalt, that ſolution will not be green, nor ſo much as greeniſh, as long as you keep it ſtopped in the bolt-head, or ſuch like glaſs, wherein it is made. But if you pour it out into a vial, which, by not being ſtopped, leaves it expoſed to the air, it will after a while ſooner or later attain that delightful green, that ſo much endears it to the beholder's eye. This appeared ſo odd an experiment to the virtuoſi, to whom I firſt related it, that thoſe, that could not gueſs by what means I attained it, could ſcarce believe it. But that troubled not me, who, to ſatisfy myſelf not only of the truth of the experiment, but that it was not ſo contingent as many others, repeated it ſeveral times, and found the ſolution, till the air made it floriſh, to be of a muddy reddiſh colour, quite differing from green. So that I remember, that having once kept ſome of the liquor in the ſame glaſs egg, wherein the ſolution had been made, it looked like very dirty water, whilſt the other part of the ſame ſolution, having been expoſed to the air, emulated the colour of an emerald. In which change it is remarkable, that to clarify this liquor and give it a tranſparent greeneſs, I perceived not, that any precipitation of foul matter was made, to which the alteration could be aſcribed; and yet to make it the more probable, that this change proceeded not from a ſubſidence made of ſome opacating matter effected by ſome days reſt, I kept ſome of the ſolution ſealed up in a fine vial ſeveral months, without finding it at the end of that time other than a dark or muddy liquor, which in ſhort time it ceaſed to be, when, the hermetic ſeal being broken off, the air was permitted to work upon it. And this I further obſerved in our various experiments on this liquor, that, according to the quality of the matter and other circumſtances, the greenneſs was not attained to but at certain periods of time, now and then diſcloſing itſelf within two or three days, and ſometimes not before nine or ten.]

WITH how little confidence of ſucceſs trials, that have the aims of theſe I have been ſpeaking of, are to be attempted, not only conſideration but experience have made me ſenſible. But yet I would not diſcourage men's curioſity from venturing even upon ſlight probabilities, where the nobleneſs of the ſubjects and ſcope may make even ſmall attainments very deſirable. And till trial have been made on occaſions of great moment, it is not eaſy to be ſatisfied, that men have not been wanting to themſelves; which I ſhall only illuſtrate by propoſing what, I preſume, will not need, that I ſhould make an application of it. Thoſe adventurous navigators, that have made voyages for diſcovery in unknown ſeas, when they firſt diſcerned ſomething obſcure near the horizon, at a great diſtance off, have often doubted, whether what they had ſo imperfect a ſight of, were a cloud, or an iſland, or a mountain: but though ſometimes it were more likely to be the former, as that, which more frequently occurred, than the latter; yet they judged it adviſable to ſteer towards it, till they had a clearer proſpect of it: for if it were a deluding meteor, they would not however ſuſtain ſo great a loſs in that of a little labour, as, in caſe it were a country, they would in the loſs of what might prove a rich diſcovery: and if they deſiſted too ſoon from their curioſity, they could not rationally ſatisfy themſelves, whether they ſlighted a cloud, or neglected a country.

SOME ADDITIONAL

EXPERIMENTS

RELATING TO THE

SUSPICIONS about the HIDDEN QUALITIES of the AIR.

THE essay about Suspicions of some Hidden Qualities of the Air having been detained somewhat long at the press, that it might come abroad accompanied with the other tracts designed to attend it, whilst I was rumaging among several papers to look for some other things, I met now and then with an experiment or observation, that seemed to relate to some of the things delivered in that tract; and though they be in themselves of no great moment, I am content to annex them to the rest, because, as in that company they may signify somewhat, so I am unwilling, that any matter of fact, relating to such a subject, should perish to save the labour of transcribing.

EXPERIMENT I.

HAVING occasion to dulcify some calx of Dantzick vitriol, from which the oil had been a good while before distilled; water was put upon two large portions of it, that the liquor might be impregnated with the vitriolate particles remaining in the calx; the water put upon one of these portions was, soon after it was sufficiently impregnated, filtrated and gently abstracted, by which means it afforded many drachms of a kind of salt of vitriol, that seemed to differ very little from the vitriol, that had been calcined: but the water, that was put upon the other portion of calcined vitriol, was in a wide-mouthed vessel left in the air for a month or six weeks; after which time, when it came to be abstracted after the manner formerly recited, it afforded many drachms of a salt, that did not then, nor long after, look at all like common vitriol, or like the other, but shot white almost like salt-petre, or some other untincted salt. Whether this experiment will constantly succeed, and at other seasons of the year than that it was made in, which was summer, I had not the opportunity to make a full trial, though I endeavoured it. But that the air may have a great stroke in varying the salts obtainable from calcined vitriol, seemed the more probable, because we had some colcothar, that had lain many months, if not some years, in the air, but in a place sheltered from the rain; and having caused a lixivium to be made of it, to try what sort or plenty of saline particles it would yield, we found, when the superfluous moisture was exhaled, that they began to shoot into salt far more white than vitriol, and very differing from it in its figure and way of concretion.

EXPERIMENT II.

WE took colcothar of venereal vitriol carefully dulcified, and leaving it in my study in the months of *January* and *Febuary*, by weighing it carefully before an ounce of it was exposed to the air, and after it had continued there some weeks, we found it to have encreased in weight four grains and about a quarter, besides some little dust, that stuck to the glass. This was made at *Oxford*.

THIS slight experiment is here mentioned, that, being compared with the next ensuing trial, it may appear, that the difference of airs, seasons, calces of vitriol, or other circumstances, may produce a notable disparity in the increment of weight, the exposed bodies gain in the air.

EXPERIMENT III.

WE put eight ounces of outlandish vitriol, calcined to a deep redness, into a somewhat broad and flat metalline vessel, and set it by upon a shelf, in a study, that was seldom frequented; and at the same time, that we might observe what increment would be gained by exposing to the air a larger superficies of the powder, in reference to the bulk, we put into another metalline vessel, smaller than the other, only two ounces of colcothar, and set it on the same shelf with the other, this was done at the vernal equinox, (the twelfth of *March*;) on the twenty-fifth of *June* we weighed these powders again, and found the eight ounces to have gained one drachm and seventeen grains; but the two ounces had acquired the same weight within a grain: then putting them back into their former vessels, we left them in the same place as formerly, till the twenty-fourth of *August*, when we found cause to suppose, that the greater parcel of colcothar had met with some mischance, either by mice or otherwise; but the lesser parcel weighed twenty-six grains heavier than it did in *June*, amounting now to two ounces, one dram, forty-two grains, having increased, in less than six months, above an hundred grains, and consequently above a tenth part of its first weight.

No

No trial was made to discover, what this acquired substance may be, that we might not disturb the intended prosecution of the experiment.

EXPERIMENT IV.

BECAUSE in most of the experiments of substances exposed to be impregnated by the air, or detain its saline or other exotic particles, we employed bodies prepared and much altered by the previous operation of the fire; we thought fit to make some trials with bodies unchanged by the fire; and to this purpose we took a marchasite, which was partly of a shining and partly of a darkish colour, and which seemed well-disposed to afford vitriol: of this we took several smaller lumps, that amounted to two ounces; these were kept in a room, where they were freely accessible to the air, which, by reason that the house, that was seated in the country, stood high, was esteemed to be very pure. After the marchasites had been kept in this room somewhat less than seven weeks, we weighed them again in the same balance, and found the two ounces to have gained above twelve grains in weight.

EXPERIMENT V.

THE experiment used at the latter end of our paper, about celestial and aerial magnets, seeming to some virtuosi very strange, and the way, that I employed in making that liquor, that turns green in the air, being somewhat troublesome, I remember I thought fit to try, upon the same ground, a way of producing the same phænomenon more easy and more expeditious. And though perhaps this way will not succeed so constantly, nor always so well as the other, yet, for its easiness and cheapness, it will not probably be unwelcome to those, that are desirous to see the odd phænomenon.

WE took then, more than once, filings of clean crude copper, and having put on them a convenient quantity of good spirit of salt, we suffered the menstruum in heat (which need not be very great) to work upon the metal, which it usually does slowly, and not like aqua fortis. When the liquor had by this operation acquired a thick and muddy colour, we decanted it into a clean glass with a wide mouth, which being left for a competent time in the open air, the exposed liquor came to be of a fair green, though it did not appear, that any thing was precipitated at the bottom, to make it clear.

EXPERIMENT VI.

PERHAPS it may not be impertinent to add, that I once or twice observed the fumes of a sharp liquor to work more quickly or manifestly on a certain metal sustained in the air, than did the menstruum itself, that emitted those fumes on those parts of the metal, that it covered. And this brings into my mind, that, asking divers questions of a chemist, that had been in *Hungary*, and other parts, purposely to see the mines; he answered me, among other things, that, as to the ladders and other wooden work employed in one or more of the deep Hungarian mines, those, that were in the upper part of the grooves, any thing near the external air, would, by the fretting exhalations, be rendered unserviceable, in not many months; whereas those ladders, and pieces of timber, &c. that were employed in the lower part of the mine, would hold good for two or three times as long.

EXPERIMENT VII.

WE took, about the bigness of a nutmeg, of a certain soft but consistent body, that we had caused to be chemically prepared, and which, in the free air, would continually emit a thick smoke: this being put into a vial, and placed in a middle-sized receiver in our engine, continued for some time to afford manifest fumes, whilst the exhaustion was making; till at length, the air having been more and more pumped out, the visible ascension of fumes out of the vial quite ceased; and the matter having remained some time in this state, the smoking substance was so altered, that it would not emit fumes, not only when the air was let into the receiver, but not in a pretty while after the vial was taken out of it, till it had been removed to the window, where the wind blowing in fresh and fresh air, it began to smoke as formerly.

THE other phænomena of this experiment belong not to this place; but there are two, which will not be impertinent here, and the latter of them may deserve a serious reflection.

THE first of them was, that the substance hitherto mentioned had been kept in a large glass, wherein it had been distilled at least five or six weeks, and yet would smoke very plentifully upon the contact of the air, and be kept from smoking, though the chemical receiver were stopped but with a piece of paper.

THE second was, that, when the vial was put unstopped in the receiver, and the receiver close luted on, though no exhaustion were made, yet the white fumes did very quickly cease to ascend into the receiver, as if this smoke participated of the nature of flame, and presently glutted the air, or otherwise made it unfit (and yet without diminution of its gravity) to raise the body, that should ascend.

ANIMADVERSIONS UPON Mr. *HOBBES*'s PROBLEMATA DE VACUO.

PREFACE.

UPON the coming abroad of Mr. *Hobbes*'s *Problemata Physica*, finding them in the hands of an ingenious person, that intended to write a censure of them, which several employments, private and publick, have, it seems, hindered him to do, I began, as is usual on such occasions, to turn over the leaves of the book, to see what particular things it treated of. This I had not long done, before I found, by obvious passages in the third chapter, or dialogue, as well as by the title, which was *Problemata de Vacuo*, that I was particularly concerned in it; upon which I desired the possessor of the book, who readily consented, to leave me to examine that dialogue, on which condition I would leave him to deal with all the rest of the book. Nor did I look upon the reflections I meant to make, as repugnant to the resolutions I had taken against writing books of controversy, since the explications, Mr. *Hobbes* gave of his problems, seemed to contain but some variations of, or an appendix to, his tract *De Natura Aeris*, which, being one of the two first pieces, that were published against what I had written, was one of those, that I had expresly reserved myself the liberty to answer. But the animadversions I first made upon Mr. *Hobbes*'s Problems *de Vacuo*, having been casually mislaid ere they were finished; before I had occasion to resume my task, there passed time enough to let me perceive, that his doctrine, which it will easily be thought, that the vacuists disapproved, was not much relished by most of the Plenists themselves, the modernest Peripateticks, and the Cartesians; each of them maintaining the fullness of the world, upon their own grounds, which are differing enough from those of our author, the natural indisposition I have to polemical discourses, easily persuaded me to let alone a controversy, that did appear needful. And I had still persisted in my silence, if Mr. *Hobbes* had not, as it were, summoned me to break it by publishing again his explications, which in my Examen of his dialogue *De Natura Aeris*, I had shewn to be erroneous.

AND I did not grow at all more satisfied, to find him so constant, as well as stiff an adversary to interspersed vacuities, by comparing what he maintains in his dialogue *De Vacuo*, with some things, that he teaches, especially concerning God, the cause of motion, and the imperviousness of glass, in some other of his writings, that are published in the same volume with it. For since he asserts, that there is a God, and owns Him to be the Creator of the World; and since, on the other side, the penetration of dimensions is confessed to be impossible, and he denies, that there is any vacuum in the universe; it seems difficult to conceive, how in a world, that is already perfectly full of body, a corporeal Deity, such as he maintains in his *Append. ad Leviath. cap.* 3. can have that access, even to the minute parts of the mundane matter, that seems requisite to the attributes and operations, that belong to the Deity, in reference to the world. But I leave divines to consider, what influence the conjunction of Mr. *Hobbes*'s two opinions, the corporeity of the Deity, and the perfect plenitude of the world, may have on theology. And perhaps, I should not in a physical discourse have taken any notice of the proposed difficulty, but that, to prevent an imputation on the study of nature's works, (as if it taught us rather to degrade, than admire their author,) it seemed not amiss to hint (*in transitu*) that Mr. *Hobbes*'s gross conception of a corporeal God is not only unwarranted by sound philosophy, but ill-befriended even by his own.

MY adversary having proposed his problems by way of dialogue between *A.* and *B*; it will not, I presume, be wondered at, that I have given the same form to my animadversions; which come forth no earlier, because I had divers other treatises, that I was more concerned for, to publish before them.

BUT, because it will probably be demanded, why, in a tract, that is but short, my animadversions should take up so much room? it will be requisite, that I here give an account of the bulk of this treatise.

AND first, having found, that there was not any one problem, in whose explication, as proposed by Mr. *Hobbes*, I saw cause to acquiesce, I was induced, for the reader's ease, and that I might be sure to do my adversary no wrong, to transcribe his whole dialogue, bating some few transitions, and other clauses not needful to be transferred hither.

NEXT, I was not willing to imitate Mr. *Hobbes*, who recites in the dialogue we are considering the same experiments, that he had already mentioned in his tract *De Natura Aeris*, without adding as his own (that I remember) any new one to them. But my unwillingness to tire the reader with bare repetitions of the arguments I employed in my Examen of that tract, invited me to endeavour to make him some amends for the exercise of his patience by inserting, as occasion was offered, five or six new experiments, that will not perhaps be so easily made by every reader, that will be able (now that I have perspicuously proposed them) to understand them.

AND lastly, since Mr. *Hobbes* has not been content to manage himself and his way of treating of physical matters, but has been pleased to speak very slightingly of experimentarian philosophers (as he stiles them) in general, and, which is worse, to disparage the making of elaborate experiments; I judged the thing, he seemed to aim at, so prejudicial to true and useful philosophy, that I thought it might do some service to the less knowing, and less wary sort of readers, if I tried to make his own explications enervate his authority, and by a somewhat particular Examen of the solutions he has given of the problems I am concerned in, shew, that it is much more easy to undervalue a frequent recourse to experiments, than truly to explicate the phænomena of nature without them. And since our author, speaking of his Problemata Physica, (which is but a small book) scruples not to tell his majesty, to whom he dedicates them, that he has therein comprised (to speak in his own terms) the greatest and most probable part of his physical meditations; and since by the alterations, he has made in what he formerly writ about the phænomena of my engine, he seems to have designed to give it a more advantageous form; I conceive, that by these selected solutions of his, one may, without doing him the least injustice, make an estimate of his way of discoursing about natural things. And though I would not interest the credit of experimentarian philosophers in no considerabler a paper than this; yet if Mr. *Hobbes*'s explication and mine be attentively compared, it will not, I hope, by them be found, that the way of philosophising he employs is much to be preferred before that, which he undervalues.

* *Credo,* (says Mr. *Hobbes* in his *Dialogus Physicus:*) *Nam motus hic Resti tutionis Hobbii est, & ab illo primo & solo explicatus in Lib. de Corpore, cap.* 21. *Art* 1. *Sine qua Hypothesi, quantuscunque labor, ars, sumptus, ad rerum Naturalium invisibiles causas inveniendus adhibeatur, frustra erit.* And speaking of the Gentlemen (to whom it were not here proper for me to give Epithets) that used to meet at *Gresham-College*, and are known by the Name of the *Royal-Society*, he thus treats them and their way of inquiring into Nature: *Conveniant, studia conferant, Experimenta faciant quantum volunt, nisi & Principiis utantur meis, nihil proficient.*

A. *Fateris ergò nihil hactenus à Collegis tuis promotam esse scientium Causarum Naturalium, nisi quod unus eorum Machinam invenerit, quâ motus excitari Aeris possit talis, ut partes Sphæræ simul undiquaque tendant ad Centrum, & ut Hypotheses Hobbianæ, antè quidem satis probabiles, hinc reddantur probabiliores.*

B. *Nec fateri pudet, nam est aliquid prodire tenus, si non datur altra.*

A. *Quid tenus? quorsum autem tantus apparatus & sumptus Machinarum factu difficilium, ut eatenus tantum prodiretis, quantum ante prodierat Hobbius? Cur non inde potiùs incepistis, ubi ille desiit? Cur principiis ab illo positis non estis usi? Cumque Aristoteles rectè dixit, ignorato motu ignorari Naturam, &c.*

— *Ad Causas autem, propter quas proficere ne paululum quidem potuistis, nec poteritis, accedunt etium aliæ, ut odium Hobbii, &c.*

ANIMADVERSIONS, &c.

A. MAY, one, without too bold an inquisitiveness, ask, what book you are reading so attentively?

B. YOU will easily believe you may, when I shall have answered you, that it was Mr. *Hobbes*'s lately published tract of physical problems, which I was perusing.

A. WHAT progress have you made in it?

B. I was finishing the third dialogue or chapter, when you came in, and finding myself, though not named, yet particularly concerned, I was perusing it with that attention, which it seems you take notice of.

A. DIVERS of your experiments are so expressly mentioned there, that one need not be skilled in decyphering to perceive, that you are interested in that chapter; and therefore seeing you have heedfully read it over, pray give me leave to ask your judgment, both of Mr. *Hobbes*'s opinion, and his reasonings about a vacuum.

B. CONCERNING his opinion, I am sorry I cannot now satisfy your curiosity, having long since taken, and ever since kept a resolution to decline, at least, until a time, that is not yet come, the declaring myself either for or against the Plenists. But as to the other part of your question, which is about Mr. *Hobbes*'s arguments for the absolute plenitude of the world, I shall not scruple readily to answer, that his ratiocinations seem to me far short of that cogency, which the noise he would make in the world, and the way, wherein he treats both ancient and modern philosophers, that dissent from him, may warrant us to expect.

A. YOU will allow me the freedom to tell you, that, to convince me, that your resentment of his explicating divers of the phænomena of your pneumatick engine otherwise than you have been wont to do, (and perhaps in terms, that might well have been more civil) has had no share in dictating this judgment of yours; the best way will be, that entering for a while into the party of the vacuists, you answer the arguments he alledges in his chapter to confute them.

B. HAVING always, as you know, forborn to declare myself either way in this controversy, I shall not tye myself strictly to the principles and notions of the vacuists, nor, though but for a while, oppose myself to those of the Plenists: but so far I shall comply with your commands, as either upon the doctrine of the vacuists, or upon other grounds, to consider, whether this dialogue of Mr. *Hobbes* have cogently proved his, and the schools assertion, *Non dari vacuum*; and whether he has rightly explained some phænomena of nature, which he undertakes to give an account of, and especially some produced in our engine, whereof he takes upon him to render the genuine causes. And this last inquiry is that, which I chiefly design.

A. BY this I perceive, that if you can make out your own explications of your adversary's problems *de vacuo*, and shew them to be preferable to his, you will think you have done your work; and that it is but your secondary scope to shew, that in Mr. *Hobbes*'s way of solving them, he gives the vacuists an advantage against him, though not against the Plenists in general.

B. YOU do not mistake my meaning, and therefore without any further preamble, let us now proceeed to the particular phænomena considered by Mr. *Hobbes*; the first of which is an experiment proposed by me in the one and thirtieth of the physico-mechanical experiments concerning the adhesion of two flat and polished marbles, which I endeavoured to solve by the pressure of the air. And this experiment Mr. *Hobbes* thinks so convincing an one to prove the plenitude of the world, that, though he tells us he has many cogent arguments to make it out, yet he mentions but this one, because that, he says, suffices.

A. THE confidence he thereby expresses of the great force of the argument does the less move me, because I remember, that formerly in his elements of philosophy he thought it sufficient to employ one argument to evince the plenitude of the world, and for that one he pitched upon the vulgar experiment of a gardener's watering-pot: but whether he were wrought upon by the objections made to his inference from that phænomenon in your examen of his dialogue *De Natura Aeris*, or by some other considerations, I will not pretend to divine. But I plainly perceive, he now prefers the experiment of the cohering marbles.

B. OF which it will not be amiss, though the passage be somewhat long, to read you his whole discourse out of the book I have in my hand.

A. IT is fit, that you, who for my sake are content to take the pains of answering what he says, should be eased of the trouble of reading it, which I will therefore, with your leave, take upon me. His discourse then about the marbles is this:

A. AD *probandam universi plenitudinem, nullum nostin' argumentum cogens?*

B. IMO *multa: unum autem sufficit ex eo sumptum, quod duo corpora plana, si se mutuò secundùm amborum planitiem communem tangant, non facile in instante divelli possunt; successivè verò facillimè. Non dico, impossibile esse duo durissima marmora ita cohærentia divellere, sed difficile; & vim postulare tantam, quanta sufficit ad duritiem lapidis superandam. Siquidem verò majore vi ad separationem opus sit quàm illa, quâ moventur separata, id signum est, non dari vacuum,*

A. ASSERTIONES *illæ demonstratione indigent. Primò autem ostende, quomodo ex duorum durissimorum corporum, conjunctorum ad superficies exquisite læves, diremptione difficili, sequatur plenitudo mundi?*

A. SI *duo plana, dura, polita corpora (ut marmora) collocentur unum supra alterum, ita ut eorum superficies se mutuò per omnia puncta exactè, quantum fieri potest, contingant, illa sine magna difficultate ita divelli non possunt, ut eodem instante per omnia puncta dirimantur. Veruntamen marmora eadem, si communis eorum superficies ad horizontem erigatur, aut non valde inclinetur, alterum ab altero facillimè (ut scis) etiam solo pondere dilabentur. Nonne causa hujus rei hæc est, quod labenti marmori succedit aer, & relictum locum semper implet?*

A. CERTISSIME. *Quid ergo?*

B. QUANDO *verò eadem uno instante divellere conaris, nonne multo major vis adhibenda est; quam ob causam?*

A. EGO, *& mecum (puto) omnes causam statuunt, quod spatium totum inter duo illa marmora divulsa simul uno instante implere aer non potest, quantacunque celeritate fiat divulsio.*

B. AN *qui spatia in aere dari vacua contendunt, in illo aere solo dari negant, qui marmora illa conjuncta circumdat?*

A. MINIME, *sed ubique interspersa.*

B. DUM *ergo illi, qui marmor unum ab altero revellentes aerem comprimunt, & per consequens vacuum exprimunt, vacuum faciunt locum per revulsionem relictum; nulla ergo separationis erit difficultas, saltem non major, quàm est difficultas corpora eadem movendi in aere, postquam separata fuerint. Itaque quoniam, concesso vacuo, difficultas marmora illa dirimendi nulla est, sequitur per difficultatis experientiam, nullum esse vacuum.*

A. RECTE *quidem illud infers. Mundi autem plenitudine supposita, quomodo demonstrabis possibile omnino esse, ut divellantur?*

B. COGITA *primo corpus aliquod ductile, nec nimis durum, ut ceram, in duas partes distrahi, quæ tamen partes non minus exactè in communi plano se mutuo tangunt quàm lævissima marmora. Jam quo pacto distrahatur cera, consideremus. Nonne perpetuo attenuatur, donec in filum evadat tenuissimum, & omni dato crasso tenuius, & sic tandem divellitur? Eodem modo etiam durissima columna in duas partes distrahetur, si vim tantam adhibeas, quanta sufficit ad resistentiam duritiei superandam. Sicut enim in*

cera

cera partes primò extimæ distrahuntur, in quarum locum succedit aer; ita etiam in corpore quantumlibet duro aer locum subit partium extimarum, quæ primæ vulsionis viribus dirumpuntur. Vis autem, quæ superat resistentiam partium extimarum duri, facilè superabit resistentiam reliquarum. Nam resistentia prima est à toto duro, reliquarum verò semper à residuo.

A. Ita *quidem videtur consideranti, quàm corpora quædam, præsertim verò durissima, fragilia sint.*

Does this ratiocination seem to you as cogent, as it did to the proposer of it?

B. You will quickly think it does not; and perhaps you would think it should not, if you please to consider with me some of the reflections, that the reading of it suggested to me.

And first, without declaring for the vacuist's opinion, I must profess myself unsatisfied with Mr. *Hobbes*'s way of arguing against them: for, where he says, *Dum ergo illi, qui marmor unum ab altero revellentes aerem comprimunt, & per consequens vacuum exprimunt, vacuum faciunt locum per revulsionem relictum; nulla ergo separationis erit difficultas, saltem non major, quàm est difficultas corpora eadem movendi in aere, postquam separata fuerint. Itaque quoniam, concesso vacuo, difficultas marmora illa dirimendi nulla est, sequitur per difficultatis experientiam, nullum esse vacuum.* Methinks he expresses himself but obscurely, and leaves his readers to guess, what the word *dum* refers to. But that, which seems to be his drift in this passage, is, that, since the vacuists allow interspersed vacuities, not only in the air, that surrounds the conjoined marbles, but in the rest of the ambient air, there is no reason, why there should be any difficulty in separating the marbles, or at least any greater difficulty than in moving the marbles in that air after their separation. But, not to consider, whether his adversaries will not accuse his phrase of squeezing out a vacuum, as if it were a body, they will easily answer, that notwithstanding the vacuities they admit in the ambient air, a manifest reason may be given in their hypothesis of our finding a difficulty in the divulsion of the marbles. For, the vacuities they admit, being but interspersed, and very small, and the corpuscles of the atmosphere being, according to them, endowed with gravity, there lean so many upon the upper surface of the uppermost marble, that that stone cannot be at once perpendicularly drawn up from the lower marble contiguous to it, without a force capable to surmount the weight of the aerial corpuscles, that lean upon it. And this weight has already so constipated the neighbouring parts of the ambient air, that he, that would perpendicularly raise the upper marble from the lower, shall need a considerable force to make the revulsion, and compel the already contiguous parts of the incumbent air to a subingression into the pores or intervals, intercepted between them. For the conatus of him, that endeavours to remove the upper marble, whilst the lower surface of it is fenced from the pressure of the atmosphere, by the contact of the lower marble, which suffers no air to come in between them, is not assisted by the weight or pressure of the atmosphere, which, when the marbles are once separated, pressing as strongly against the undermost surface of the upper marble, as the incumbent atmospherical pillar does against the upper surface of the same marble, the hand, that endeavours to raise it in the free air, has no other resistance, than that small one of the marble's own weight to surmount.

A. But what say you to the reason, that Mr. *Hobbes*, and, as he thinks, all others give of the difficulty of the often-mentioned divulsion? namely, *Quòd spatium totum inter duo illa marmora divulsa simul uno instante implere aer non potest, quantacunque celeritate fiat divulsio.*

B. I say, that, for aught I know, the plenists may give a more plausible account of this experiment, than Mr. *Hobbes* has here done; and therefore, abstracting from the two opposite hypotheses, I shall further say, that the genuine cause of the phænomenon seems to be that, which I have already assigned; and that difficulty of raising the upper stone, that accompanies the air's not being able to come in all at once, to possess the space left between the surfaces of the two marbles upon their separation, proceeds from hence, that, till that space be filled with the atmospherical air, the hand of him, that would lift up the superior marble, cannot be fully assisted by the pressure of the air against the lower surface of that marble.

A. This is a paradox, and therefore I shall desire to know on what you ground it.

B. Though I mention it but as a conjecture proposed *ex abundanti*, yet I shall on this occasion countenance it with two things; the first, that, since I declare not for the hypothesis of the Plenists, as it is maintained by Mr. *Hobbes*, I am not bound to allow, what the common explication, adopted by my adversary, supposes; namely, that either nature abhors a vacuum (as the schools would have it,) or that there could be no divulsion of the marbles, unless, at the same time, the air were admitted into the room, that divulsion makes for it. And a vacuist may tell you, that, provided the strength employed to draw up the superior marble be great enough to surmount the weight of the aerial corpuscles accumulated upon it, the divulsion would ensue, though, by divine Omnipotence, no air, or other body, should be permitted to fill the room made for it by the divulsion; and that the air's rushing into that space does not necessarily accompany, but in order of nature and time follow upon, a separation of the marbles, the air, that surrounded their contiguous surfaces, being by the weight of the collaterally superior air, impelled into the room newly made by the divulsion. But I shall rather countenance what you call my paradox, by an experiment I purposely made in our pneumatical receiver, where, having accommodated two flat and polished marbles, so that the lower being fixed, the upper might be laid upon it, and drawn up again as there should be occasion, I found, that if, when the receiver was well exhausted, the upper marble was, by a certain contrivance, laid

laid flat upon the lower, they would not then cohere as formerly, but be with great eaſe ſeparated, though it did not, by any phænomenon appear, that any air could come to ruſh in, to poſſeſs the place given it, by the receſs of the upper marble, whoſe very eaſy avulſion is as eaſily explicable by our hypotheſis; ſince the preſſure of that little air, that remained in the receiver, being too faint to make any at all conſiderable reſiſtance to the avulſion of the upper marble, the hand, that drew it up, had very little more than the ſingle weight of the ſtone to ſurmount.

A. An Anti-pleniſt had expected, that you would have obſerved, that the difficult ſeparation of the marbles in the open air does rather prove, that there may be a vacuum, than that there can be none. For in caſe the air can ſucceed as faſt at the ſides, as the divulſion is made, a vacuiſt may demand, whence comes the difficulty of the ſeparation? And if the air cannot fill the whole room made for it by the ſeparated marbles, at the ſame inſtant they are forced aſunder, how is a vacuum avoided for that time, how ſmall ſoever, that is neceſſary for the air to paſs from the edges to the middle of the room newly made?

B. What the Pleniſts will ſay to your argument, I leave them to conſider; but I preſume, they will be able to give a more plauſible account of the phænomenon we are treating of, than is given by Mr. *Hobbes.*

A. What induces you to diſlike his explication of it?

B. Two things; the one, that I think the cauſe he aſſigns improbable; and the other, that I think another, that is better, has been aſſigned already.

And firſt, whereas Mr. *Hobbes* requires to the divulſion of the marbles a force great enough to ſurmount the hardneſs of the ſtone, this is aſſerted gratis, which it ſhould not be; ſince it ſeems very unlikely, that the weight of ſo few pounds, as will ſuffice to ſeparate two coherent marbles of about an inch, for inſtance, in diameter, ſhould be able to ſurmount the hardneſs of ſuch ſolid ſtones, as we uſually employ in this experiment. And though it be generally judged more eaſy to bend, if it may be, or break a broader piece of marble *cæteris paribus*, than a much narrower; yet, whereas neither I, nor any elſe that I know, nor I believe Mr. *Hobbes*, ever obſerved any difference in the reſiſtance of marbles to ſeparation from the greater or leſſer thickneſs of the ſtones; I find by conſtant experience, that, *cæteris paribus*, the broadneſs of the coherent marble does exceedingly encreaſe the difficulty of disjoining them: inſomuch, that, whereas not many pounds, as I was ſaying, would ſeparate marbles of an inch, or a leſſer diameter; when I encreaſed their diameter to about four inches, if I miſ-remember not, there were ſeveral men, that ſucceſſively tried to pull them aſunder, without being able by their utmoſt force to effect it.

A. But what ſay you to the illuſtration, that Mr. *Hobbes*, upon the ſuppoſition of the world's plenitude, gives of our phænomenon, by drawing aſunder the oppoſite parts of a piece of wax?

B. To me it ſeems an inſtance improper enough. For firſt, the parts, that are to be divided in the wax, are of a ſoft and yielding conſiſtence, and according to him, of a ductile, or, if you pleaſe, of a tractile nature, and not, as the parts of the coherent marbles, very ſolid and hard. Next, the parts of the wax do not ſtick together barely by a ſuperficial contact of two ſmooth planes, as do the marbles we are ſpeaking of; but have their parts implicated, and as it were intangled with one another. And therefore, they are far from a diſpoſition to ſlide off, like the marbles, from one another, in how commodious a poſture ſo ever you place them. Beſides, it is manifeſt, that the air has opportunity to ſucceed in the places ſucceſſively deſerted by the receding parts of the attenuated wax; but it is neither manifeſt, nor as yet well proved, by Mr. *Hobbes*, that the air does after the ſame manner ſucceed between the two marbles, which, as I lately noted, are not forced aſunder after ſuch a way, but are, as himſelf ſpeaks, ſevered in all their points at the ſame inſtant.

A. I know, you forget not what he ſays, of the dividing of a hard column into two parts, by a force ſufficient to overcome the reſiſtance of its hardneſs.

B. He does not here either affirm, that he, or any he can truſt, has ſeen the thing done; nor does he give us any ſuch account of the way wherein the pillar is to be broken, whether in an erected, inclined, or horizontal poſture; nor deſcribe the particular circumſtances, that were fit to be mentioned in order to the ſolution of the phænomenon. Wherefore, till I be better informed of the matter of fact, I can ſcarce look upon what Mr. *Hobbes* ſays of the pillar, as other than his conjecture, which now I ſhall the rather paſs by, not only becauſe the caſe is differing from that of our poliſhed marbles, which are actually diſtinct bodies, and only contiguous in one commiſſure; but alſo, becauſe I would haſten to the ſecond reaſon of my diſlike of Mr. *Hobbes*'s explication of our phænomenon, which is, that a better has been given already, from the preſſure of the atmoſphere upon all the ſuperficial parts of the upper marble, ſave thoſe, that touch the plane of the lower.

A. You would have put fair for convincing Mr. *Hobbes* himſelf, at leaſt would have put him to unuſual ſhifts, if you had ſucceeded in the attempt you made, among other of your phyſico-mechanical experiments, to disjoin two coherent marbles, by ſuſpending them horizontally in your pneumatical receiver, and pumping out the air, that environed them; for, from your failing in that attempt, though you rendered a not improbable reaſon of it, Mr. *Hobbes* took occaſion, in his dialogue *De Natura Aeris*, to ſpeak in ſo high a ſtrain as this: *Nihil iſthic erat, quod ageret pondus; experimento hoc excogitari contra opinionem eorum, qui vacuum aſſerunt, aliud argumentum fortius aut evidentius non potuit. Nam ſi duorum cohærentium alterutrum ſecundùm eam viam, in qua jacent*

cent ipsæ contiguæ superficies, propulsum esset, facile separarentur, Aere proximo in locum relictum successivè semper influente; sed illa ita divellere, ut simul totum amitterent contactum, impossibile est, mundo pleno. Oporteret enim aut motum fieri ab uno termino ad alium in instante, aut duo corpora eodem tempore in eodem esse loco: quorum utrumvis dicere, est absurdum.

B. You may remember, that where I relate that experiment, I expressed a hope, that, when I should be better accommodated than I then was, I might attempt the trial with prosperous success; and accordingly afterwards, having got a lesser engine than that I used before, wherewith the air might be better pumped out, and longer kept out, I cheerfully repeated the trial. To shew then, that when two coherent marbles are sustained horizontally in the air, the cause, why they are not to be forced asunder, if they have two or three inches in diameter, without the help of a considerable weight, is the pressure I was lately mentioning of the ambient air; I caused two such coherent marbles to be suspended in a large receiver, with a weight at the lowermost, that might help to keep them steady, but was very inconsiderable to that, which their cohesion might have surmounted; then causing the air to be pumped by degrees out of the receiver, for a good while the marbles stuck close together, because, during that time, the air could not be so far pumped out, but that there remained enough to sustain the small weight, that endeavoured their divulsion: but when the air was further pumped out, at length the spring of the little, but not a little expanded air, that remained, being grown too weak to sustain the lower marble and its small clog, they did, as I expected, drop off.

A. This will not agree over-well with the confident and triumphant expressions just now recited.

B. I never envied Mr. *Hobbes*'s forwardness to triumph, and am content, his conjectures be recommended by the confidence, that accompanies them, if mine be by the success, that follows them. But to confirm the explication given by me of our phænomenon, I shall add, that as the last mentioned trial, which I had several times occasion to repeat, shews, that the cohesion of our two contiguous marbles would cease upon the withdrawing of the pressure of the atmosphere; so by another experiment I made, it appears, that the supervening of that pressure sufficed to cause that cohesion. For, in prosecution of one of the lately mentioned trials, having found, that when the receiver was well exhausted, two marbles, though considerably broad, being laid upon one another after the requisite manner, their adhesion was, if any at all, so weak, that the uppermost would be easily drawn up from off the other; we laid them again one upon the other, and then letting the external air flow into the receiver, we found, according to expectation, that the marbles now cohered well, and we could not raise the uppermost, but accompanied with the lowermost. But I am sensible, I have detained you too long upon the single experiment of the marbles: And though I hope the stress Mr. *Hobbes* lays on it, will plead my excuse, yet, to make your patience some amends, I shall be the more brief in the other particulars, that remain to be considered in his dialogue *De Vacuo.* And it will not be difficult for me to keep my promise without injuring my cause, since almost all these particulars being but the same, which he has already alledged in his dialogue *De Natura Aeris*, and I soon after answered in my Examen of that dialogue, I shall need but to refer you to the passages, where you may find these allegations examined, only subjoining here some reflections upon those few and slight things, that he has added in his problems *De Vacuo.*

A. I may then, I suppose, read to you the next passage to that long one, you have hitherto been considering, and it is this: *Ad vacuum nunc revertor: Quas causas sine suppositione vacui redditurus es illorum effectuum, qui ostenduntur per Machinam illam, quæ est in Collegio* Greshamensi?

B. *Machina illa—*

A. Stop here, I beseech you a little, that, before we go any further, I may take notice to you of a couple of things, that will concern our subsequent discourse.

Whereof the first is, that it appears by Mr. *Hobbes*'s Dialogue about the Air, that the explications he there gave of some of the phænomena of the Machina Boyliana, were directed partly against the virtuosi, that have since been honoured with the title of the Royal Society, and partly against the author of that engine, as if the main thing therein designed were to prove a vacuum. And since he now repeats the same explications, I think it necessary to say again, that if he either takes the Society or me for professed vacuists, he mistakes, and shoots besides the mark; for, neither they nor I have ever yet declared either for or against a vacuum.

And the other thing I would observe to you, is, that Mr. *Hobbes* seems not to have rightly understood, or at least not to have sufficiently heeded in what chiefly consists the advantage, which the vacuists may make of our engine against him: for, whereas in divers places he is very solicitous to prove, that the cavity of a pneumatical receiver is not altogether empty, the vacuists may tell him, that since he asserts the absolute plenitude of the world, he must, as indeed he does, reject not only great vacuities, but also those very small and interspersed ones, that they suppose to be intercepted between the solid corpuscles of other bodies, particularly of the air: so that it would not confute them to prove, that in our receiver, when most diligently exhausted, there is not one great and absolute vacuity, or, as they speak, a *vacuum coacervatum*, since smaller and disseminated vacuities would serve their turn. And therefore they may think their pretensions highly favoured, as by several particular effects, so by this general phænomenon of our engine, that it appears by several circumstances, that the common or atmospherical air, which

which, before the pump is set to work, possessed the whole cavity of our receiver, far the greatest part is by the intervention of the pump made to pass out the cavity into the open air, without being able, at least for a little while, to get in again; and yet it does not appear by any thing alledged by Mr. *Hobbes*, that any other body succeeds to fill adequately the places deserted by such a multitude of aerial corpuscles.

A. If I guess aright, by those words, (viz. " it appears not by any thing alledged by Mr. " *Hobbes*,") you design to intimate, that you would not in general prejudice the plenists.

B. Your conjecture was well founded: for I think divers of them, and particularly the Cartesians, who suppose a subtile matter or æther fine enough to permeate glass, though our common air cannot do it, have not near so difficult a task to avoid the arguments the vacuists may draw from our engine, as Mr. *Hobbes*, who, without having recourse to the porosity of glass, which indeed is impervious to common air, strives to solve the phænomena, and prove our receiver to be always perfectly full, and therefore as full at any one time as at any other of common or atmospherical air, as far as we can judge of his opinion by the tendency or import of his explications.

A. Yet, if I were rightly informed of an experiment of yours, Mr. *Hobbes* may be thereby reduced either to pass over to the vacuists, or to acknowledge some ætherial or other matter more subtil than air, and capable of passing through the pores of glass; and therefore, to shew yourself impartial between the vacuists and their adversaries in this controversy, I hope you will not refuse to gratify the plenists by giving your friends a more particular account of the experiment.

B. I know which you mean, and remember it very well. For, though I long since devised it, yet having but the other day had occasion to peruse the relation I writ down of one of the best trials, I think I can repeat it, almost in the very words, which if I mistake not, were these:

There was taken a bubble of thin white glass, about the bigness of a nutmeg, with a very slender stem, of about four or five inches long, and of the bigness of a crow's-quill. The end of the quill being held in the flame of a lamp blown with a pair of bellows, was readily and well sealed up, and presently the globous part of the glass, being held by the stem, was kept turning in the flame, until it was red hot and ready to melt: then being a little removed from the flame, as the included air began to lose of its agitation and spring, the external air manifestly and considerably pressed in one of the sides of the bubble. But the glass being again, before the cold could crack it, held as before in the flame, the rarified air distended and plumped up the bubble; which being the second time removed from the flame, was the second time compressed; and, being the third time brought back to the flame, swelled as before, and removed, was again compressed, (either this time or the last by two distinct cavities;) until at length, having satisfied ourselves, that the included air was capable of being condensed or dilated without the ingress or egress of air (properly so called) we held the bubble so long in the flame, strengthened by nimble blasts, that not only it had its sides plumped up, but a hole violently broken in it by the over-rarified air, which, together with the former watchfulness, we employed from time to time to discern, if it were any where cracked or perforated, satisfied us, that it was until then entire.

A. I confess, I did not readily conceive before, how you could, (as I was told you had,) make a solid vessel, wherein there was no danger of the air's getting in or out, whose cavity should be still possessed with the same air, and yet the vessel be made by turns bigger and lesser. And, though I presently thought upon a well stopped bladder, yet I well foresaw, that a distrustful adversary might make some objections, which are by your way of proceeding obviated; and the experiment agrees with your doctrine in shewing, how impervious we may well think your thick pneumatick receivers are to common air, since a thin glass bubble, when its pores were opened or relaxed by flame, would not give passage to the springy particles of the air, though violently agitated; for if those particles could have got out of the pores, they never would have broke the bubble, as at length a more violent degree of heat made them do; nor probably would the compression, that afterwards ensued, of the bubble by the ambient air, be checked near so soon, if those springy corpuscles had not remained within to make the resistance. Methinks, one may hence draw a new proof of what I remember you elsewhere teach, that the spring of the air may be much strengthened by heat. For, in our case, the spring of the air was thereby inabled to expand the compressed glass, it was imprisoned in, in spite of the resisting pressure of the external air; and yet, that this pressure was considerable, appears by this, that the weight of so small a column of atmospherical air, as could bear upon the bubble, was able to press in the heated glass, in spite of the resistance of its tenacity and arched figure.

B. Yet that, which I mainly designed in this experiment, was, (if I were able) to shew and prove at once, by an instance not liable to the ordinary exceptions, the true nature of rarefaction and condensation, at least of the air. For to say nothing of the Peripatetick rarefaction and condensation, strictly so called, which I scruple not to declare, I think to be physically inconceptible or impossible; it is plain by our experiment, that, when the bubble, after the glass had been first thrust in towards the center, was expanded again by heat, the included air possessed more room than before, and yet it could perfectly fill no more room than formerly, each aerial particle taking up, both before and after the heating of the bubble, a portion of space adequate to its own bulk; so that in the cavity of the expanded bubble we must admit either vacuities interspersed between the corpuscles of the air, or that some fine particles of the flame, or other subtil matter, came in to fill up to those intervals, which matter must have entered the cavity of the glass at its pores: and afterwards, when the red-hot bubble was removed from the flame, it is evident, that since the grosser particles of the air could not get through the glass, which they were not able to do, even when vehemently agitated by an ambient flame, the compression of the bubble, and the condensation of the air, which was necessarily consequent upon it, could not, supposing the plenitude of the world, be performed without squeezing out some of the subtile matter contained in the cavity of the bubble, whence it could not issue but at the pores of the glass: But I will no longer detain you from Mr. *Hobbes*'s explications of the *Machina Boyliana*; to the first of which you may now, if you please, advance.

A. The passage I was going to read, when you interrupted me, was this:

B. Machina *illa eosdem effectus producit, quos produceret in loco non magno magnus inclusus ventus.*

A. Quomodo *ingreditur istuc ventus? Machinam nosti cylindrum esse cavum, æneum, in quem protruditur cylindrus alius solidus ligneis, corio tectus, (quem suctorem dicunt) ita exquisitè congruens, ut ne minimus quidem aer inter corium & æs intrare (ut putant) possit.*

B. Scio, *& quò suctor facilius intrudi possit, foramen quoddam est in superiori parte cylindri, per quod aer (qui suctoris ingressum alioqui impedire possit) emittatur. Quod foramen aperire possunt, & claudere, quoties usus postulat. Est etiam in cylindri cavi recessu summo datus aditus aeri in globum concavum vitreum, quem etiam aditum claviculâ obturare & aperire possunt, quoties volunt. Denique in globo vitreo summo relinquitur foramen satis amplum, (claviculâ item claudendum & recludendum) ut in illum, quæ volunt, immittere possint, experiendi causâ.*

B. The imaginary wind, to which Mr. *Hobbes* here abscribes the effects of our engine, he formerly had recourse to in the thirteenth page of his Dialogue; and I have sufficiently answered that passage of it in a part of my Examen, to which I therefore refer you.

A. I presume, you did not overlook the comparison Mr. *Hobbes* annexes to what I last read out of the problems, since he liked the conceit so well, that we meet with it in his Dialogue *De Natura Aeris*. The words (as you see) are these: *Tota denique machina non multum differt, si naturam ejus spectes, à sclopeto ex sambuco, quo pueri se delectant, imitantes sclopetos militum, nisi quòd major sit, & majori arte fabricatus, & pluris constet.*

B. I could scarce, for the reason you give, avoid taking notice of it. And if Mr. *Hobbes* intended it for a piece of raillery, I willingly let it pass, and could more easily forgive him a more considerable attempt than this, to be revenged on an engine, that has destroyed several of his opinions: but if he seriously meant to make a physical comparison, I think he made a very improper one. For not to urge, that one may well doubt how he knows, that in the enclosed cavity of his pot-gun, there is a very vehement wind, since that does not necessarily follow from the compression of the encluded air: in Mr. *Hobbes*'s instrument, the air, being forcibly compressed, has an endeavour to expand itself, and when it is able to surmount the resistance of its prison, that part, that is first disjoined, is forcibly thrown downwards; whereas in our engine it appears by the passage lately cited of our Examen, that the air is not compressed, but expanded, in our receiver, and if an intercourse be opened, or the vessel be not strong enough, the outward air violently rushes in; and if the receiver chance to break, the fragments of the glass are not thrown outwards, but forcibly inwards.

A. So that, whether or no Mr. *Hobbes* could have pitched upon a comparison more suitable to his intentions, he might easily have employed one more suitable to the phænomena.

B. I presume, you will judge it the less agreeable to the phænomena, if I here subjoin an experiment, that possibly you will not dislike; which I devised to shew, not only that in our exhausted receivers there is no such strong endeavour outwards, as most of Mr. *Hobbes*'s explications of the things, that happen in them are built upon, but that the weight of the atmospherical air, when it is not resisted by the counterpressure of any internal air, is able to perform what a weight of many pounds would not suffice to do.

A. I shall the more willingly learn an experiment to this purpose, because in your receivers the rigidity of the glass keeps us from seeing, by any manifest change of its figure, whether, if it could yield without breaking, it would not be pressed in, as your hypothesis requires.

B. The desires to obviate that very difficulty, for their satisfaction, that had not yet penetrated the grounds of our hypothesis, made me think of employing, instead of a receiver, of glass, one of a stiff and tough, but yet somewhat flexible, metal. And accordingly, having provided a new pewter porringer, and whelmed it upside down upon an iron plate, fastened to the upper end of our pnematical pump, we carefully fastened, by cement, the orifice to the plate; and though the inverted vessel, by reason of its stiffness and thickness, and the convexity of its superficies, were strong enough to have supported a great weight without changing its figure; yet, as soon as, by an exsuction or two, the remaining part of the included air was brought to such a degree of expansion, that its weakened spring was able to afford but little assistance to the tenacity and firmness of the metal, the weight of the pillar of the incumbent atmosphere (which, by reason of the breadth of the vessel, was considerably wide also) did presently and notably depress the upper part of the porringer, both lessening its capacity and changing its figure; so that, instead of the convex surface, the receiver had before, it came to a concave one, which new figure was somewhat, though not much, increased by the further withdrawing of the included and already rarified air. The experiment succeeded also another common porringer of the same metal. But in such kind of vessels, made purposely of iron plates, it will sometimes succeed and sometimes not, according to the diameter of the vessel, and the thickness of the plate, which was sometimes strong enough, and sometimes too weak to resist the pressure of the incumbent air. And sometimes I found also, that the vessel would be thrust in, not at the top but side-ways, in case that side were the only part, that were made too thin to resist the pressure of the ambient; which phænomenon I therefore take notice of, that you may see, that that powerful pressure may be exercised laterally as well as perpendicularly.

PERHAPS this experiment, and that I lately recited of an hermetically sealed bubble, by their fitness to disprove Mr. *Hobbes*'s doctrine, may do somewhat towards the letting him see, that he might have spared that not over-modest and wary expression, where, speaking of the gentlemen, that meet at *Gresham-college*, (of whom I pretend not to be one of the chief) he is pleased to say, *Experimenta faciant quantum volunt, nisi principiis utantur meis, nihil proficient.* But let us, if you please, pass on to what he further alledges to prove, that the space in the exhausted receiver, which the vacuists suppose to be partly empty, is full of air. *Video*, (says A.) *si suctor trudatur usque ad fundum cylindri ænei, obturenturque foramina, secuturum esse, dum suctor retrahitur, locum in cylindro cavo relictum fore vacuum. Nam ut in locum ejus succedat aer, est impossibile.* To which B. answers, *Credo equidem, suctorem cum cylindri cavi superficie satis arctè cohærere ad excludendum stramen & plumam, non autem acrem neque aquam. Cogita enim, quod non ita accuratè congruerent, quin undiquaque interstitium relinqueretur, quantum tenuissimi capilli capax esset. Retracto ergo suctore, tantum impelleretur aeris, quantum viribus illis conveniret, quibus aer propter suctoris retractionem reprimitur, idque sine omni difficultate sensibili. Quanto autem interstitium illud minus esset, tantum ingrederetur aer velocius: vel si contactus sit, sed non per omnia puncta, etiam tunc intrabit aer, modò suctor majore vi retrahatur. Postremò, etsi contactus ubique exactissimus sit, vi tamen satis auctâ per cochleam ferream, tum corium cedet, tum ipsum æs; atque ita quoque ingredietur aer. Credin' tu, possibile esse duas superficies ita exactè componere, ut has compositas esse supponunt illi; aut corium ita durum esse, ut aeri, qui cochleæ ope incutitur, nihil omnino cedat? Corium, quanquam optimum, admittit aquam, ut ipse scis, si fortè fecisti unquam iter vento & pluvia* νόμενος καὶ ἀήμενος. *Itaque dubitare non potes, quin retractus suctor tantum aeris in cylindrum adeoque in ipsum recipiens incutiat, quantum sufficit ad locum semper relictum* perfectè *implendum. Effectus ergo, qui oritur à retractione suctoris, alius non est quàm ventus (inquam) vehementissimus, qui ingreditur undiquaque inter suctoris superficiem convexam, & cylindri ænei concavam, proceditque (versâ claviculâ) in cavitatem globi vitrei, sive (ut vocatur) recipientis.*

THE substance of this ratiocination having been already proposed by Mr. *Hobbes*, in his dialogue of the air, the eleventh page, I long since answered it in my Examen; and therefore I shall only now take notice *in transitu* of some slight, whether additions or variations, that occur in what you have been reading. And, first, I see no probability in what he *gratis* asserts, that so thick a cylinder of brass, as made the chief part of the pump of our engine, should yield to the sucker, that was moved up and down in it, though by the help of an iron rack. And whereas he adds, that the leather, that surrounds the more solid part of the sucker, would yield to such a force; it seems, that that compression of the leather should, by thrusting the solid parts into the pores, make the leather rather less, than more fit to give passage to the air. Nor would it however follow, notwithstanding Mr. *Hobbes*'s example, that, because a body admits water, it must be pervious to air; for I have several times, by ways elsewhere taught, made water penetrate the pores of bladders, and yet bladders resist the passage of the air so well, that even when air included in them was sufficiently rarified by heat, or by our engine, it was necessary for the air to break them before it could get out; which would not have been, if it could have escaped through their pores. What Mr. *Hobbes* inculcates here again concerning his *ventus vehementissimus*, you will find answered in the place of my Examen I lately directed you to.

A. WE may then proceed to Mr. *Hobbes*'s next explication, which he proposes in these terms:

A. CAUSAM *video nunc unius ex machinæ mirabilibus, nimirum cur suctor, postquam est aliquatenus retractus & deinde amissus, subitò recurrit ad cylindri summitatem. Nam aer, qui vi magna fuit impulsus, rursus per repercussionem ad externa vi eadem revertitur.*

B. ATQUE *hoc quidem argumenti satis est, etiam solum, quòd locus à suctore relictus non est vacuus. Quid enim aut attrahere aut impellere suctorem potuit ad locum illum, unde retractus erat, si cylindrus fuisset vacuus? Nam ut aeris pondus aliquod id efficere potuisset, falsum esse satis supra demonstravi ab eo, quod aer in aere gravitare non potest. Nosti etiam, quod cum è recipiente aerem omnem (ut illi loquuntur) exegerint, possunt tamen trans vitrum id quod intus fit videre, & sonum, si quis fiat, inde audire. Id quod solum, etsi nullum aliud argumentum esset (sunt autem multa,) ad probandum,* nullum *esse in recipiente vacuum, abundè sufficit.*

B. HERE are several things joined together, which the author had before separately alledged in his often-mentioned dialogue. The first is, the cause he assigns of the ascension of the sucker, forcibly depressed to the bottom of the exhausted cylinder, and then let alone by him that pumped; to which might be added, that this ascension succeeded, when the sucker was clogged with an hundred pound weight. This explication of Mr. *Hobbes* you will find examined in my discourse. And as to his deny-

denying, that the weight or pressure of the air could drive up the sucker in that phænomenon, because the air does not weigh in air, we may see the contrary largely proved in divers places of my Examen, and more particularly and expresly in the first pages of the third chapter. And whereas he says, in the last place, that the visibility of bodies included in our receivers, and the propagation of sound, (which, by the way, is not to be understood of all sound, that may be heard, though made in the exhausted receiver,) are alone sufficient arguments to prove no vacuum; I have considered that passage in the answer I made to the like allegation, in a part of the Examen; and shall only observe here, that, since the vacuists can prove, that much of the air is pumped out of the exhausted receiver, and will pretend, that, notwithstanding many interspersed vacuities, there may be in the receiver corporeal substance enough to transmit light and stronger sounds, Mr. *Hobbes* has not performed what he pretended, if he have but barely proved, that there may be substances capable of conveying light and sound in the cavity of our receiver, since he triumphantly asserts, *nullum esse in recipienti vacuum.* But we may leave Mr. *Hobbes* and his adversaries to dispute out this point, and go on to the next passage.

A. WHICH follows in these words:

Ad illud autem, quod si vesica aliquatenus inflata in recipiente includatur, paulo post per exuctionem aeris inflatur vehementiùs, & dirumpitur, quid respondes?

B. *Motus partium aeris undiquaque concurrentium velocissimus & per concursum in spatiis brevissimis numeroque infinitis gyrationis velocissimæ vesicam in locis innumerabilibus simul & vi magna, instar totidem terebrarum, penetrat, præsertim si vesica, antequam immittatur, quò magis resistat aliquatenus inflata sit. Postquam autem aer penetrans semel ingressus est, facile cogitare potes, quo pacto deinceps vesicam tendet, & tandem rumpet. Verùm si antequam rumpatur, versâ claviculâ, aer externus admittatur, videbis vesicam propter vehementiam motus temperatum diminutâ tensione rugosiorem. Nam id quoque observatum est. Jam si hæc, quam dixi, causa minùs tibi videatur verisimilis, vide an tu aut alius quicunque imaginari potest, quo pacto vesica distendi & rumpi possit à viribus vacui, id est, nihili.*

B. THIS explication Mr. *Hobbes* gives us in the 19th page of his dialogue *De Natura aeris*, and you may find it at large confuted in the latter part of the third chapter of my Examen. Nor does, what he here says in the close about the *vires vacui* or *nihili*, deserve to detain us, since there is no reason at all, that the vacuists should ascribe to nothing a power of breaking a bladder, of whose rupture the spring of the included air supplies them so easily with a sufficient cause.

AFTER what Mr. *Hobbes* has said of the breaking of a bladder, he proceeds to an experiment, which he judges of affinity with it, and his academian having proposed this question:

Unde fit, ut animalia tam cito, nimirum spatio quatuor minutorum horæ, in recipiente interficiantur?

FOR answer to it our author says:

B. *Nonne animalia sic inclusa insugunt in pulmones aerem vehementissimè motum? quo motu necesse est, ut transitus sanguinis ab uno ad alterum cordis ventriculum interceptus, non multò pòst sistatur. Cessatio autem sanguinis mors est. Possunt tamen animalia cessante sanguine reviviscere, si aer externus satis maturè intromittatur, vel ipsa in aerem temperatum, antequam refrixerit sanguis, extrahantur.*

THIS explication is not probable enough, to oblige me to add any thing about it to what I have said in my Examen; especially the most vehement motion, ascribed to the air in the receiver, having been before proved to be an imaginary thing. You may therefore, if you please, take notice of the next explication.

[*Idem aer* (says he) *in recipiente carbones ardentes extinguit, sed & illi, si, dum satis calidi sunt, eximantur, relucebunt. Notissimum est, quòd in fodinis carbonum terreorum (cujus rei experimentum ipse vidi) sæpissime è lateribus foveæ ventus quidam undiquaque exit, qui fossores interficit, ignemque extinguit, qui tamen reviviscunt, si satis cito ad aerem liberum extrahantur.*]

THIS comparison, which Mr. *Hobbes* here summarily makes, he more fully displayed in his dialogue *De Natura Aeris*, and I considered, what he there alledged, in my Examen. And though I will not contradict Mr. *Hobbes* in what he historically asserts in this passage; yet I cannot but somewhat doubt, whether he mingles not his conjecture with the bare matter of fact. For, though I have with some curiosity visited mines in more places than one, and proposed questions to men, that have been conversant in other mines, both elsewhere and in *England* (and particularly in *Derbyshire* where Mr. *Hobbes* lived long;) yet I could never find, that any such odd and vehement wind, as Mr. *Hobbes* ascribes the phænomenon to, had been by them observed to kill the diggers, and extinguish well-lighted coals themselves: and indeed, it seems more likely, that the damp, by its tenacity or some peculiarly malign quality, did the mischief, than a wind, of which I found not any notice taken; especially since we see, what vehement winds men will be able to endure for a long time, without being near killed by them; and that it seems very odd, that a wind, that Mr. *Hobbes* does not observe to have blown away the coals, that were let down, should be able (instead of kindling them more fiercely) to blow them out.

A. THE last experiment of your engine, that your adversary mentions in these problems, is delivered in this passage:

A. *Si phialam aquæ in recipiens dimiseris, exucto aere bullire videbis aquam. Quid ad hoc respondebis?*

B. *Credo sanè in tanta aeris motitatione saltaturam esse aquam, sed ut calefiat, nondum audivi. Sed imaginable non est,* saltationem illam à vacuo nasci posse.

B. This phænomenon he likewiſe took notice of, and attempted to explicate in his above-mentioned dialogue, which gave me occaſion, to ſhew, how unlikely it is, that the vehement motion of the air ſhould be the cauſe of it; but he here tells us, that it is not imaginable, that this dancing of the water (as he is pleaſed to call it) proceeds from a vacuum, nor do I know any man, that ever pretended, that a vacuum was the efficient cauſe of it. But the vacuiſts perhaps will tell him, that, though the bubbling of the water be not an effect of a vacuum, it may be a proof of it againſt him; for they will tell him, that it has been formerly proved, that a great part of the atmoſpherical air is by pumping removed out of our exhauſted receiver, and conſequently can no more, as formerly, preſs upon the ſurface of the water. Nor does Mr. *Hobbes* ſhew what ſucceeds in the room of it; and therefore it will be allowable, for them to conclude againſt him (though not perhaps againſt the Carteſians) that there are a great many interſperſed vacuities left in the receiver, which are the occaſion, though not the proper efficient cauſe of the phænomenon. For they will ſay, that the ſpringy particles of the yet included air, having room to unbend themſelves in the ſpaces deſerted by the air, that was pumped out, the aerial and ſpringy corpuſcles, that lay concealed in the pores of the water, being now freed from the wonted preſſure, that kept them coiled up in the liquor, expanded themſelves into numerous bubbles, which becauſe of their comparative lightneſs, are extruded by the water, and many of them appear to have riſen from the bottom of it. And Mr. *Hobbes*'s vehement wind, to produce the ſeveral circumſtances of this experiment, muſt be a laſting one. For, after the agitation of the pump has been quite left off, provided the external air be kept from getting in, the bubbles will ſometimes continue to riſe for an hour after. And that, which agrees very well with our explication, and very ill with that of Mr. *Hobbes*'s, is, that, when by having continued to pump a competent time, the water has been freed from the aerial particles, that lurked in it before, though one continue to pump as luſtily as did, yet the water will not at all be covered with bubbles, as it was, the air, that produced them, being ſpent; though, according to Mr. *Hobbes*'s explications, the wind in the receiver continuing, the dance of the water ſhould continue too.

A. I eaſily gueſs, by what you have ſaid already, what you may ſay of that epiphonema, wherewith Mr. *Hobbes* (in his 18th page) concludes the explications of the phænomena of your engine. [*Spero jam te certum eſſe*, ſays he, *nullum eſſe machinæ illius phænomenon, quo demonſtrari poteſt ullum in univerſo locum dari corpore omni vacuum.*]

B. If you gueſſed aright, you gueſſed, that I would ſay, that as to the phænomena of my engine, my buſineſs was to prove, that he had not ſubſtituted good explications of them in the place of mine, which he was pleaſed to reject. And as for the proving a vacuum by the phænomena of my engine, though I declared, that was not the thing intended, yet I ſhall not wonder, that the vacuiſts ſhould think thoſe phænomena give them an advantage againſt Mr. *Hobbes*. For, though in the paſſage recited by you he ſpeak more cautiouſly than he is wont to do, yet, by what you may have already obſerved in his argumentations, the way he takes to ſolve the phænomena of our engine, is by contending, that our receiver, when we ſay it is almoſt exhauſted, is as full as ever (for he will have it perfectly full,) of common air; which is a conceit ſo contrary to I know not how many phænomena, that I do not remember I have met with or heard of any naturaliſt, whether vacuiſt or pleniſt, that having read my phyſico-mechanical experiments and his dialogue, has embraced his opinion.

A. After what you have ſaid, I will not trouble you with what he ſubjoins about vacuum in general, where having made his academian ſay, [*Mundum ſcis finitum eſſe, & per conſequens vacuum eſſe oportere totum illud ſpatium, quod eſt extra mundum infinitum. Quid impedit, quo minus vacuum illud cum aere mundano permiſceatur?*] he anſwers: *De rebus tranſmundanis nihil ſcio.* For I know, that it concerns not you to take notice of it. But poſſibly the vacuiſts will think he fathers upon them an impropriety they would not be guilty of, making them ſpeak, as if they thought, the *ultra-mundan vacuum* were a real ſubſtance, that might be brought into this world, and mingled with our air. And ſince, for aught I know, Mr. *Hobbes* might have ſpared this paſſage, if he had not deſigned it ſhould intruduce the ſlighting anſwer he makes to it; I ſhall add, that by the account Mr. *Hobbes* has given of ſeveral phænomena within the world, it is poſſible, that the vacuiſts may believe his profeſſion of knowing nothing of things beyond it.

After the *Experimenta Boyliana* (as your other adverſary calls them;) Mr. *Hobbes* proceeds to the Torricellian experiment, of which he thus diſcourſes:

A. *Quid de experimento cenſes Torricelliano, probante vacuum per argentum vivum hoc modo: eſt in ſeq. figura ad* A, *pelvis, ſive aliud vas, & in eo argentum vivum uſque ad* B; *eſt autem* C D *tubus vitreus concavus repletus quoque argento vivo. Hunc tubum ſi digito obturaveris, erexeriſque in vaſe* A, *manumque abſtuleris, deſcendet argentum vivum à* C; *verùm non effundetur totum in pelvim, ſed ſiſtetur in diſtantia quadam, puta in* D. *Nonne ergo neceſſarium eſt, ut pars tubi inter* C *&* D *ſit vacua? non enim puto negabis, quin ſuperficies tubi concava & argenti vivi convexa ſe mutuo exquiſitiſſimè contingant.*

B. *Ego neque nego contactum, neque vim conſequentiæ intelligo.*

By which paſſage it ſeems, that he ſtill perſiſts in the ſolution of this experiment, which he gave in his dialogue *De Natura Aeris*, and formerly did, for the main, either propoſe, or adopt, in his elements of philoſophy.

B. This opinion or explication of Mr. *Hobbes* I have, as far as concerns me, considered in my Examen, to which it may well suffice me to refer you. But yet let me take notice of what he now alledges:

B. *Si quis* (says he) *in argentum vivum, quod in vase est, vesicam immerserit inflatam, nonne illa amotâ manu emerget?*

A. *Ita certè, etsi esset vesica ferrea vel ex materia quacunque præter aurum.*

B. *Vides igitur ab aere penetrari posse argentum vivum.*

A. *Etiam, & quidem illâ ipsâ vi, quam à pondere accipit argenti vivi.*

I confess this allegation did a little surprize me: it concerned Mr. *Hobbes* to prove, that as much air, as was displaced by the descending mercury, did at the orifice of the tube, immersed in stagnant mercury, invisibly ascend to the upper part of the pipe. To prove this he tells us, that a bladder full of air being depressed in quick-silver, will, when the hand, that depressed it, is removed, be squeezed up by the very weight of the mercury, whence it follows, that air may penetrate quick-silver. But I know not, who ever denyed, that air invironed with quick-silver may thereby be squeezed upwards; but, since even very small bubbles of air may be seen to move in their passage through mercury, I see not, how this example will at all help the proposer of it. For it is by mere accident, that the air included in the bladder comes to be buoyed up, because the bladder itself is so; and if it were filled with water instead of air, or with stone instead of water, it would nevertheless emerge, as himself confesses it would do, if it were made of iron, or of any matter besides gold, because all other bodies are lighter in specie than quick-silver. But since the immersion of the bladder is manifest enough to the sight, I see not how it will serve Mr. *Hobbes*'s turn, who is to prove, that the air gets into the Torricellian tube invisibly; since it is plain, that even heedful observations can make our eyes discover no such trajection of the air; which (to add that enforcement of our argument) must not only pass unseen through the sustained quick-silver, but must likewise unperceivedly dive, in spite of its comparative lightness, beneath the surface of the ponderous stagnant mercury, to get in at the orifice of the erected tube. But let us, if you please, hear the rest of his discourse about this experiment.

A. Though it be somewhat prolix, yet, according to my custom hitherto, I will give it you verbatim.

B. Simul *atque argentum vivum descenderit ad* D, *altius erit in vase* A *quàm antè, nimirum plus erit argenti vivi in vase quàm erat ante descensum, tanto quantum capit pars tubi* C, D. *Tanto quoque minus erit aeris extra tubum quàm ante erat. Ille autem aer, qui ab argento vivo loco suo extrusus est, (suppositâ universi plenitudine) quò abire potest nisi ad eum locum, qui in tubo inter* C & D *à descensu argenti vivi relinquebatur? sed quâ, inquies, viâ in illum locum successurus est? Quà, nisi per ipsum corpus argenti vivi aerem urgentis? Sicut enim omne grave liquidum, sui ipsius pondere, aerem, quem descendendo premit, ascendere cogit (si via aliâ non detur) per suum ipsius corpus; ita quoque aerem quem premit ascendendo, (si via alia non detur) per suum ipsius corpus transire cogit. Manifestum igitur est, suppositâ mundi plenitudine posse aerem externum ab ipsa gravitate argenti vivi cogi in locum illum inter* C & D. *Itaque phænomenon ilud necessitatem vacui non demonstrat. Quoniam autem corpus argenti vivi penetrationi, quæ fit ab aere, non nihil resistit, & ascensioni argenti vivi in vase* A *resistit aer; quando illæ duæ resistentiæ æquales erunt, tunc in tubo sistetur alicubi argentum vivum; atque ibi est* D.

B. In answer to this explication I have in my Examen proposed divers things, which you may there meet with: and indeed his explication has appeared so improbable to those, that have written of this experiment, that I have not found it embraced by any of them, though, when divers of them opposed it, the phænomena of our engine were not yet divulged. Not then needlesly to repeat what has been said already, I shall on this occasion only add one experiment, that I afterwards made, and it was this: having made the Torricellian experiment (in a straight tube) after the ordinary way, we took a little piece of a fine bladder, and raising the pipe a little in the stagnant mercury, but not so high as the surface of it, the piece of bladder was dexterously conveyed in the quick-silver, so as to be applied by one's finger to the immersed orifice of the pipe, without letting the air get into the cavity of it; then the bladder was tied very straight and carefully to the lower end of the pipe, whose orifice, as we said, it covered before, and then the pipe being slowly lifted out of the stagnant mercury, the impendent quick-silver appeared to lean but very lightly upon the bladder, being so near an exact æquilibrium with the atmospherical air, that, if the tube were but a very little inclined, whereby the gravitation of the quick-silver, being not so perpendicular, came to be somewhat lessened, the bladder would immediately be driven into the orifice of the tube, and to the eye, placed without, appear to have acquired a concave superficies instead of the convex it had before. And when the tube was re-erected, the bladder would no longer appear sucked in, but be again somewhat protuberant. And if, when the mercury in the pipe was made to descend a little below its station into the stagnant mercury, if, I say, at that nick of time, the piece of bladder were nimbly and dexterously applied, as before, to the immersed orifice, and fastened to the sides of the pipe, upon the lifting the instrument out of the stagnant mercury, the cylinder of that liquor being now somewhat short of its due height, was no longer able fully to counterpoise the weight of the atmospherical air, which consequently, though the glass were held in an erected posture, would press up the bladder into the orifice of the

the pipe, and both make and maintain there a cavity ſenſible both to the touch and the eye.

A. WHAT did you mainly drive at in this experiment?

B. To ſatisfy ſome ingenious men, that were more diffident of, than ſkilful in hydroſtaticks, that the preſſure of the external air is capable of ſuſtaining a cylinder of twenty-nine or thirty inches of mercury; and upon a ſmall leſſening of the gravitation of that ponderous liquor, to preſs it up higher into the tube. But a farther uſe may be made of it againſt Mr. *Hobbes*'s pretenſion. For, when the tube is again erected, the mercury will ſubſide as low as at firſt, and leave as great a ſpace as formerly was left deſerted at the top; into which, how the air ſhould get to fill it, will not appear eaſy to them, that, like you and me, know by many trials, that a bladder will rather be burſt by air than grant it paſſage. And if it ſhould be pretended, either, that ſome air from without had yet got through the bladder, or, that the air, that they may preſume to have been juſt before included between the bladder and the mercury, made its way from the lower part of the inſtrument to the upper; it is obvious to anſwer, that it is no way likely, that it ſhould paſs all along the cylinder unſeen by us; ſince when there are really any aerial bubbles, though ſmaller than pins heads, they are eaſily diſcernible. And in our caſe, there is no ſuch reſiſtance of the air to the aſcenſion of the ſtagnant mercury, as Mr. *Hobbes* pretends in the Torricellian experiment made the uſual way.

A. BUT, whatever becomes of Mr. *Hobbes*'s explication of the phænomenon; yet may not one ſtill ſay, that it affords no advantage to the vacuiſts againſt him?

B. WHETHER or no it do againſt other Pleniſts, I ſhall not now conſider; but I doubt, the vacuiſts will tell Mr. *Hobbes*, that he is fain in two places of the explication, we have read, to ſuppoſe the plenitude of the world, that is, to beg the thing in queſtion, which it is not to be preſumed they will allow.

A. BUT may not Mr. *Hobbes* ſay, that it is as lawful for him to ſuppoſe a plenum, as for them to ſuppoſe a vacuum.

B. I think he may juſtly ſay ſo; but it is like they will reply, that, in their way of explicating the Torricellian experiment, they do not ſuppoſe a vacuum as to air, but prove it. For they ſhew a great ſpace, that having been juſt before filled with quickſilver, is now deſerted by it, though it appeared not, that any air ſucceeded in its room; but rather, that the upper end of the tube, is either totally, or near totally ſo devoid of air, that the quickſilver may without reſiſtance, be barely inclining the tube, be made to fill it to the very top: whereas, Mr. *Hobbes* is fain to have recourſe to that, which he knows they deny, the plenitude of the world, not proving by any ſenſible phænomena, that there did get in, through the quickſilver, air enough to fill the deſerted part of the tube, but only concluding, that ſo much air muſt have got in there, becauſe, the world being full, it could find no room any where elſe; which the vacuiſts will take for no proof at all, and the Carteſians, though Pleniſts, who admit an etherial matter capable of paſſing through the pores of glaſs, will, I doubt, look upon but as an improper explication.

A. I remember on this occaſion another experiment of yours, that ſeems unfavourable enough to Mr. *Hobbes*'s explication; and you will perhaps call it to mind, when I tell you, that it was made in a bended pipe almoſt filled with quickſilver.

B. To ſee, whether we underſtand one another, I will briefly deſcribe the inſtrument I think you mean. We took a cylindrical pipe of glaſs, cloſed at the upper end, and of that length, that being dexterouſly bent at ſome inches from the bottom, the ſhorter leg was made as parallel, as we could, to the longer: in this glaſs we found an expedient, (for it is not eaſy to do,) to make the Torricellian experiment, the quickſilver in the ſhorter leg ſerving inſtead of the ſtagnant quickſilver in the uſual baroſcope, and the quickſilver in the longer leg reaching above that in the ſhorter, about eight or nine and twenty inches. Then, by another artifice, the ſhorter leg, into which the mercury did not riſe within an inch of the top, was ſo ordered, that it could in a trice be hermetically ſealed, without diſordering the quickſilver. And this is the inſtrument, that I gueſs you mean.

A. IT is ſo, and I remember, that it is the ſame with that, which in the paradox about ſuction you call, whilſt the ſhorter leg remains unſealed, a travelling baroſcope. But when I ſaw you make the experiment, that leg was hermetically ſealed, an inch of air in its natural or uſual conſiſtence being left in the upper part of it, to which air you outwardly applied a pair of heated tongs.

B. YET that, which I chiefly aimed at in the trial, was not the phænomenon I perceive you mean; for my deſign was, by breaking the ice for them, to encourage ſome, that may have more ſkill and accommodation than I then had, to make an attempt, that I did not find to have been made by any; namely, to reduce the expanſive force of heat in every way included air, if not in ſome other bodies alſo, to ſome kind of meaſure, and, if it were poſſible, to dermine it by weight. And I preſumed, that, at leaſt, the event of my trial would much confirm ſeveral explications of mine, by ſhewing, that heat is able, as long as it laſts, very conſiderably to encreaſe the ſpring or preſſing power of the air. And in this conjecture I was not miſtaken; for, having ſhut up, after the manner newly recited, a determinate quantity of uncompreſſed air, which (in the experiment you ſaw) was about one inch; we warily held a pair of heated tongs near the outſide of the glaſs, (without making it touch the inſtrument, for fear of breaking it,) whereby the air being agitated, was enabled to expand itſelf to double its former dimenſions

menſions, and conſequently had its ſpring ſo ſtrengthened by heat, that it was able to raiſe all the quickſilver in the longer leg, and keep up, or ſuſtain, a mercurial cylinder of about nine and twenty inches high, when, by its expanſion, it would, if it had not been for the heat, have loſt half the force of its elaſticity. But whatever I deſign in this experiment, pray tell me, what uſe you would make of it againſt Mr. *Hobbes*.

A. I believe, he will find it very difficult to ſhew, what keeps the mercury ſuſpended in the longer leg of the travelling baroſcope, when the ſhorter leg is unſtopped, at which it may run out; ſince this inſtrument may, as I have tried, be carried to diſtant places, where it cannot with probability be pretended, that any air has been diſplaced by the fall of the quickſilver in the longer leg, which perhaps fell long before above a mile off. And when the ſhorter leg is ſealed, it will be very hard for Mr. *Hobbes* to ſhew there the odd motions of the air, to which he aſcribes the Torricellian experiment. For, if you warily incline the inſtrument, the quick-ſilver will riſe to the top of the longer leg, and immediately ſubſide, when the inſtrument is again erected, and yet no air appears to paſs through the quick-ſilver interpoſed between the ends of the longer and the ſhorter leg. But that, which I would chiefly take notice of in the experiment, is, that upon the external application of a hot body to the ſhorter leg of the baroſcope, when it was ſealed up, the included air was expanded from one inch to two, and ſo raiſed the whole cylinder of mercury in the longer leg, and, whilſt the heat continued undiminiſhed, kept it from ſubſiding again. For, if the air were able to get unſeen through the body of the quick-ſilver, why had it not been much more able, when rarified by heat, to paſs through the quick-ſilver, than for want of doing ſo to raiſe and ſuſtain ſo weighty a cylinder of mercury? I ſhall not ſtay to enquire on this occaſion, how Mr. *Hobbes* will, according to his hypotheſis, explicate the rarefaction of the air to double its former dimenſions, and the condenſation of it again; eſpecially ſince, aſſerting that part of the upper leg, that is unfilled with the Quick-ſilver, to be perfectly full of air, he affirms that, which I doubt he cannot prove, and which may very probably be diſproved by the experiment you mention in the diſcourſe about ſuction, where you ſhew, to another purpoſe, that in a travelling baroſcope, whoſe ſhorter leg is ſealed, if the end of the longer leg be opened, whereby it comes indeed to be filled with air, the preſſure of that air will enable the ſubjacent mercury notably to compreſs the air included in the ſhorter leg.

B. I leave Mr. *Hobbes* to conſider what you have objected againſt his explication of the Torricellian experiment; to which I ſhall add nothing, though, perhaps, I could add much, becauſe I think it may be well ſpared, and our conference has laſted long already.

A. I will then proceed to the laſt experiment recited by Mr. *Hobbes* in his *problemata de vacuo.*

A. *Si phialam, collum habentem longiuſculum, eandémque omni corpore præter aerem vacuam ore ſugas, continuoque phialæ os aquæ immergas, videbis aquam aliquouſque aſcendere in phialam. Quî fieri hoc poteſt, niſi factum ſit vacuum ab exuctione aeris, in cujus locum poſſit aqua illa aſcendere?*

B. *Conceſſo vacuo, oportet quædam loca vacua fuiſſe in illo aere, etiam qui erat intra phialam ante ſuctionem. Cur ergo non aſcendebat aqua ad ea implenda abſque ſuctione? is qui ſugit phialam, neque in ventrem quicquam, neque in pulmones, neque in os è phiala exugit. Quid ergo agit? Aerem commovet, & in partibus ejus conatum ſugendo efficit per os exeundi, & non admittendo, conatum redeundi. Ab his conatibus contrariis componitur circumitio intra phialam, & conatus exeundi quaquaverſum. Itaque phialæ ore aquæ immerſo, aer in ſubjectam aquam penetrat è phiala egrediens, & tantundem aquæ in phialam cogit.*

Præterea vis illa magna ſuctionis facit, ut ſugentis labra cum collo phialæ aliquando arctiſſimè cohæreant propter contactum exquiſitiſſimum.

B. As to the firſt clauſe of Mr. *Hobbes*'s account of our phænomenon, the vacuiſts will eaſily anſwer his queſtion, by acknowledging, that there were indeed interſperſed vacuities in the air contained in the vial before the ſuction; but they will add, there was no reaſon, why the water ſhould aſcend to fill them, becauſe, being a heavy body, it cannot riſe of itſelf, but muſt be raiſed by ſome prevalent weight or preſſure, which then was wanting. Beſides, that there being interſperſed vacuities as well in the reſt of the air, that was very near the water, as in that contained in the vial, there was no reaſon, why the water ſhould aſcend to fill the vacuities of one portion of air rather than thoſe of another. But when once by ſuction a great many of the aerial corpuſcles were made to paſs out of the vial, the ſpring of the remaining air being weakened, whilſt the preſſure of the ambient air, which depends upon its conſtant gravity, is undiminiſhed, the ſpring of the internal becomes unable to reſiſt the weight of the external air, which is therefore able to impel the interpoſed water, with ſome violence, into the cavity of the glaſs, until the air, remaining in that cavity, being reduced almoſt to its uſual denſity, is able, by its ſpring, and the weight of the water got up into the vial, to hinder any more water from being impelled up. For, as to what Mr. *Hobbes* affirms, that, *Is qui ſugit phialam neque in ventrem quicquam, neque in pulmones, neque in os quicquam exugit*; how it will agree with what he elſewhere delivers about ſuction, I leave him to conſider. But I confeſs, I cannot but wonder at his confidence, that can poſitively aſſert a thing ſo repugnant to the common ſentiments of men of all opinions, without offering any proof for it. But I ſuppoſe, they, that are by trial acquainted with ſucking, and have felt the air come in at their mouths, will prefer their own experience to his authority. And as to what he adds, that the perſon, that ſucks, agitates the air, and turns it within the vial into a kind of circulating wind, that endeavours every where to get out; I wiſh, he had ſhewn us by what means a man, that ſucks, makes this odd commotion of the air; eſpecially in ſuch vials as I uſe to employ about the experiment, the orifice of whoſe neck is ſometimes leſs than a pin's head.

A. THAT there may be really air extracted by ſuction out of a glaſs, methinks you might argue from an experiment I ſaw you make with a receiver, which was exhauſted by your pump, and conſequently by ſuction. For I remember, when you had counterpoiſed it with very good ſcales, and afterwards by turning a ſtop-cock, let in the outward air, there ruſhed in as much air to fill the ſpace, that had been deſerted by the air pumped out, as weighed ſome ſcruples (conſiſting of twenty grains a-piece) though the receiver were not of the largeſt ſize.

B. YOU did well to add that clauſe; for, the *Magdeburgic* experiment, mentioned by the induſtrious *Schottus*, having been made with a vaſt receiver, the re-admitted air amounted to a whole ounce and ſome drachms. But to return to Mr. *Hobbes*, I fear not, that he will perſuade you, that have ſeen the experiment he recites, that as ſoon as the neck of the vial is unſtopped under water, the air, that whirled about before, makes a ſally out, and forces in as much water. For, if the orifice be any thing large, you will, inſtead of feeling an endeavour to thruſt away your finger, that ſtopped it, find the pulp of your finger ſo thruſt inward, that a *Peripatetick* would affirm, that he felt it ſucked in. And that intruſion may be the reaſon, why the lip of him, that ſucks, is oftentimes ſtrongly faſtened to the orifice of the vial's neck, which Mr. *Hobbes* aſcribes to a moſt exquiſite contact, but without clearly telling us, how that extraordinary contact is effected. And when your finger is removed, inſtead of perceiving any air go out of the vial through the water, (which, if any ſuch thing happen, you will eaſily diſcover by the bubbles,) you ſhall ſee the water briſkly ſpring up in a ſlender ſtream to the top of the vial, which it could not do, if the cavity were already full of air. And to let you ſee, that when the air does really paſs in or out of the vial immerſed under water, it is very eaſy to perceive its motions, if you dip the neck of the vial in water, and then apply to the globulous part of it either your warm hands or any other competent heat, the internal air being rarified; you ſhall ſee a portion of it, anſwerable to the degree of heat you applied, manifeſtly paſs through the water in ſucceſſive bubbles, whilſt yet you ſhall not ſee any water get into the vial to ſupply the place deſerted by that air. And if, when you have (as you may do by the help of ſucking) filled the neck and part of the belly of the vial with water, you immerſe the orifice into ſtagnant water, and apply warm hands to the globulous part as before, you will find the water in the vial to be driven out, before any bubbles paſs out of the vial into the ſurrounding water; which ſhews, that the air is not ſo forward to dive under the water, and much leſs under ſo ponderous a liquor as quickſilver,) as Mr. *Hobbes* has ſuppoſed.

A. THAT it is the preſſure of the external air, that (ſurmounting the ſpring of the internal) drives up the water into the vial we have been ſpeaking of, does, I confeſs, follow upon your hypotheſis: but an experimentarian philoſopher, as Mr. *Hobbes* calls you among others, may poſſibly be furniſhed with an experiment to confirm this to the eye.

B. YOU bring into my mind what I once deviſed to confirm my hypotheſis about ſuction, but found a while ſince, that I had omitted it in my diſcourſe about that ſubject. And therefore I ſhall now repeat to you the ſubſtance at leaſt of the memorial, that was written of that experiment, by which the great intereſt of the weight of the atmoſpherical air in ſuction will appear, and in which alſo ſome things will occur, that will not well agree with Mr. *Hobbes*'s explication, and prevent ſome of his allegations againſt mine.

A. HAVING not yet met with an experiment of this nature, ſuch an one, as you ſpeak of, will be welcome to me.

B. WE took a glaſs bubble, whoſe long ſtem was both very ſlender and very cylindrical; then by applying to the outſide of the ball or globulous part a convenient heat, we expelled ſo much of the air, as that, when the end of the pipe was dipped in water, and the inward air had time to recover its former coolneſs, the water aſcended either to the top of the pipe or very near it. This done, we gently and warily rarified the air in the cavity of the bubble, 'till by its expanſion it had driven out almoſt all the water, that had got up into the ſtem, that ſo it might attain, as near as could be, to that degree of heat and meaſure of expanſion, that it had when the water began to riſe in it. And we were careful to leave two or three drops of water unexpelled at the bottom of the pipe, that we might be ſure, that none of the included air was by this ſecond rarefaction driven out at the orifice of it; as the depreſſion of the water ſo low aſſured us, on the other ſide, that the included air wanted nothing conſiderable of the expanſion it had when the water began to aſcend into the pipe. Whilſt the air was in this rarified ſtate, we preſently removed the little inſtrument out of the ſtagnant water into ſtagnant quickſilver, which in a ſhort time began to riſe in the pipe. Now, if the aſcenſion of the liquor were the effect of nature's abhorrence of a vacuum; or of ſome internal principle of motion; or of the compreſſion and propagated pulſion of the outward air by that, which had been expelled; why ſhould not the mercury have aſcended to the top of the pipe, as the water did before? But *de facto* it did not aſcend half, or perhaps a quarter ſo far; and if the pipe had been long enough, as well as it was ſlender enough, I queſtion, whether the mercury would have aſcended (in proportion to the length of the ſtem) half ſo high as it did.

Now of this experiment, which we tried more than once, I ſee not, for the reaſon lately ex-

expressed, how any good account will be given without our hypothesis, but according to that it is clear.

A. I think I perceive, why you say so; for the ascension of liquors being an effect of the prevalency of the external air's pressure against the resistance it meets with in the cavity of the instrument, and the quicksilver being bulk for bulk many times heavier than water, the same surplusage of pressure, that was able to impel up water to the top of the pipe, ought not to be able to impel up the quicksilver to any thing near that height. And if it be here objected, as it very plausibly may be, that the raised cylinder of mercury was much longer than it ought to have been in reference to a cylinder of water, the proportion in gravity between those two liquors (which is almost that of fourteen to to one) being considered; I answer, that when the cylinder of water reached to the pipe, the air possessed no more than the cavity of the globulous part of the instrument, being very little assisted to dilate itself by so light a cylinder as that of water: but when the quicksilver came to be impelled into the instrument by the weight of the external air, that ponderous body did not stop its ascent as soon as it came to be equiponderant to the formerly expelled cylinder of water; because, to attain that height, it reached but a little way into the pipe, and left all the rest of the cavity of the pipe to be filled with part of that air, which formerly was all shut up in the cavity of the bubble; by which means the air, included in the whole instrument, must needs be in a state of expansion, and thereby have its spring weakened, and consequently disabled to resist the pressure of the external air, as much as the same included air did before, when it was less rarified; on which account, the undiminished weight or pressure of the external air was able to raise the quicksilver higher and higher, till it had obtained that height, at which the pressure, compounded of the weight of the mercurial cylinder, and the spring of the internal air (now less rarified than before,) was equivalent to the pressure of the atmosphere or external air.

B. You have given the very explication I was about to propose; wherefore I shall only add, that, to confirm this experiment by a kind of inversion of it, we drove by heat a little air out of the bubble, and dipped the open end of the pipe into quicksilver, which by this means we made to ascend, till it had filled about a fourth part, or less, of the pipe, when that was held erected. Then carefully removing it without letting fall any quicksilver, or letting in any air, we held the orifice of the pipe a little under the surface of a glass full of water, and applying a moderate heat to the outside of the ball, we warily expelled the quicksilver, yet leaving a little of it to make it sure, that no air was driven out with it; then suffering the included air to cool, the external air was found able to make the water not only ascend to the very top of the pipe, and thence spread itself a little into the cavity of the ball, but to carry up before it the quicksilver, that had remained unexpelled at the bottom of the stem. And if in making the experiment we had first raised, as we sometimes did, a greater quantity of quicksilver, and afterwards drove it out, the quantity of water, that would be impelled into the cavity of the pipe and ball, would be accordingly increased.

A. In this experiment it is manifest, that something is driven out of the cavity of the glass, before the water or quicksilver begins to ascend in it: and here also we see not, that the air can pass through the pores of quicksilver or water, but that it drives them on before it, without sensibly mixing with them. In this experiment there appears not at all any circular wind, as Mr. *Hobbes* fancies in the sucked vial we are disputing of, nor any tendency outwards of the included air upon the account of such a wind; but, instead of these things, that the ascension of the liquors into the cavity of the pipe depends upon the external air, pressing up the liquors into that cavity, may be argued by this, that the same weight of the atmosphere impelled up into the pipe so much more of the lighter liquor, water, than of the heavier liquor, mercury.

B. You have said enough on this experiment; but it is not the only I have to oppose to Mr. *Hobbes* his explication: for, that there is no need of the sallying of air out of a vial, to make the atmospherical air press against a body, that closes the orifice of it, when the pressure of the internal air is much weakened; I have had occasion to shew some virtuosi, by sucking out, with the help of an instrument, a considerable portion of the air contained in a glass; for having then, instead of unstopping the orifice under water, nimbly applied a flat body to it, the external air pressed that body so forcibly against it, as to keep it fastened and suspended, though it were clogged with a weight of many ounces.

A. Another experiment of yours Mr. *Hobbes*'s explication brings into my mind, by which it appears, that, if there be such a circular wind, as he pretends, produced by suction in the cavity of the vial, it must needs be strangely lasting. For I have seen more than once, that, when you have by an instrument sucked much of the air out of a vial, and afterwards carefully closed it, though you kept the slender neck of it stopped a long time, perhaps for some weeks or months, yet when it was opened under water, a considerable quantity of the liquor would be briskly impelled up into the neck and belly of the vial. So that, though I will not be so pleasant with Mr. *Hobbes*, as to mind you on this occasion of those writers of natural magick, that teach us to shut up articulate sounds in a vessel, which being transported to a distant place, and opened there, will render the words, that are committed to it; yet I must needs say, that so lasting a circular wind, as, according to Mr. *Hobbes*, your experiments exhibited, may well deserve our wonder.

B. Your admiration would perchance increase, if I should assure you, that, having with the sun-beams produced smoke in one of those well-stopped vials, this circular wind did not at all appear to blow it about, but suffered it it to rise, as it would have done if the incuded air had been very calm. And now I shall add but one experiment more, which will not be liable to some of the things, as invalid as they are, which Mr. *Hobbes* has alledged in his account of the vial, and which will let you see, that the weight of the atmospherical air is a very considerable thing; and which may also incline you to think, that, whilst Mr. *Hobbes* does not admit a subtiler matter, than common air, to pass through the pores of close and solid bodies, the air, he has recourse to will sometimes come too late to prevent a vacuum. The experiment, which was partly accidental, I lately found registered to this sense, if not in these words: [Having, to make some discovery of the weight of the air, and for other purposes, caused an æolipile, very light, considering its bulk, to be made by a famous artist, I had occasion to put it so often into the fire for several trials, that at length the copper scaled off by degrees, and left the vessel much thinner than when it first came out of the artificer's hands; and a good while after, this change in the instrument being not in my thoughts, I had occasion to employ it, as formerly, to weigh how many grains it would contain of the air at such a determinate constitution of the atmosphere, as was to be met with, where I then chanced to be. For the making this experiment the more exactly, the air was, by a strong but warily applied fire, so carefully driven away, that, when clapping a piece of sealing-wax to the pin-hole, at which it had been forced out, we hindered any communication betwixt the cavity of the instrument and the external air, we supposed the æolipile to be very well exhausted, and therefore laid it by, that, when it should be grown cold, we might, by opening the orifice with a pin, again let in the outward air, and observe the encrease of weight, that would thereupon ensue: but the instrument, that, as I was saying, was grown thin, had been so diligently freed from air, that the very little that remained, and was kept by the wax from receiving any assistance from without, being unable, by its spring, to assist the æolipile to support the weight of the ambient air; this external fluid did, by its weight, press against it so strongly, that it compressed it, and thrust it so considerably inwards, and in more than one place so changed its figure, that, when I shewed it to the virtuosi, that were assembled at *Gresham-college*, they were pleased to command it of me to be kept in their repository, where I presume it is still to be seen.

OF

OF THE CAUSE OF ATTRACTION BY SUCTION, A PARADOX.

PREFACE.

HAVING, about twelve years ago, summarily expressed and published my opinion of the cause of suction, and a while before, or after, brought to the Royal Society the glass instrument I employed to make it out; I desisted for some time to add any thing about a problem, that I had but occasionally handled: only, because the instrument, I mentioned in my Examen of Mr. *Hobbes*'s opinion, and afterwards used at *Gresham-college*, was difficult enough to be well made, and not to be procured ready made, I did, for the sake of some virtuosi, that were curious of such things, devise a slight and easy made instrument, described in the following tract, chap. iv. in which the chief phænomena, I shewed before the Society, were easily producible. But afterwards the mistakes and erroneous opinions, that in print, as well as in discourse, I met with, even among the learned men, about suction, and the curiosity of an ingenious person, engaged me to resume that subject and treat of it, as if I had never before meddled with it, for the reason intimated in the beginning of the ensuing paper. And finding, upon the review of my latter Animadversions on Mr. *Hobbes*'s *Problemata de Vacuo*, that some passages of this tract are referred to there, I saw myself thereby little less than engaged to annex that discourse to those animadversions. And this I the rather consented to, because it contains some experiments, that I have not elsewhere met with, which, together with some other parts of that essay, may, I hope, prove of some use to illustrate and confirm our doctrine about the weight and spring of the air, and supply the less experienced than ingenious friends to our hypothesis, with more grounds of answering the latter objections of some learned men, against whose endeavours I perceive it will be useful to employ variety of experiments and other proofs, to evince the same truth; that some or other of these may meet with those arguments or evasions, with which they strive to elude the force of the rest.

THE title of the following essay may sufficiently keep the reader from expecting to find any other kind of attraction discoursed of, than that which is made by suction. But yet thus much I shall here intimate in general, that I have found by trials purposely made, that the examples of suction are not the only noted ones of attraction, that may be reduced to pulsion.

OF THE

CAUSE

OF

ATTRACTION BY SUCTION.

CHAP. I.

I MIGHT, Sir, save myself some trouble in giving you that account you desire of me about suction, by referring you to a passage in my Examen, I long since writ, of Mr. *Hobbes*'s *Dialogus Physicus de Natura Aeris*, if I knew you had those two books lying by you. But because I suspect, that my Examen may not be in your hands, since it is almost out of print, and has not for some years been in my own; and because I do not so well remember, after so long a time, the particulars, that I writ there about suction, as I do in general, that the Hypothesis I proposed, was very incidentally and briefly discoursed of, upon an occasion ministered by a wrong explication given of suction by Mr. *Hobbes*, I shall here decline referring you to what I there writ; and proposing to you those thoughts about suction, that I remember I there pointed at, I shall annex some things to illustrate and confirm them, that would not have been so proper for me to have insisted on in a short, and but occasional excursion.

AND I should immediately proceed to what you expect from me, but, that suction being generally looked upon as a kind of attraction, it will be requisite for me to premise something about attraction itself. For, besides that the cause of it, which I here dispute not of, is obscure, the very nature and notion of it is wont by naturalists to be either left untouched, or but very darkly delivered, and therefore will not be unfit to be here somewhat explained.

How general and ancient soever the common opinion may be, that attraction is a kind of motion quite differing from pulsion, if not also opposite to it; yet I confess, I concur in opinion, though not altogether upon the same grounds, with some modern naturalists, that think attraction a species of pulsion. And at least among inanimate bodies I have not yet observed any thing, that convinces me, that attraction cannot be reduced to pulsion; for, these two seem to me to be but extrinsical denominations of the same local motion, in which, if a moved body precede the movent, or tend to acquire a greater distance from it, we call it pulsion; and if, upon the score of the motion, the same body follow the movent or approach to it, we call it attraction. But this difference may consist but in an accidental respect, which does not physically alter the nature of the motion, but is founded upon the respect, which the line, wherein the motion is made, happens to have to the situation of the movent. And that, which seems to me to have been the chief cause of men's mistaking attraction for a motion opposite to pulsion, is, that they have looked upon both the moving and moved bodies in too popular and superficial a manner, and considered in the movent rather the situation of the conspicuous and more bulky part of the animal or other agent, than the situation of that part of the animal, or instrument, that does immediately impress that motion upon the mobile.

FOR those, that attentively heed this, may easily take notice, that some part of that body, or of the instrument, which by reason of their conjunction in this operation is to be looked on but as making one with it, is really placed behind some part of the body to be drawn, and therefore cannot move outwards itself without thrusting that body forward. This will be easily understood, if we consider, what happens when a man draws a chain after him; for though his body do precede the chain, yet his finger or some other part of the hand, wherewith he draws it, has some part or other, that reaches behind the fore part of the first link, and the hinder part of this link comes behind the anteriour part of the second link; and so each link has one of its parts placed behind some part of the link next after it, till you come to the last link of all. And so, as the finger, that is in the first link, cannot move forwards, but it must thrust on that link, by this series of trusions the whole chain is moved forwards; and if any other body be drawn by that chain, you may perceive, that some part of the last link comes behind some part of that body, or of some intervening body, which, by its cohesion with it, ought in our present case to be considered as part of it. And thus attraction seems to be but a species of pulsion, and usually belongs to that kind of it, which, for distinction's sake, is called trusion, by which we understand that kind of pulsion, wherein the movent goes along with the moved body, without quitting it, whilst the progress lasts; as it happens, when a gardener drives his wheel-barrow before him without letting go his hold of it.

BUT

But I must not here dissemble a difficulty, that I foresee may be speciously urged against this account of attraction. For it may be said, that there are attractions, where it cannot be pretended, that any part of the attrahent comes behind the attracted body; as in magnetical and electrical attractions, and in that, which is made of water, when it is drawn up into springs and pumps.

I need not tell you, that you know so well, as that partly the Cartesians, and partly other modern philosophers, have recourse on this occasion, either to screwed particles and other magnetical emissions, to explicate phænomena of this kind. And, according to such hypothesis, one may say, that many of these magnetical and electrical effluvia come behind some parts of the attracted bodies, or at least of the little solid particles, that are, as it were, the walls of their pores, or procure some discussion of the air, that may make it thrust the moveable towards the loadstone or amber, &c. But if there were none of these, nor any other subtil agents, that cause this motion by a real, though unperceived pulsion; I should make a distinction betwixt other attractions and these, which I should then stile attraction by invisibles. But, whether there be really any such in nature, and why I scruple to admit things so hard to be conceived, may be elsewhere considered. And you will, I presume, the freelier allow me this liberty, if (since in this place it is proper to do it,) I shew you, that in the last of the instances I formerly objected, (that of the drawing up of water into the barrel of a syringe,) there is no attraction of the liquor made by the external air. I say then, that by the ascending rammer, as a part of which I here consider the obtuse end, plug or sucker, there is no attraction made of the contiguous and subjacent water, but only there is room made for it, to rise into, without being exposed to the pressure of the superior air. For, if we suppose the whole rammer to be by divine omnipotence annihilated, and consequently uncapable of exercising any attraction; yet, provided the superior air were kept of from the water by any other way, as well as it was by the rammer, the liquor would as well ascend into the cavity of the barrel; since (as I have elsewhere abundantly proved) the surface of the terraqueous globe being continually pressed on by the incumbent air or atmosphere, the water must be, by that pressure, impelled into any cavity here below, where there is no air to resist it; as by our supposition there is not in the barrel of our syringe, when the rammer, or whatever else was in it, had been annihilated. Which reasoning may be sufficiently confirmed by an experiment, whereby I have more than once shewn some curious persons, that, if the external air, and consequently its pressure, be withdrawn from about the syringe, one may pull up the sucker as much as he pleases, without drawing up after it the subjacent water. In short, let us suppose, that a man standing in an inner room does by his utmost resistance keep shut a door, that is neither locked nor latched, against another, who with equal force endeavours to thrust it open: in this case, as if one should forcibly pull away the first man, it could not be said, that this man, by his recess from the door he endeavoured to press outwards, did truly and properly draw in his antagonist, though upon that recess the coming in of his antagonist would presently ensue; so it cannot properly be said, that by the ascent of the rammer, which displaces the superior air, either the rammer itself, or the expelled air, does properly attract the subjacent water, though the ingress of that liquor into the barrel does thereupon necessarily ensue. And that, as the comparison supposes, there is a pressure of the superior air against the upper part of the sucker, you may easily perceive, if having well stopped the lower orifice of the syringe with your finger, you forcibly draw up the sucker to the top of the barrel. For if then you let go the rammer, you will find it impelled downwards by the incumbent air with a notable force.

CHAP. II.

Having thus premised something in general about the nature of attraction, as far as it is necessary for my present design; it will be now seasonable to proceed to the consideration of that kind of attraction, that is employed to raise liquors, and is by a distinct name called suction.

About the cause of this there is great contention between the New Philosophers, as they are stiled, and the Peripateticks. For the followers of *Aristotle*, and many learned men, that in other things dissent from him, ascribe the ascension of liquors upon suction to nature's abhorrence of a vacuum. For, say they, when a man dips one end of a straw, or reed, into stagnant water, and sucks at the other end, the air contained in the cavity of the reed passes into that of his lungs, and consequently the reed would be left empty, if no other body succeed in the place it deserts; but there are only (that they take notice of) two bodies, that can succeed, the air and the (grosser liquor) the water; and the air cannot do it, because of the interposition of the water, that denies it access to the immersed orifice of the reed, and therefore it must be the water itself, which accordingly does ascend to prevent a vacuum detested by nature.

But many of the modern philosophers, and generally all the Corpuscularians, look upon this *Fuga Vacua* as but an imaginary cause of suction, though they do it upon very differing grounds. For, the atomists, that willingly admit of vacuities, properly so called, both within and without our world, cannot think, that nature hates or fears a vacuum, and declines her usual course to prevent it: And the Cartesians, though they do, as well as the Peripateticks, deny, that there is a vacuum, yet since they affirm not only, that there is none *in rerum natura*, but that there can be none, because what others call an empty space having three dimensions, hath all, that they think belonging to the essence of a body, they

they will not grant nature to be so indiscreet, as to strain her self to prevent the making of a thing, that is impossible to be made.

THE Peripatetick opinion about the cause of suction, though commonly defended by the schools, as well modern as ancient, supposes in nature such an abhorrence of a vacuum, as neither has been well proved, nor does well agree with the lately discovered phænomenon of suction. For, according to their hypothesis, water and other liquors should ascend upon suction to any height to prevent a vacuum, which yet is not agreeable to experience. For I have carefully tried, that by pumping with a pump far more stanch than those, that are usually made, and indeed as well closed as we could possibly bring it to be, we could not, by all our endeavours, raise water by suction to above $33\frac{1}{2}$ foot. The Torricellian experiment shews, that the weight of the air is able to sustain, and some of our experiments shew, it is able to raise a mercurial cylinder equal in weight to as high a cylinder of water, as we were able to raise by pumping. For mercury being near 14 times as heavy as water of the same bulk, if the weight of the air be equivalent to that of a mercurial cylinder of 29 or 30 inches, it must be able to counterpoise a cylinder of water near fourteen times as long, that is, from thirty-four to near thirty-six foot. And very disagreeable to the common hypothesis, but consonant to ours, is the experiment, that I have more than once tried, and I think elsewhere deliver'd, namely; that, if you take a glass pipe of about three foot long, and, dipping one end of it in water, suck at the other, the water will be suddenly made to flow briskly into your mouth. But, if instead of water you dip the lower end into quick-silver, though you suck as strongly as ever you can, provided, that in this case, as in the former, you hold the pipe upright, you will never be able to suck up the quick-silver near so high as your mouth; so that if the water ascended upon suction to the top of the same pipe, because else there would have been a vacuum left in the cavity of it, why should not we conclude, that, when we have sucked up the quick-silver as strongly as we can, as much of the upper part of the tube, as is deserted by the air, and yet not filled by the mercury, admits, in part at least, a vacuum, (as to air) of which consequently nature cannot reasonably be supposed to have so great and unlimited an abhorrency, as the Peripateticks and their adherents presume. Yet I will not determine, whether there be any more than many little vacuities, or spaces devoid of air, in the cavity, so called, of the pipe unfilled by the mercury, (so that the whole cavity is not one entire empty space; it being sufficient for my purpose, that my experiment affords a good argument *ad hominem* against the Peripateticks, and warrants us to seek for some other cause than the *fuga vacui*, why a much stronger suction, than that, which made water ascend with ease into the sucker's mouth, will not also raise quick-silver to the same height or near it.

See Cont. of Phys. Mech. Exp. the 15th Exp.

THOSE modern philosophers, that admit not the *faga vacui* to be the cause of the raising of liquors in suction, do generally enough agree in referring it to the action of the sucker's thorax. For, when a man endeavours to suck up a liquor, he does by means of the muscles enlarge the cavity of his chest, which he cannot do, but at the same time he must thrust away those parts of the ambient air, that were contiguous to his chest, and the displaced air does, according to some learned men, (therein, if I mistake not, followers of *Gassendus*,) compress the contiguous air, and that the next to it, and so outwards, till the pressure, successively passing from one part of the air to the other, arrive at the surface of the liquor; and all other places being as to sense full, the impelled air cannot find place, but by thrusting the water into the room made for it in the pipe, by the recess of the air, that passed into the sucker's lungs. And they differed not much from this explication, that, without taking in the compression of the ambient air made by the thorax, refer the phænomenon to the propagated motion or impulse, that is impressed on the air displaced by the thorax in its dilatation, and yet unable to move in a world perfectly filled, as they suppose ours to be, unless the liquor be impelled into as much of the cavity of the pipe, as fast as it is deserted by the air, that is said to be sucked up. But though I readily confess this explication to be ingenious, and such as I wonder not they should acquiesce in, who are acquainted but with the long known, and obvious phænomena of suction; and though I am not sure, but that in the most familiar cases the causes assigned by them may contribute to the effect; yet, preserving for *Cartesius* and *Gassendus* the respect I willingly pay such great philosophers, I must take the liberty to tell you, that I cannot acquiesce in their theory. For I think, that the cause of suction they assign, is in many cases not necessary, in others not sufficient. And first, as to the condensation of the air by the dilatation of the sucker's chest; when I consider the extent of the ambient air, and how small a compression no greater an expansion than that of the thorax is like to make, I can scarce think so slight a condensation of the free air can have so considerable an operation on the surface of the liquor to be raised, as the hypothesis I examine requires: and that this impulse of the air by a sucker's dilated thorax, though it be wont to accompany the ascension of the water procured by suction, yet is not of absolute necessity to it, will, I presume, be easily granted, if it can be made out, that even a propagated pulsion, abstracted from any condensation of air, is not so necessarily the cause of it, but that the effect may be produced without it. For suppose, that by divine omnipotence so much air, as is displaced by the thorax, were annihilated; yet I see not, why the ascension of the liquor should not ensue. For, when a man begins to suck, there is an æquilibrium, or rather æquipollency between

tween the preſſure, which the air, contained in the pipe, (which is ſhut up with the preſſure of the atmoſphere upon it,) has, by virtue of its ſpring, upon that part of the ſurface of the water, that is environed by the ſides of the pipe, and the preſſure, which the atmoſpherical air has, by virtue of its weight, upon all the reſt of the ſurface of the ſtagnant water; ſo that, when by the dilatation of the ſucker's thorax, the air within the cavity of the pipe comes to be rarified, and conſequently loſe of its ſpring, the weight of the external air continuing in the mean time the ſame, it muſt neceſſarily happen, that the ſpring of the internal air will be too weak to compreſs any longer the gravitation of the external, and conſequently, that part of the ſurface of the ſtagnant water, that is included in the pipe, being leſs preſſed upon, than all the other parts of the ſame ſurfaces muſt neceſſarily give way, where it can leaſt reſiſt, and conſequently be impelled up into the pipe, where the air, having had its ſpring weakened by expanſion, is no longer able to reſiſt, as it did before. This may be illuſtrated by ſomewhat varying an inſtance already given, and conceiving, that within a chamber three men thruſt all together with their utmoſt force againſt a door, (which we ſuppoſe to have neither bolt nor latch) to keep it ſhut, at the ſame time the three other men have juſt equal ſtrength, and employ their force to thruſt it open. For though, whilſt their oppoſite endeavours are equal, the door will continue to be kept ſhut, yet if one of the three men, within the room, ſhould go away, there will need no new force, nor other acceſſion of ſtrength to the three men, to make them prevail and thruſt open the door againſt the reſiſtance of thoſe, that endeavoured to keep it ſhut, who are now but two.

AND here (upon the by) you may take notice, that, to raiſe water in ſuction, there is no neceſſity of any rarified and forcibly ſtretched rope, as it were, of the air, to draw up the ſubjacent water into the pipe, ſince the bare debilitation of the ſpring of the included air may very well ſerve the turn. And though, if we ſhould ſuppoſe the air within the pipe to be quite annihilated, it could not be pretended (ſince it would not have ſo much as exiſtence) that it exerciſes an attractive power; yet in this caſe the water would aſcend into the pipe, without the aſſiſtance of nature's imaginary abhorrence of a vacuum, but by a mechanical neceſſity, plainly ariſing from this, that there would be a preſſure of the incumbent atmoſphere upon the reſt of the ſurface of the ſtagnant water, and no preſſure at all upon that part of the ſurface, that is within the pipe, where conſequently there could be no reſiſtance made to the aſcenſion of the water, every where elſe ſtrongly urged by the weight of the incumbent air.

I ſhall add on this occaſion, that, to ſhew ſome inquiſitive men, that the weak reſiſtance within a veſſel, that had but one orifice expoſed to the water, may much more contribute to the aſcenſion of that liquor into the veſſel, than either the compreſſion, or the continued or reflected impulſe of the external air; I thought fit to produce a phænomenon, which by the beholders was without ſcruple judged an effect of ſuction, and yet could not be aſcribed to the cauſe of ſuction, aſſigned by either of the ſects of philoſophers I diſſent from. The experiment was this: by a way, elſewhere delivered, the long neck of a glaſs bubble was ſealed up, and almoſt all the air had been by heat driven out of the whole cavity of the bubble or vial, and then the glaſs was laid aſide for ſome hours, or as long as we pleaſed; afterwards the ſealed apex of the neck was broken off under water. I demand now of a Peripatetic, whether the liquor ought to be ſucked or drawn into the cavity of the glaſs, and why? If he ſays, as queſtionleſs he will, that the water would be attracted to hinder a vacuum, he would thereby acknowledge, that, till the glaſs was unſtopped under water, there was ſome empty ſpace in it; for, till the ſealed end was broken off, the water could not get in, and therefore, if the *fuga vacui* had any thing to do in the aſcenſion, the liquor muſt riſe, not to prevent an empty ſpace, but to fill one, that was made before. Nor does our experiment much more favour the other philoſophers I diſſent from; for in it there is no dilatation made of the ſides of the glaſs, as in ordinary ſuction there is made of the thorax, but only there is ſo much air driven out of the cavity of the bubble, into whoſe room ſince neither common air nor water is permitted to ſucceed, it appears not, how the propagated and returning impulſe, or the circle of motion, as to common air and water, does here take place. And then I demand, what becomes of the air, that has been by heat driven out, and is by the hermetical ſeal kept out of the cavity of the bubble? If it be ſaid, that it diffuſes it ſelf into the ambient air, and mingles with it, that will be granted, which I contended for, that ſo little air, as is uſually diſplaced in ſuction, cannot make any conſiderable compreſſion of the free ambient air; for, what can one cubic inch of air, which is ſometimes more than one of our glaſſes contains, do, to the condenſation ſo much as of all the air in the chamber, when the expelled corpuſcles are evenly diſtributed among thoſe of the ambient? And how comes this inconſiderable condenſation to have ſo great an effect in every part of the room, as to be able there to impel into the glaſs as much water in extent, as the whole air, that was driven out of the cavity of it? But if it be ſaid, that the expelled air condenſed only the contiguous or very neighbouring air, it is eaſy to anſwer, that it is no way probable, that the expelled particles of the air ſhould not, by the differing motions of the ambient air, be quickly made to mingle with it, but ſhould rather wait (which, if it did, we ſometimes made it do for many hours) till the veſſels, whence it was driven out, were unſtopped again. But, though this could probably be pretended, it cannot truly be aſſerted. For if you carry the ſealed glaſs quite out of the room or houſe, and unſtop it at ſome other place, though two or three miles diſtant; the aſcenſion of the water will (as I found by trial) neverthelefs enfue; in which cafe I prefume, it will not be faid, that the air, that was expelled out of the glafs, and condenfed the contiguous or near contiguous air, attended the bubble in all its motions, and was ready at hand to impel in the water, as foon as the fealed apex of the vial was broken off. But I doubt not, but moft of the embracers of the opinion I oppofe, being learned and ingenious perfons, if they had been acquainted with thefe and the like phænomena, would rather have changed their opinion about fuction, than have gone about to defend it by fuch evafions, which I fhould not have thought worth propofing, if I had not met with objections of this nature publickly maintained by a learned writer, on occafion of the air's rufhing into the exhaufted Magdenburgic engine. But as in our experiment thefe objections have no place, fo in our hypothefis the explication is very eafy, as will anon be intimated.

CHAP. III.

HAVING thus fhewn, that the afcenfion of water upon fuction may be caufed otherwife than by the condenfation or the propagated pulfion of air contiguous to the fucker's thorax, and thruft out of place by it; it remains, that I fhew, (which was one of the two things I chiefly intended) that there may be cafes, wherein the caufe, affigned in the hypothefis I am examining, will not have place. But this will be better underftood, if, before I proceed to the proof of it, I propofe to you the thoughts, I had many years fince, and do ftill retain, about the caufe of the afcenfion of liquors in fuction.

To clear the way to the right underftanding of the enfuing difcourfe, it will not be amifs here to premife a fummary intimation of fome things, that are fuppofed in our hypothefis.

We fuppofe then firft, without difputing either the exiftence or the nature of elementary air, that the common air we breathe in, and which I often call atmofpherical air, abounds with corpufcles not devoid of weight, and endowed with elafticity or fpringinefs, whereby the lower parts, compreffed by the weight of the upper, inceffantly endeavour to expand themfelves, by which expanfion, and in proportion to it, the fpring of the air is weakened, (as other fprings are wont to be) the more they are permitted to ftretch themfelves.

Next, we fuppofe, that the terraqueous globe, being environed with this gravitating and fpringy air, has its furface and the bodies placed on it, preffed by as much of the atmofphere, as either perpendicularly leans on them, or can otherwife come to bear upon them. And this preffure is, by the Torricellian and other experiments, found to be equivalent to a perpendicularly erected cylinder of about twenty-nine or thirty inches of quick-filver, (for the height is differing, as the gravity of the atmofphere happens to be various.)

Lastly, we fuppofe, that air being contained in a pipe or other hollow body, that has but one orifice open to the free air, if this orifice be hermetically fealed, or otherwife (as with the mouth of one, that fucks) clofed, the now included air, whilft it continues without any farther expanfion, will have an elafticity equivalent to the weight of as much of the outward air, as did before prefs againft it. For if the weight of the atmofphere, to which it was then expofed, had been able to comprefs it further, it would have done fo, and then the clofing of the orifice, at which the internal and external air communicated, as it fenced the included air from the preffure of the incumbent, fo it hindred the fame included air from expanding it felf; fo that, as it was fhut up with the preffure of the atmofphere upon it, that is in a ftate of as great compreffion, as the weight of the atmofphere could bring it to, fo, being fhut up, and thereby kept from weakening that preffure by expanfion, it muft retain a fpringinefs equipollent to the preffure it was expofed to before, which (as I juft now noted) was as great, as the weight of the incumbent pillar of the atmofphere could make it. But if, as was faid in the firft fuppofition, the included air fhould come to be dilated or expanded, the fpring being then unbent, its fpring, like that of other elaftical bodies, would be debilitated anfwerably to that expanfion.

To me then it feems, that, fpeaking in general, liquors are upon fuction raifed into the cavities of pipes and other hollow bodies, when, and fo far as there is a lefs preffure on the furface of the liquor, in the cavity, than on the furface of the external liquor, that furrounds the pipe, whether that preffure on thofe parts of the external liquor, that are from time to time impelled up into the orifice of the pipe, proceed from the weight of the atmofphere, or the propagated compreffion, or impulfe of fome parts of the air, or the fpring of the air, or fome other caufe, as the preffure of fome other body quite diftinct from air.

Upon the general view of this hypothefis, it feems very confonant to the mechanical principles. For, if there be on the differing parts of the furface of a fluid body unequal preffures, it is plain, as well by the nature of the thing, as by what has been demonftrated by *Archimedes*, and his commentators, that the greater force will prevail againft the leffer, and that that part of the water's furface muft give way, where it is leaft preffed. So that that, wherein the hypothefis I venture to propofe to you, differs from that, which I diffent from, is not, that mine is lefs mechanical; but partly in this, that, whereas the hypothefis, I queftion, fuppofes a neceffity of the protrufion or impulfe of the air, mine does not require that fuppofition, but, being more general, reaches to other ways of procuring the afcenfion of liquors, without raifing them by the impulfe of the air; and partly, and indeed chiefly, in that the hypothefis, I decline, makes the caufe of the afcenfion of liquors to be only the increafed preffure of the air external to the pipe; and I chiefly make it to depend upon the diminifhed preffure of the air within the pipe, on the fcore of the expanfion it is brought to by fuction.

TO proceed now to some experiments, that I made in favour of this hypothesis, I shall begin with that which follows:

WE took a glass-pipe bended like a syphon, but so, that the shorter leg was as parallel to the longer, as we could get it made, and was hermetically sealed at the end: into this syphon we made a shift (for it is not very easy) to convey water, so that the crooked part being held downwards, the liquor reached to the same height in both the legs, and yet there was about an inch and a half of uncompressed air shut up in the shorter leg. This little instrument (for it was but about fifteen inches long) being thus prepared, it is plain, that according to the hypothesis I dissent from, there is no reason, why the water should ascend upon suction. For, though we should admit, that the external air were considerably compressed, or received a notable impulse, when the sucker's chest is enlarged; yet in our case, that compression, or protrusion, will not reach the surface of the water in the shorter leg, because it is there fenced from the action of the external air by the sides of the glass, and the hermetical seal of the top, and yet, if one sucked strongly at the open orifice in the longer leg, the water in the shorter would be depressed; and that in the longer ascended at one suck about an inch and half: of which the reason is clear in our hypothesis. For, the spring of the included air, together with the weight of the water in the shorter leg, and the pressure of the atmospherical air, assisted by the weight of the liquor in the longer leg, counter-balanced one another before the suction began: But when afterwards upon suction, the air in the longer leg came to be dilated, and thereby weakened, it was rendered unable to resist the undiminished pressure of the air included in the shorter leg, which consequently expanding itself by virtue of its elasticity, depressed the contiguous water, and made it proportionably rise in the oppsite leg, till, by the expansion, its spring being more and more weakened, it arrived at an equipollency with the gravitation or pressure of the atmosphere. Which last clause contains the reason, why, when the person, that sucked, had raised the water in the longer leg, less than three inches higher by repeated endeavours to suck, and that without once suffering the water to fall back again, he was not able to elevate the water in the longer, so much as three inches above its first station. And if in the shorter leg, there was but an inch and a quarter of space left for the air unfilled by the water, by divers skilfully reiterated acts of suction, he could not raise the liquor in the longer leg above two inches; because, by that time, the air included in the shorter leg, had, by expanding itself further and further, proportionably weakened its spring, till at length it became as rarified, as was the air in the cavity of the longer leg, and consequently was able to thrust away the water with no more force than the air in the long leg was able to resist. And by the recited trial it appeared, that the rarefaction usually made of air by suction is not near so great, as one would expect, probably, because by the dilatation of the lungs, the air being still shut up, is but moderately rarified, and the air in the longer leg, can by them, be brought to no greater degree of rarity, than that of the air within the chest. For, whereas the included air in our instrument was not expanded, by my estimate, at one suck, to above the double of its former dimensions, and by divers successive sucks was expanded but from one inch and an half, to less than four inches and an half, if the suction could have been conveniently made with a great and stanch syringe, the rarefaction of the air would probably have been far greater; since in our pneumatick engine air may, without heat, and by a kind of suction, be brought to possess many hundreds of times the space it took up before. From this rarefaction of the air in both the legs of our instrument proceeds another phænomenon, readily explicable by our hypothesis. For if, when the water was impelled up as high as the suction could raise it, the instrument were taken from the sucker's mouth, the elevated water would with violence return to its wonted station. For, the air, in both the legs of the instrument, having by the suction lost much of the spring, and so of its power of pressing; when once the orifice of the longer leg was left open, the atmospherical air came again to gravitate upon the water in that leg, and the air, included in the other leg, having its spring debilitated by the precedent expansion, was not able to hinder the external air from violently repelling the elevated water, till the included air was thrust into the space it possessed before the suction; in which space it had density and elasticity enough to resist the pressure, that the external air exercised against it through the interposed water.

BUT our hypothesis about the cause of suction would not need to be sollicitously proved to you by other ways, if you had seen what I have sometimes been able to do in our pneumatick engine. For, there we found, by trials purposely devised, and carefully made, that a good syringe being so conveyed into our receiver, that the open orifice of the pipe or lower part was kept under water, if the engine were exhausted, though the handle of the syringe were drawn up, the water would not follow it, which yet it would do, if the external air were let in again. The reason of which is plain in our hypothesis; for the air, that should have pressed upon the surface of the stagnant water, having been pumped out, there was nothing to impel up the water into the deserted cavity of the syringe, as there was, when the receiver was filled with air.

CHAP. IV.

BUT because such a conveniency as our engine, and the apparatus necessary for such trials, are not easily procurable, I shall endeavour to confirm our hypothesis about suction, by subjoining some experiments, that may be tried without the help of that engine, for the making out these three things:

I.

I. That a liquor may be raised by suction, when the pressure of the air, neither as it has weight nor elasticity, is the cause of the elevation.

II. That the weight of the atmospherical air is sufficient to raise up liquors in suction.

III. That, in some cases, suction will not be made, as, according to the hypothesis I dissent from, it should, although there be a dilatation of the sucker's thorax, and no danger of a vacuum, though the liquor should ascend.

And first, to shew, how much the rising of liquors in suction depends upon the weight or pressure of the impellent body, and how little necessity there is, where that pressure is not wanting, that, in the place deserted by the liquor, that is sucked there should succeed air, or some other visible body, as the Peripatetic schools would have it; to shew this, I say, I thought on the following experiments. We took a glass pipe, fit to have the Torricellian experiment made with it, but a good deal longer than was necessary for that use: this pipe being hermetically sealed at one end, the other end was so bent, as to be reflected upwards, and make as it were the shorter leg of the syphon as parallel as we could to the longer, so that the tube now was shaped like an inverted syphon, with legs of a very unequal length. This tube, notwithstanding its inconvenient figure, we made a shift (for it is not easily done) to fill with mercury, when it was in an inclined posture, and then erecting it, the mercury subsided in the longer leg, as in the Torricellian experiment, and attained to between two foot and a quarter and two foot and an half above the surface of the mercury in the shorter leg, which in this instrument answers to the stagnant mercury in an ordinary barometer, from which to distinguish it, I have elsewhere called this syphon, furnished with mercury, a travelling baroscope, because it may be safely carried from place to place. Out of the shorter leg of this tube, we warily took as much mercury, as was thought convenient for what we had further to do; and this we did by such a way, as to hinder any air from getting into the deserted cavity of the longer leg, by which means the mercurial cylinder (estimated as I lately mentioned) retained the same height above the stagnant mercury in the shorter: the upper and closed part of this travelling baroscope, you will easily grant to have been free from common air, not only for other reasons, that have been given elsewhere, but particularly for this, that if you gently incline the instrument, the quicksilver will ascend to the top of the tube; which you know it could not do, if the place, formerly deserted by it, were possessed by the air, which, by its spring, would hinder the ascension of the mercury, (as is easy to be tried.) The instrument having been thus fitted, I caused one of the by-standers to suck at the shorter leg, whereupon (as I expected) there presently ensued an ascension of four or five inches of mercury in that leg, and a proportionable subsidence of the mercury in the longer, and yet in this case the raising of the mercury cannot be pretended to proceed from the pressure of the air. For the weight of the atmosphere is fenced off by that, which closes the upper end of the longer tube, and the spring of the air has here nothing to do, since, as we have lately shewn, the space deserted by the mercury is not possessed by the included air, and the pulsion or condensation of the air, supposed, by divers modern philosophers, to be made by the dilatation of the sucker's chest, and to press upon the surface of the liquors, that are to be sucked up: this, I say, cannot here be pretended, in regard the surface of the liquor in the longer leg is every way fenced from the pressure of the ambient air. So that it remains, that the cause, which raised the quicksilver in the shorter leg, upon the newly recited suction, was the weight of the collaterally superior quicksilver in the longer leg, which being (at the beginning of the suction) equivalent to the weight of the atmosphere, there is a plain reason, why the stagnant mercury, in the shorter leg, should be raised some inches by suction; as mercury, stagnant in an open vessel, will be raised by the weight of the atmosphere, when the suction is made in the open air. For, in both cases, there is a pipe, that reaches to the stagnant mercury, and a competent weight to impel it into the pipe; when the air in the cavity of the pipe has its spring weakened by the dilatation, that accompanied suction.

The second point formerly proposed, which is, that the weight of the air is sufficient to raise liquors in suction, may not be ill proved by arguments, legitimately drawn from the Torricellian experiment itself, and much more clearly by the first and fifteenth of our continued physico-mechanical experiments. And therefore I shall only here take notice of a phænomenon, that may be exhibited by the travelling baroscope, which, though it be much inferior to the experiments newly referred to, may be of some use on the present occasion.

Having then provided an instrument like the travelling baroscope, mentioned under the former head, but whose legs were not so unequally long, and having in it made the Torricellian experiment, after the manner lately described, we ordered the matter so, that there remained in the shorter leg the length of divers inches unfilled with stagnant mercury. Then I caused one, versed in what he was to do, so to raise the quicksilver by suction to the open orifice of the shorter leg, that the orifice being seasonably and dexterously closed, the mercury continued to fill that leg, as long as we thought fit; and then, having put a mark to the surface of the mercury in the longer leg, we unstopped the orifice of the shorter; whereupon the mercury, that before filled it, was depressed, till the same liquor in the longer leg was raised five inches or more above the mark, and continued at that height. I said, that the mercury, that had been raised by suction, was depressed, rather than that it subsided, because its own weight could not here make it fall, since a mercurial cylinder of five inches was far from being able to raise so tall a cylinder of mercury, as made a counterpoiſe in the longer leg; and therefore the depreſſion we ſpeak of, is to be referred to the gravitation of the atmoſpherical air upon the ſurface of the mercury in the ſhorter leg: and I ſee no cauſe to doubt, but that, if we could have procured an inſtrument, into whoſe ſhorter leg a mercurial cylinder of many inches higher could have been ſucked up, it would, by this contrivance, have appeared, that the preſſure of the atmoſphere would eaſily impel up a far taller cylinder of mercury, than it did in our recited experiment.

THAT this is no groundleſs conjecture, may appear probable by the experiment you will preſently meet with. For, if the gravity of an incumbent pillar of the atmoſphere be able to compreſs a parcel of included air, as much as a mercurial cylinder, equivalent in weight to between thirty and five and thirty foot of water, is able to condenſe it, it cannot well be denied, that the ſame atmoſpherical cylinder may be able, by its weight, to raiſe and counter-balance eight or nine and twenty inches of quickſilver, or an equivalent pillar of water in tubes, where the reſiſtance of theſe two liquors, to be raiſed and ſuſtained by the air, depends only upon their own unaſſiſted gravity.

To confirm our doctrine of the gravitation of the atmoſphere upon the ſurface of the liquors expoſed to it, I will ſubjoin an experiment, that I deviſed to ſhew, that the incumbent air, in its natural or uſual ſtate, would compreſs other air not rarified, but in the like natural ſtate, as much as a cylinder of eight or nine and twenty inches of mercury would condenſe or compreſs it.

IN order to the making of this, I muſt put you in mind of what I have ſhewn elſewhere * at large, and ſhall further confirm by one of the experiments, that follows the next; namely, that about twenty-nine or thirty inches of quick-ſilver will compreſs air, that being in its natural or uſual ſtate (as to rarity and denſity) has been ſhut up in the ſhorter leg of our travelling or ſyphon-like baroſcope, into half the room, that included air poſſeſſed before. This premiſed, I paſs on to my experiment, which was this:

WE provided a travelling baroſcope, wherein the mercury in the longer leg was kept ſuſpended by the counterpoiſe of the air, that gravitated on the ſurface of the mercury in the ſhorter leg, which we had ſo ordered, that it reached not by about two inches to the top of the ſhorter leg. Then making a mark at the place, where the ſtagnant mercury reſted, it was manifeſt, according to our hypotheſis, that the air in the upper part of the ſhorter leg was in its natural ſtate, or of the ſame degree of denſity with the outward air, with which it freely communicated at the open orifice of the ſhorter leg; ſo that this ſtagnant air was equally preſſed upon by the weight of the collaterally ſuperior cylinder of mercury in the longer leg, and the equivalent weight of a directly incumbent pillar of the atmoſphere. Things being in this poſture, the upper part of the ſhorter leg, which had been before purpoſely drawn out to an almoſt capillary ſmallneſs, was hermetically ſealed, which, though the inſtrument was kept erected, was ſo nimbly done by reaſon of the ſlenderneſs of the pipe, that the included air did not appear to be ſenſibly heated, though for greater caution we ſtaid a while from proceeding, that, if any rarefaction had been produced in the air, it might have time to loſe it again. This done, we opened the lower end of the longer leg, (which had been ſo ordered before, that we could eaſily do it, and without concuſſion of the veſſel,) by which means the atmoſpherical air, gaining acceſs to the mercury included in the longer leg, did, as I expected, by its gravitation upon it, ſo compreſs the air included in the ſhorter leg, that, according to the eſtimate we made with the help of a ruler, (for by reaſon of the conical figure of the upper part of the glaſs we could not take preciſe meaſures,) it was thruſt into near half the room it took up before, and conſequently, according to what I put you lately in mind of, endured a compreſſion like that, which a mercurial cylinder of about twenty nine inches would have given it.

THIS experiment, as to the main of it, was for greater caution made the ſecond time with much the like ſucceſs; and though it had been more eaſy to meaſure the condenſation of the air, if, inſtead of drawing out and ſealing up the ſhorter leg of the inſtrument, we had contented our ſelves to cloſe it ſome other way; yet we rather choſe to employ *Hermes*'s ſeal, leſt, if any other courſe had been taken, it might be pretended, that ſome of the included air, when it began to be compreſſed, might eſcape out at the not perfectly and ſtrongly cloſed orifice of the leg, wherein it was impriſoned.

To make it yet further appear, how much the aſcenſion of liquors by ſuction depends upon preſſure, rather than upon nature's imaginary abhorrence of a vacuum, or the propagated pulſion of the air; I will ſubjoin an inſtance, wherein that preſumed abhorrence cannot be pretended. The experiment was thus made:

A glaſs-ſiphon, like thoſe lately deſcribed, with one leg far longer than the other, was hermetically ſealed at the ſhorter leg, and then by degrees there was put in, at the orifice of the longer leg, as much quick-ſilver, as by its weight ſufficed to compreſs the air in the ſhorter leg into about half the room it poſſeſſed before; ſo that, according to the Peripatetick doctrine, the air muſt be in a ſtate of preternatural condenſation, and, that to a far greater degree, than (as I have tried) it is uſually brought to by cold, intenſe enough to freeze water. Then meaſuring the heighth of the quick-ſilver in the longer tube above the ſuperficies of that in the ſhorter, we found it not exceed thirty inches. Now, if liquors did riſe in ſuction *ob fugam vacui*, there is no rea-

* See the Author's Defence of the Doctrine touching the Spring and Weight of the Air, againſt *Fr. Linus*, chap. v.

ſon, why this quick-ſilver in the longer part of the ſiphon ſhould not eaſily aſcend upon ſuction, at leaſt till the air in the ſhorter leg had regained its former dimenſions, ſince it cannot in this place be pretended, that, if the mercury ſhould aſcend, there would be any danger of a vacuum in the ſhorter leg of the tube, in regard, that the contiguous included air is ready at hand to ſucceed, as faſt as the mercury ſubſides in the ſhorter leg of the ſiphon. Nor can it be pretended, that, to fill the place deſerted by the quick-ſilver, the included air muſt ſuffer a preternatural rarefaction or deſcenſion; ſince it is plain in our caſe, that on the contrary, as long as the air continues in the ſtate, whereto the weight of the quick-ſilver has reduced it, it is kept in a violent ſtate of compreſſion; ſince in the ſhorter leg it was in its natural ſtate, when the mercury, poured into the longer leg, did by its weight thruſt it into about half the room it took up before. And yet, having cauſed ſeveral perſons, one of them verſed in ſucking, to ſuck divers times as ſtrongly as they could, they were neither of them able, not ſo much as for a minute of an hour, to raiſe the mercury in the longer leg, and make it ſubſide in the ſhorter for more than about an inch at moſt. And yet to ſhew you, that the experiment was not favourably tried for me, the height of the mercurial cylinder in the longer leg above the ſurface of that in the ſhorter leg was, when the ſuction was tried, an inch or two ſhorter than thirty inches, and the compreſſed air in the ſhorter leg was ſo far from having been by the exſuction expanded beyond its natural and firſt dimenſions, that it did not, when the contiguous mercury ſtood as low as we could make it ſubſide, regain ſo much as one half of the ſpace it had loſt by the precedent compreſſion, and conſequently was in a preternatural ſtate of condenſation, when it had been freed from that ſtate as far as ſuction would do it. Whence it ſeems evident, that it was not *ob fugam vacui*, that the quick-ſilver did upon ſuction aſcend one inch; for, upon the ſame ſcore it ought to have aſcended two, or perhaps more inches, ſince there was no danger, that by ſuch an aſcenſion any vacuum ſhould be produced or left in the ſhorter leg of the ſiphon; whereas, according to our hypotheſis, a clear cauſe of the phænomenon is aſſignable. For, before the ſuction was begun, there was an æquilibrium, or equipollency, between the weight of the ſuperior quick-ſilver in the longer leg, and a ſpring of the compreſſed air included in the ſhorter leg; but when the experimentor began to ſuck, his cheſt being widened, part of the air included in the upper part of the longer leg paſſed into it, and that, which remained, had by that expanſion its preſſure ſo weakened, that the air in the ſhorter leg, finding no longer the former reſiſtance, was able by its own ſpring to expand itſelf, and conſequently to depreſs the contiguous mercury in the ſame ſhorter leg, and raiſe it as much in the longer.

But here a hydroſtatician, that heedfully marks this experiment, may diſcern a difficulty, that may perhaps ſomewhat perplex him, and ſeems to overthrow our explication of the phænomenon. For he may object, that if the compreſſed air in the ſhorter leg had a ſpring equipollent to the weight of the mercury in the longer leg, it appears not, why the mercury ſhould not be ſucked up in this inſtrument, as well as in the free air; ſince, according to me, the preſſure of the included air upon the ſubjacent mercury muſt be equivalent to the weight of the atmoſphere, and yet experience ſhews, that the weight of the atmoſphere will, upon ſuction, raiſe quickſilver to the height of ſeveral inches.

To clear this difficulty, and ſhew, that, though it be conſiderable, it is not at all inſuperable, be pleaſed to conſider with me, that I make indeed the ſpring of the compreſſed air to be equipollent to the weight of the compreſſing mercury, and I have a manifeſt reaſon to do it; becauſe, if the ſpring of the air were not equipollent to that weight, the mercury muſt neceſſarily compreſs the air farther, which it is granted *de facto* not to do. But then I conſider, that in our caſe there ought to be a great deal of difference between the operation of the ſpring of the included air and the weight of the atmoſphere, after ſuction has been once begun. For the weight of the atmoſphere, that impels up mercury and other liquors, when the ſuction is made in the open air, continues ſtill the ſame; but the force or preſſure of the included air is equal to the counterpreſſure of the mercury, no longer than the firſt moment of the ſuction; after which, the force of the impriſoned air ſtill decreaſes more and more, ſince this compreſſed air, being further and further expanded, muſt needs have its ſpring proportionably weakened; ſo that it need be no wonder, that the mercury was not ſucked up any more than we have related; for there was nothing to make it aſcend to a greater height, than that, at which the debilitated ſpring of the (included but) expanded air was brought to an equipollency with the undiminiſhed, and indeed ſomewhat encreaſed weight of the mercurial cylinder in the longer leg, and the preſſure of the aerial cylinder in the ſame leg, leſſened by the action of him, that ſucked. For whereas, when the orifice of this leg ſtood open, the mercury was preſſed on by a cylinder of the atmoſpherical air, equivalent to about thirty inches of quick-ſilver; by the mouth and action of him, that ſucked the tube, was freed from the external air, and by the dilatation of his thorax, the neighbouring air, that had a free paſſage through his wind-pipe to it, was proportionably expanded, and had its ſpring and preſſure weakened: by which means, the compreſſed air in the ſhorter leg of the ſyphon was enabled to impel up the mercury, until the lately mentioned equilibrium or equipollency was attained. And I muſt here take notice, that, as the quick-ſilver was raiſed by ſuction but a little way, ſo the cylinder, that was raiſed, was a very long one; whereas, when mercury is ſucked up in the free air, it is ſeldom raiſed to half that length; though, as I noted before, the impellent cauſe, which

which is the weight of the atmoſphere, continued ſtill the ſame, whereas in our ſyphon, when the mercury was ſucked up but an inch, the compreſſed air, poſſeſſing double the ſpace it did before, had by this expanſion already loſt a very conſiderable part of its former ſpring and preſſure.

I ſhould here conclude this diſcourſe, but that I remember a phænomenon of our pneumatick engine, which to divers learned men, eſpecially Ariſtotelians, ſeemed ſo much to argue, that ſuction is made either by a *fuga vacui*, or ſome internal principle, that divers years ago I thought fit to ſet down another account of it, and lately meeting with that account among other papers, I ſhall ſubjoin it juſt as I found it, by way of appendix to the foregoing Tract.

AMONG the more familiar phænomena of the *Machina Boyliana*, as they now call it, none leaves ſo much ſcruple in the minds of ſome ſorts of men, as this, that when one's finger is laid cloſe upon the orifice of the little pipe, by which the air is wont to paſs from the receiver into the exhauſted cylinder, the pulp of the finger is made to enter a good way into the cavity of the pipe, which doth not happen without a conſiderable ſenſe of pain in the lower part of the finger. For moſt of thoſe, that are ſtrangers to hydroſtaticks, eſpecially if they be prepoſſeſſed with the opinions generally received, both in the Peripatetick and other ſchools, perſuade themſelves, that they feel the newly mentioned and painful protuberance of the pulp of the finger, to be effected, not by preſſure, as we would have it, but diſtinctly by attraction.

To this we are wont to anſwer, that common air being a body not devoid of weight, the phænomenon is clearly explicable by the preſſure of it: for, when the finger is firſt laid upon the orifice of the pipe, no pain nor ſwelling is produced, becauſe the air, which is in the pipe, preſſes as well againſt that part of the finger, which covereth the orifice, as the ambient air doth againſt the other parts of the ſame finger. But when by pumping, the air in the pipe, or the moſt part of it, is made to paſs out of the pipe into the exhauſted cylinder, then there is nothing left in the pipe, whoſe preſſure can any thing near countervail the undiminiſhed preſſure of the external air on the other parts of the finger; and conſequently, that air thruſts the moſt yielding and fleſhy part of the finger, which is the pulp, into that place, where its preſſure is unreſiſted, that is, into the cavity of the pipe, where this forcible intruſion cauſeth a pain in thoſe tender parts of the finger.

To give ſome viſible illuſtration of what we have been ſaying, as well as for other purpoſes, I thought on the following experiment.

WE took a glaſs pipe of a convenient length, and open at both ends, whoſe cavity was near about an inch in diameter, (ſuch a determinate breadth being convenient, though not neceſſary:) to one of the ends of this pipe we cauſed to be firmly tied on a piece of very fine bladder, that had been ruffled and oiled, to make it both very limber and unapt to admit water; and care was taken, that the piece of bladder tied on ſhould be large enough, not only to cover the orifice, but to hang looſe ſomewhat beneath it.

THIS done, we put the covered end of the pipe into a glaſs-body, or cucurbit, purpoſely made more than ordinarily tall, and the pipe being held in ſuch manner, as that the end of it reached almoſt, but not quite, to the bottom of the glaſs-body, we cauſed water to be poured, both into this veſſel, and into the pipe (at its upper orifice, which was left open) that the water might aſcend equally enough, both without and within the pipe. And when the glaſs-body was full of water, and the ſame liquor was level to it, or a little higher within the pipe, the bladder at the lower orifice was kept plump, becauſe the water within the pipe did, by its weight, preſs as forcibly downwards, as the external water in the large glaſs endeavoured to preſs it inwards and upwards.

ALL this being done, we cauſed part of the water in the pipe to be taken out of it, (which may be done either by putting in and drawing out a piece of ſpunge or of linnen, or more expeditiouſly by ſucking up part of the water with a ſmaller pipe to be immediately after laid aſide;) upon which removal of part of the internal water, that, which remained in the pipe, being no longer able, by reaſon of its want of weight, to preſs againſt the inſide of the bladder near as forcibly as it did before, the external water, whoſe weight was not leſſened, preſſed the ſides and bottom of the bladder, whereto it was contiguous into the cavity of the pipe, and thruſted it up therein ſo ſtrongly, that the diſtended bladder made a kind of either thimble or hemiſphere within the pipe. So that here we have a protuberance, like that above-mentioned of the finger, effected by pulſion, not attraction; and in a caſe, where there can be no juſt pretence of having recourſe to nature's abhorrence of a vacuum, ſince the upper orifice of the pipe being left wide open, the air may paſs in and out without reſiſtance.

THE like ſwelling of the bladder in the pipe we could procure without taking out any of the internal liquor, by thruſting the pipe deeper into the water; for then the external liquor having, by reaſon of its increaſe of depth, a greater preſſure on the outſide of the bladder, than the internal liquor had on the inſide of it, the bladder muſt yield to the ſtronger preſſure, and conſequently be impelled up.

IF the bladder lying looſe at the lower end of the pipe, the upper end were carefully cloſed with one's thumb, that the upper air might not get out, until the experimentor thought fit, and if the thus cloſed pipe were thruſt almoſt to the bottom of the water, the bladder would not be protuberant inwards, as formerly; becauſe the included air, by virtue of its ſpring, reſiſted from within the preſſure of the external water againſt the outſide of the bladder: but if the thumb, that ſtopped the pipe's upper orifice, were removed, the formerly compreſſed

preſſed air having liberty to expand itſelf, and its elaſticity being weakened thereby, the external water would with ſuddenneſs and noiſe enough, not to be unpleaſant to the ſpectators, drive up the bladder into the cavity of the pipe, and keep it there very protuberant.

To obviate an objection, that I foreſaw might be brought in by perſons not well verſed in hydroſtaticks, I cauſed the pipe fore-mentioned, or ſuch another, to be ſo bent near the lower end, as that the orifice of it ſtood quite on one ſide, and the parts of the pipe made an angle as near to a right one, as he, that blew it, could bring it to. This lower orifice being fitted with a bladder, and the pipe, with its contained liquor, being thruſt under water after the former manner, the lateral preſſure of the water forced the bladder into the ſhort and horizontal leg, and made it protuberate there, as it had done when the pipe was ſtraight.

Lastly, that the experiment might appear not to be confined to one liquor; inſtead of water, we put into the unbent pipe, as much red wine (whoſe colour would make it conſpicuous) as was requiſite to keep the bladder ſomewhat ſwelling outwards, when it was ſomewhat near the bottom of the water; and then it was manifeſt, that, according as we had foreſeen, the ſuperficies of the red liquor in the pipe was a good deal higher than that of the external water, and if the depth of both liquors were proportionably leſſened, the difference of height betwixt the two ſurfaces would indeed, as it ought to happen, decreaſe, but ſtill the ſurface of the wine would be the higher of the two, becauſe, being lighter in ſpecie than the common water, the æquilibrium between the preſſures of the two liquors upon the bladder would not be maintained, unleſs a greater height of wine compenſated its defect of ſpecifick gravity. And if the pipe was thruſt deeper into the water, then the bladder would be made protuberant inwards, as when the pipe had water in it. By which it appears, that theſe phænomena, without recourſe to attraction, may be explicated barely by the laws of the æquilibrium of liquors.

NEW EXPERIMENTS ABOUT THE PRESERVATION OF BODIES IN VACUO BOYLIANO.

PREFACE.

My willingneſs to make the bulk of the papers about the hidden qualities of the air leſs inconſiderable, by things, that were of affinity to the ſubject, inducing me to tumble over ſome of my *adverſaria*, I met among them with divers looſe notes, or ſhort memorials of ſome experiments I made ſeveral years ago (and ſome of a freſher date) about the preſervation of bodies by excluding the air. Wherefore I was eaſily perſuaded to ſubjoin theſe to the additional experiments laſt recited. For it ſeems not yet clear, by what manifeſt quality the excluſion of the air ſhould ſo much contribute to keep from putrefaction variety of bodies, that are uſually found very much diſpoſed to it. And therefore, till the cauſe of this preſervation be further penetrated, it may not be altogether impertinent to mention ſome experiments relating to it. And though theſe be only ſuch, as come now to hand, and were moſt of them ſet down rather as notes than relations; yet being faithfully regiſtered, and moſt of them having been made in *Vacuo Boyliano* (as they call it) they will probably be new, and ſo perhaps not altogether uſeleſs to naturaliſts, who may vary them, and requite me for them, by trying the ſame experiments, I made by the removal of the air, by the bare excluſion of adventitious air. For ſometimes through haſte I did not, and ſometimes for want of conveniency I could not try, whether the ſame phænomena would appear, if the ſame bodies were ſhut up with air in them, provided they were diligently kept from all commerce with the air about them.

SOME

CONSIDERATIONS

ABOUT THE

RECONCILEABLENESS

OF

REASON and RELIGION.

By *T. E.* a LAY-MAN.

To which is annexed by the PUBLISHER,

A DISCOURSE of Mr. *BOYLE*,

ABOUT THE

POSSIBILITY of the RESURRECTION.

The PUBLISHER to the READER.

THESE considerations about religion and reason, delivered by a person of an excellent genius and ability to consider the nature of the things he is wont to discourse upon, being fallen into my hands, nor being forbidden to publish them; I thought the subject so weighty, and the way of handling it both so discrete and solid, that I could not forbear recommending it to the press, being fully persuaded, the publick in general, as well as all persons in particular, that are concerned for the safety both of reason and religion, and consequently for their dignity as they are men, and their nobleness as they are Christians, will find sufficient cause to be pleased with the publication of it. To which I have nothing to add but that, whereas at the beginning of the following discourse there is mention made of its being to consist of two parts; one, to shew, that a Christian need not lay aside his reason; and the other, that he is not commanded to do so: the author thought fit to keep that paper, which concerned the latter, from now accompanying the former, which seems the most seasonable, and likeliest to make impressions on that sort of persons, whom he chiefly designs to persuade.

The PREFACE.

IT is the just grief, and frequent complaint of those, that take to heart the concerns of religion, that they see it now more furiously assaulted and studiously undermined than ever, not only by the vicious lives of men, but by their licentious discourses. I know, there have been vices in the world, as long as there have been men; and it is an observation as old as *Solomon*, *Eccles.* vii. 10. That men are apt to look upon their own times as worse than those, that preceded them. And because I remember too, that in reciting this complaint he disapproves it; I shall not dispute, whether other ages have been less faulty than this we live in: but this I think I may say with as much truth as grief, that among us here in *England*, the times, to which our memory can reach, have been less guilty, than the present time is, of a spreading and bold profaneness. For though many allowed themselves to court gold, and cups, and mistresses, little less than now they do; yet these were still acknowledged to be faults even by those, that committed them, and the precepts and the counsels of religion were neglected or disobeyed, but not their authority thrown off or affronted; men retaining yet such a kind of respect for her, as the elder son in the parable did for his father, when, receiving a command from him to go and work in his vineyard, he answered, "I go, Sir, though he went not, *Matth.* xxi. 30. But now too many of the vicious do not only scandalously violate the laws of religion, but question the truth, and despise the very name of it. They rather chuse to imitate the rebels in the other parable, and say of religion what they did of their lawful king, when they insolently declared, "That they would not "have him to reign over them, *Luke* xix. 14. They seek not to hide their sins, like *Adam*, but think either to cover or protect all others, by that greatest of all impiety; and instead of cheating conscience into silence, (as sinners, not impudent, are wont to do,) by deceitful promises of repenting hereafter of their sins, they endeavour to stifle or depose it, by maintaining, that repentance is a weakness of mind, and conscience ought not to be looked on as the vicegerent of a deity, whose very existence or providence they dispute.

AND that, which more troubled me, and made me most apprehend the spreading of this impiety, was, that it was propagated in a new way, that made me fear, the arguments not only of vulgar preachers, but even of learned divines themselves, would be much less fit than formerly to give a check to its progress. For, till of late, the generality of our infidels did, either as philologers, question the historical part of the scriptures, and perhaps cavil at some of the doctrines; or, if they employed philosophical arguments, as *Pomponatius* and *Vaninus* did, they borrowed them from *Aristotle*, or the Peripatetick school. And against both these sorts of adversaries, the learneder champions of the Christian religion, such as *Vives*, *Mornay*, and *Grotius*, had furnished divines with good and proper weapons. For, the historical part of the scriptures, and especially the miracles, were strongly confirmable by competent testimonies, and other moral proofs, sufficient in their kind. And *Aristotle* being himself a dark and dubious writer, and his followers being on that account divided into sects and parties, which for the most part had nothing to alledge but his single authority, it was not difficult to answer the arguments drawn from the Peripatetick philosophy; and, if that could not have been done, it had not been difficult to reject the doctrines themselves as false or precarious. But our new libertines take another and shorter way, (though I hope it will not be a more prosperous one,) to undermine religion. For, not troubling themselves to examine the historical or doctrinal parts of Christian theology, in such a way as Jews, Pagans, Mahometans, would do; these deny those very principles of natural theology, wherein the Christian, and those other differing religions agree, and which are supposed in almost all religions, that pretend to revelation, namely, the existence and providence of a Deity, and a future state (after this life is ended.) For, these libertines own themselves to be so upon the account of the Epicurean, or other mechanical principles of philosophy; and, therefore, to press them with the authorities wont to be employed by preachers, is improper, since they are so far from paying any respect to the venerable fathers of the church, that they slight the generality of the heathen philosophers themselves, judging no writers worthy of name, but those, that, like *Leucippus*, *Democritus*, *Epicurus*, &c. explicate things by matter and local motion; and therefore it is not to be expected, that they should reverence any more the Peripatetick arguments of *Scotus* or *Aquinas*, than the homilies of St. *Augustine*, or St. *Chrysostom*; and to give *Aristotle* himself the title of the philosopher, were enough to make some of them conclude the ascriber were no philosopher. And this, by the way, may excuse me for not having brought into the following papers the sentences of the fathers or the moralists, or the authority of *Aristotle*, or any of the school philosophers, which I should have declined to employ, though my frequent removes from place to place, when I was writing these papers, had not denied me the convenience of a library.

THINGS being at this pass, though the title of this discourse acknowledges the author of it to be a layman; yet I shall not beg pardon for the ensuing papers as for an intrenchment upon the ecclesiasticks. For besides that, though I know some functions, yet I know no truths of religion, that have the peculiarity of the shew-bread under the law, *Matth.* xii. 4. with which it was lawful only for the priests to meddle;

dle; I will not ſo far miſtruſt the charity of churchmen, as not to ſuppoſe, that they will rather thank than blame any man, that being not altogether a ſtranger to this warfare, offers them his aſſiſtance againſt the common enemy in ſo important a quarrel, and ſo great a danger. The fathers, and other divines, being wont to compare the church militant to a ſhip, it will not be an improper extenſion of the compariſon, to ſay, that, when the veſſel is threatened with ſhipwreck, or boarded by pirates, it may be the duty, not only of profeſſed ſeamen, but any private paſſenger, to lend his helping hand in that common danger. And I wiſh I were as ſure, that my endeavours will prove ſucceſsful, as I am, that ſuch churchmen, as I moſt eſteem, will think them neither needleſs nor unſeaſonable. Nay, perhaps my being a ſecular perſon may the better qualify me to work on thoſe I am to deal with, and may make my arguments, though not more ſolid in themſelves, yet more prevalent with men, that uſually (though how juſtly, let them conſider) have a particular pique at the clergy, and look with prejudice upon whatever is taught by men, whoſe intereſt is advantaged by having what they teach believed. And I was the more invited not to be a mere ſpectator, or a lazy deplorer of the danger I ſaw religion in, becauſe it ſeemed not unlikely, that philoſophical infidels, as they would be thought, would be leſs tractable to divines, though never ſo good humaniſts and antiquaries, than to a perſon, that reaſons with them upon their own grounds, and diſcourſes with them in their own way, having had a ſomewhat more than ordinary curioſity to acquaint himſelf with the Epicurean or Carteſian principles, and exerciſe himſelf in that philoſophy, which is very converſant with things corporeal, and ſtrives to explain them by matter and motion, and ſhakes off all authority (at leaſt that is not infallible.) Upon ſuch conſiderations as theſe, I complied with an occaſion I had of ſolemnly aſking reaſon the queſtion, that *Joſhua* once aſked the angel, that appeared to him in the plains of *Jericho*, "Art thou for "us, or for our adverſaries?" *Joſh.* v. 14; and of committing to paper thoſe thoughts, that ſhould occur to me on that ſubject. And this I the rather did, that I might thereby, as well contribute to my own ſatisfaction, as to that of my friends. For, as I think, that there is nothing, that belongs to this life, that ſo much deſerves our ſerious care, as what will become of us when we are paſt it; ſo I think, that he, who takes a reſolution, either to embrace or reject ſo important a thing, as religion, without ſeriouſly examining, why he does it, may happen to make a good choice, but can be but a bad chooſer. And, that I might not exclude, by too early a method, thoſe things, that, for aught I knew, might hereafter be pertinent and uſeful, I threw my reflections into one book, as into a repoſitory, to be kept there only as a heap of differing materials, that, if they appeared worth it, they might be afterwards reviewed, and ſorted, and drawn into an orderly diſcourſe. But, before I began to do what I intended, a ſucceſſion of accidents (wherewith it would not be proper to trouble the reader,) quite diverted me to employments of a very diſtant nature; ſo that theſe papers, being thrown by, did, for divers years, lie neglected, with many others, till at length the perſon, for whoſe peruſal I, in the firſt place, deſigned them, joined with ſome other intelligent friends, to urge me to ſend them abroad, though I was not in a condition to give them the finiſhing ſtrokes, or ſo much as to fill up ſeveral of the blanks, my haſte had made me leave to be ſupplied when I ſhould be at leiſure. And indeed, notwithſtanding the juſt averſeneſs I had from letting a piece ſo incomplete and uncorrected appear in this critical age; yet the hopes, they confidently gave me, that this piece, ſuch as it is, might not be unacceptable nor uſeleſs, were not, I confeſs, altogether groundleſs.

Novelty being a thing very acceptable in this age, and particularly to the perſons I am to deal with, to whom perhaps it is none of the leaſt endearments of their errors, I deſpair not, that it will ſomewhat recommend theſe papers, to which I deſigned to commit not tranſcripts of what I thought they may have already met with in authors, but ſuch conſiderations, as a ſerious attention, and the nature of the things I treated of, ſuggeſted to me; ſo that moſt of the things will perhaps be thought new; and ſome few things coincident with what they may have elſewhere met with, may poſſibly appear rather to have been ſuggeſted by conſidering the ſame ſubjects, to other authors and to me, than to have been borrowed by me of them. But ſome few things, I confeſs, I employ, that were commonly enough employed before, and, I hope, I may, in that, have done religion no diſſervice; for having taken notice, that ſome of the more familiar arguments had a real force in them, but had been ſo unwarily propoſed, as to be liable to exceptions, that had diſcredited them; I made it my care, by propoſing them more cautiouſly, to prevent ſuch objections, which alone kept their force from being apparent.

I was not unmindful of the great diſadvantage this tract was likely to undergo, partly for want of a more curious method, and partly becauſe my other occaſions required, that if I publiſhed it at all, it muſt be left to come abroad unpoliſhed and unfiniſhed. But though this inconvenience had like to have ſuppreſſed this diſcourſe; yet the force of it was much weakened by this conſideration, that this immethodical way of writing would beſt comply with what was deſigned and pretended in this paper, which was, not to write a compleat treatiſe of the ſubject of it, but only to ſuggeſt about it ſome of thoſe many conſiderations, that (queſtionleſs) might have occurred to (what I do not pretend to) an enlightened and penetrating intellect. And the loadſtone, divers of whoſe phænomena are mentioned in the body of this little tract, ſuggeſted ſomewhat to me in reference to the publication of it, by exciting in me a hope, that, if this diſcourſe have any thing near as much truth, as I endeavoured to furniſh it with, that truth will have its operation upon ſincere lovers of it, not-

notwithstanding the want of regularity in the method; as a good loadstone will not, by being rough and rudely shaped, be hindered from exercising its attractive and directive powers upon steel and iron.

As for the stile, I was rather shy than ambitious of bringing in the thorns of the school-men, or the flowers of rhetorick; for, the latter, though they had, of their own accord, sprung up under my pen, I should have thought improper to be employed in so serious and philosophical a subject: and as to the former, I declined them, in complaisance to the humour of my infidels, who are generally so prejudiced against the school-men, that scarce any thing can be presented them with more disadvantage than in a scholastick dress; and a demonstration will scarce pass for a good argument with some of them, if it be formed into a syllogism in mode and figure. That therefore, which I chiefly aimed at in my expressions, was significancy and clearness, that my reader might see, that I was willing to make him judge of the strength of my arguments, and would not put him to the trouble of divining in what it lay, nor inveigle him by ornaments of speech, to think it greater than it was. I was also led by my reason, as well as by my inclination, to be careful not to rail at my infidels: and though I have some cause to think, that many of them had their understandings debauched by their lives, and were sudeced from the church, not by *Diagoras* or *Pyrrho*, but by *Bacchus* and *Venus*; yet I treat them, as supposing them to be what they would be thought, friends to philosophy: and being but a layman, I did not think myself obliged to talk to them, as out of a pulpit, and threaten them with damnation, unless they believed me, but chose to discourse to them rather as to erring virtuosi, than wicked wretches.

This moderation, that I have used towards them, will, I hope, induce them to grant me two or three reasonable requests; whereof the first shall be, that they would not make a final judgment of these papers, till they have perused them quite through; especially having in their eye what is declared in the preamble, where both the design and scope of the whole discourse, and what it does not pretend to, is expressed. The next thing I am to request of them, and my readers, is, that they would not have the meaner thoughts of my arguments, for not being proposed with the confidence, wherewith many writers are wont to recommend weaker proofs. For I wrote to intelligent men, and, in the judgment of such, I never observed, that a demonstration ceased to be thought one, for being modestly proposed; but I have often known a good argument lose of its credit by the invidious title of a demonstration. And I must further beg my readers, to estimate my design in these papers, by the title of them, in which I do not pretend to make religion trample upon reason, but only to shew the reconcileableness of the one to the other, and the friendly agreement between them. I am a person, who looking upon it as my honour and happiness to be both a man, and a Christian, would neither write nor believe any thing, that might misbecome me in either of those two capacities. I am not a Christian, because it is the religion of my country, and my friends; nor, because I am a stranger to the principles, either of the atomical, or the mechanical philosophy. I admit no man's opinions in the whole lump, and have not scrupled, on occasion, to own dissents from the generality of learned men, whether philosophers or divines: and when I chuse to travel in the beaten road, it is not, because I find it is the road, but because I judge it is the way. Possibly I should have much fewer adversaries, if all those, that yet are so, had as attentively and impartially considered the points in controversy, as I have endeavoured to do. They would then, it is like, have seen, that the question I handle, is not, whether rational beings ought to avoid unreasonable assents, but whether, when the historical and other moral proofs clearly sway the scales in favour of Christianity, we ought to fly from the difficulties, that attend the granting of a Deity and Providence, to hypotheses, whether Epicurean, or others, that are themselves incumbered with confounding difficulties: on which account I conceive, that the question between them and me is not, whether they, or I, ought to submit to reason (for we both agree in thinking our selves bound to that;) but whether they or I submit to reason the fulliest informed, and least biassed by sensuality, vanity, or secular interest.

I reverence and cherish reason as much, I hope, as any of them; but I would have reason practice ingenuity as well as curiosity, and both industriously pry into things within her sphere, and frankly acknowledge, (what no philosopher, that considers, will deny,) that there are some things beyond it. And in these it is, that I think it as well her duty to admit revelation, as her happiness to have it proposed to her; and, even as to revelations themselves, I allow reason to judge of them, before she judges by them. The following papers will, I hope, manifest, that the main difference betwixt my adversaries and me is, that they judge upon particular difficulties and objections, and I upon the whole matter. And to conclude; as I make use of my watch to estimate time, when ever the sun is absent or clouded, but when he shines clearly forth, I scruple not to correct and adjust my watch by his beams cast on a dial; so, wherever no better light is to be had, I estimate truth by my own reason; but where divine revelation can be consulted, I willingly submit my fallible reason to the sure informations afforded by celestial light.

I should here put an end to this long preface, but that, to the things, which have been said concerning what I have written of my own, I see it is requisite, that I add a few words about what I quote from other writers; especially because in this very preface I mention my having intended to entertain my friend with my own thoughts. Of the citations therefore, that my reader will meet with in the following papers, I have this account to give him:

I.

NEW EXPERIMENTS, &c.

EXPERIMENT I.

A PIECE of roasted rabbet, being exactly closed up in an exhausted receiver the sixth of *November*, was two months, and some few days after taken out, without appearing to be corrupted, or sensibly altered in colour, taste, or smell.

EXPERIMENT II.

A SMALL glass-receiver, being half filled with pieces of white-bread, (part crust and part crumb) was exhausted, and secured the eleventh of *March*: the receiver being opened the first of *April*, part of the bread was shaken out, and appeared not to have been considerably, if at all sensibly, impaired in that time, save, that the outside of some pieces of crumb seemed to be a little, and but a little, less soft and white than before. There appeared no drops, or the least dew on the inside of the glass. The remaining bread was again secured soon after.

THE eighteenth of *April*, the bread was taken out again, and tasted much as it did the last time, the crust being also soft, and no drops of water appearing on the inside of the glass.

EXPERIMENT III.

THIS day (being the ninth of *March*) I opened a small exhausted and secured receiver, wherein, about the ninth of *December*, that is, about three months ago, we had included some milk: upon opening an access to the air, we found the milk well coloured, and turned partly into a kind of whey, and partly into a kind of soft curd. The taste was not offensive, only a little sourish like whey, and the smell was not at all stinking, but somewhat like that of sourish milk.

EXPERIMENT IV.

THE violet-leaves, that were put up, and freed and secured from air the fifth of *March*, being this day opened, (*April* the seventh) appeared not to have changed their shape, or colour, or consistence: for, as for their odour, it could not be well judged of, because he that included them had, for his own ease, contrary to my express direction, crushed many of them together in thrusting them down; and by such a violation of their texture, it is natural for violets to lose their fragrancy, and acquire an earthy smell.

EXPERIMENT V.

HAVING carefully placed some violets in an exhausted receiver, of a convenient size and bigness, and secured it from immediate commerce with the external air; the seventh month after we looked upon them again, and found they were not putrified, or resolved into any mucilaginous substance, but kept their shape entire, some of them retaining their colour, but more of them having so lost it, as to look like white violets.

EXPERIMENT VI.

NOVEMBER the fifth, we conveyed into a convenient shaped receiver some ounces of sheep's-blood, taken from an animal, that had been killed that afternoon. And after the exhaustion of the air, during which store of bubbles were generated in the liquor, that made it swell notably, the included blood was kept in a place, (whose warmth we judged equal to that of a digestive furnace) for twenty days; for one or two of the first of which, the blood seem to continue fluid, and of a florid colour, which afterwards degenerated into one, that tended more to blackness. On the twenty-fifth of *November*, we came to set in the external air, and found it to rush into the receiver, and the glass containing the blood, being held in a lightsome place, the most part of the bottom of it seemed to be thinly overlaid with a coagulated substance, of a higher colour than that, which swarm above it, which yet, though it appeared dark, and almost blackish in the glass, whilst it was looked on in the bulk, yet, if it was shook, those parts of it, that fell down along the inside of the glass, appeared of a deep, but fair colour. But whilst the blood continued in the glass, it was supposed not to stink, since, even when it was poured out, though its smell seemed to me (whose organs of smelling are tender) to have I know not what, that was offensive, yet to others it seemed to smell but as the blood of a newly killed dog.

EXPERIMENT VII.

SOME cream being put up and secured the seventeenth of *March*, in an exhausted receiver, did this day appear to be more thick, and almost butter-like at the top (whose superficies seemed rugged) than otherwhere; and afterwards by being well shaken together in the not inconveniently shaped glass, was easily enough reduced to butter, whose butter-milk, by the judgment of those, who were more used to deal in it than I, appeared not differing from ordinary butter-milk. And I found it had, like that, a grateful sourness. The butter was judged to be a little sourer than ordinary, but was not, as they speak, made.

[IN the entry of this experiment, blanks were left for the years; but the tenor of the words, and design of the experiment, and other circumstances, assure me, that the cream continued a year in the vessel.]

EXPERIMENT VIII.

FEBRUARY the eighteenth, we looked again upon three vials, that had been exhausted and secured the fifteenth of *September* last; the one of these had in it some slices of roasted beef, and the other some shivers of white bread, and the last some thin pieces of cheese; all which seemed to be free from putrefaction, and looked much as they did, when they were first put up: wherefore we thought not fit to let the air into the receiver, but left them as they were, to lengthen the designed trial.

EXPERIMENT IX.

FEBRUARY the eighteenth, there was a fourth vial, wherein, about six months before, viz. *August* the twelfth, had been inclosed and secured some July flowers and a rose; and yet these being kept in the same place with the rest, though they seemed a little moist, retained their shape and colour, especially the rose, which looked fresh enough to seem to have been gathered but lately.

N. B. THAT we observed not in any of these four receivers any great drops, or so much as dew in the upper parts, viz. those, that were situated above the included matter.

EXPERIMENT X.

JUNE the fourth, we left some strawberries in an exhausted receiver, and coming to look upon them after the beginning of *November*, we found them to be discoloured, but not altered in shape, nor affording any sign of corruption, by being at all mouldy: wherefore we thought fit to leave them still in the receiver for further trial.

EXPERIMENT XI.

MAY the second, 1669, a piece of roasted beef, secured *September* the fifteenth, appeared to be not at all altered: as did likewise a piece of cheese secured in another receiver; and some pieces of a French rose the same day (*September* the fifteenth) secured in a third.

N.B. THE flowers sealed up *August* the twelfth, 1668, being this day looked upon, appeared fresh, and consequently did so, after having been kept eight months and an half.

EXPERIMENT XII.

THERE was taken beer of eight shillings a barrel, of a year old, near a pint of which, *June* the seventeenth, was put into a convenient shaped glass, and it was afterwards exhausted and secured from the air; the most part of the month of *August* proved extraordinarily hot. Towards the latter end, there was, at several times, great thunder, which made the beer in our cellar, and in most of those of the neighbourhood, turn sour. The first of *September* the beer was opened, but did not seem to have degenerated into any sourness.

EXPERIMENT XIII.

BEING desirous to try, whether the thunder would have such effect upon ale, exactly stopped in glass vessels, as it often has on that liquor in the ordinary wooden casks, I caused some ale, moderately strong, to be put into a conveniently shaped receiver; and having exhausted the air, and secured a glass vessel, it was put into a quiet, but not cool, place: last week, which was about six weeks after the liquor had been inclosed, there happening some very loud thunder, and our beer, though the cask was kept in a good cellar, being generally noted to have been turned sour after this thunder; I staid yet a day or two longer, that the operation upon our included liquor might be the more certain and manifest; and then, permitting an access to the outward air, we took out the ale, and found it to be good drink, and not at all soured.

COMPARE this with the wish made in the Essay of the great efficacy of effluviums, chap. V. that such an experiment should be tried.

EXPERIMENT XIV.

SEPTEMBER the twenty-first, 1670, some blackberries, included in an exhausted receiver, were opened *June* the twentieth, 1673, and were found free from all mouldiness and ill-scent; only there was found some liquor, that was sour, which being taken out, the berries were secured again.

[AT the same time, was another parcel of the same berries exactly closed up in a receiver, whence the air was not pumped, to try what difference in the event would appear by this variation. But, coming in *October* the eleventh, 1673, to look upon the glass, we found it cracked, and the fruit all covered over with a thick mould. Nor was this the only vessel, wherein trials, made to preserve fruits, without any exhaustion of the air, miscarried.]

OCTOBER the eleventh, 1674, the same berries, being looked upon, appeared to have their colour altered, and much less black than before; but did not appear putrefied by either loss of shape, or by any stinking smell, nor was the least mouldiness observed to be on them, though they had been kept in the same receiver above four year.

THAT *fructus horarii*, especially so tender and juicy ones, should, without any additament, be preserved from putrefaction so many times longer than otherwise they would have lasted; as it is more than would be expected, so it may give hopes, that both odd and useful things of this kind, may be this way performed.

POSTSCRIPT.

THE foregoing experiments, as the memorials themselves declare, were all of them made *in vacuo Boyliano*, nor did I intend to set down any other: but meeting, among those memorials, with a short account of a couple of trials

trials made without the help of our pneumatic engine, I was induced to annex them, becauſe many may make the like, that will not be able to make ſuch as have been hitherto recited. And theſe two requiring no peculiarly ſhaped veſſels, it it thought, it may prove of ſome oeconomical, as well as phyſical uſe; if it be ſhewn by experience, that liquors hermetically ſealed the ordinary way in common bolt-heads, may be kept from ſouring very much beyond their uſual time of laſting.

JUNE the fourteenth, we put a convenient quantity of good ale into a bolt-head, and ſealed it up hermetically; the next year, on the fifth of *July*, we broke off the ſeal, and found the liquor very good, and without any ſenſible ſourneſs. The next day it was ſealed up again, and ſet by for thirteen months, at which time the neck of the glaſs being broken, the ale was found pretty ſour, and therefore the trial was proſecuted no farther: ſo that, though this liquor would not by this way of preſervation be kept from ſouring ſo long as the wine, to be mentioned in the following experiment, yet even a ſmall quantity of it was preſerved good at the leaſt above a year, which is very much longer than ale is wont to keep from ſouring.

JUNE the fourteenth, 1670, in a large bolt-head was hermetically ſealed up about a pint, by gueſs, of French claret wine, which, when we came to look upon, *July* the fifth, 1671, appeared very clear and high coloured, and had depoſited ſtore of feces at the bottom of the glaſs, but faſtened no tartar, that we could perceive to the ſides. Upon the breaking of the ſealed end of the glaſs, the byſtanders thought, that there was an eruption of included air or ſteams, and, above the ſurface of the wine, there appeared, to a pretty height, a certain white ſmoke almoſt like a miſt, and then gradually vaniſhed: the wine continued well-taſted, and was a little rough upon the tongue, but not at all ſour.

THE bolt-head was ſealed up again *July* the ſixth, 1671, and ſo ſet by till *Auguſt* the fifth, 1672, at which time it was opened again, and then the wine did ſtill taſte very well.

JUNE the twenty-ſixth, 1673, the bolt-head, with the ſame claret wine, was opened, and was found very good, and was ſealed up again.

OCTOBER the eleventh, 1674, the ſame claret wine was opened again, and appeared of a good colour, not ſour, but ſeemed ſomewhat leſs ſpiritous than other good claret wine, perhaps becauſe of the cold weather.

THIS, and the foregoing trial about the preſervation of ale, were made in Mr. *Oldenburg*'s houſe, and preſence.

An Account of the Two Sorts of the HELMONTIAN LAUDANUM, together with the Way of the Noble Baron *F. M. van Helmont*, (Son to the famous *Johannes Baptiſta*) of Preparing his LAUDANUM.

First publiſhed in the PHILOSOPHICAL TRANSACTIONS, No. cvii. p. 147, for *October* 26, 1674.

AS for the Helmontian Laudanum, you may uſe your own liberty in ſuſpecting the receipts, that go about of it. For the name itſelf ſeems ambiguous to me, who am well informed, that there are two ſorts of the Helmontian Laudanum; the one uſed by the elder *Helmont*, the other by his ſon. The former was as a great ſecret communicated to me by an expert chemiſt, ſent by a German prince to compliment *Johannes Baptiſta Van Helmont*, ſome of whoſe manuſcripts (one of which periſhed in the fire of *London*,) he procured, together with a way of making his Laudanum, which, having received from him fourteen or fifteen years ago, I carefully prepared, and thought my labour ſo well recompenſed by the extraordinary operations it happened not ſo much in my hands, as thoſe of learned phyſicians and others, to whom I preſented portions of it, that I ſhould have thought the chemiſt a benefactor to phyſic, if he would have made publick, or permitted me to publiſh the way of making ſo ſucceſsful a medicine. And though the acceſs to my laboratory was ſo free to ingenious men, who knew ſuch a medicine to be preparing there, that ſome of them might eaſily ſuppoſe themſelves maſters of the ſecret; yet my juſtice to the communicator, who made a great and deſerved benefit of the laudanum, made me take that care to conceal ſome circumſtances, that men may eaſily be much more confident than ſure, that they have the right way of making the medicine. Which becauſe I durſt not communicate,

nicate, meeting two years ago with that obliging and very ingenious perſon, *F. M.* baron *Van Helmont*, ſon to the famous *Johannes Baptiſta*, I obtained from him, by word of mouth, ſome directions about the laudanum he uſes, which though he confeſſed, and I ſoon perceived to be differing from his father's, yet he ſeemed to think it not inferior and more parable. But he having, for a certain reaſon, imparted to me his proceſs only by word of mouth; leſt it ſhould ſlip out of my memory, I ſoon after committed it to writing, as the particulars I gathered from his writing occurred to me; and at the next ſeaſon cauſed the medicine to be prepared in my laboratory, where the progreſs was often watched in my abſence by a very learned and induſtrious London doctor, who having, at my requeſt, made many trials with it, and in ſome caſes, where other laudanums had been found unavailable, both uſes it, and commends it, more than I could expect from ſo wary and judicious a man. This medicine being ſomewhat more cheap and eaſy to be made than the elder *Helmont*'s, the experience of its efficacy made me deſire of the younger a permiſſion to communicate it for the publick good, and to prevent thoſe ſpurious receipts, that go about of the Helmontian laudanum: which requeſt of mine being almoſt as ſoon granted as made, I think myſelf bound both to his own readineſs, to oblige the publick, and to acquaint them with his way of making ſo conſiderable a medicine, as I practiſed it; though if I had received his directions in writing, they might have been more full and methodical. But though I perceived, that he ſometimes a little varies his preparations; yet that laudanum proving very ſucceſsful, that was made according to the annexed paper, I think it will not be amiſs to keep to that: which I wiſh could have been publiſhed, before the ſeaſon of the quinces were ſo far advanced. And I ſhall the more hope it may come abroad before it be quite too late, if you pleaſe to afford it room in the papers, wherewith I am informed you intend this week to gratify the curious:

Laudanum Helmontii Junioris.

TAKE of opium a quarter of a pound, and of the juice of quinces four pound at leaſt **; the opium being cut into very thin ſlices, and then as it were minced, to reduce it into ſmaller parts, is to be put into, and well mixed with, the liquor, (firſt made lukewarm) and fermented with a moderate heat for eight or ten days, rather more than leſs; then filter* it, and having infuſed in it of cinnamon, nutmeg and cloves, of each an ounce ||, let them ſtand three or four days more; if it be a full week, it may be ſo much the better; then filter § the liquor once more, having let it boil a whalm or two after the ſpices have been put in: this being done evaporate away the ſuperfluous water to the conſiſtence of an extract, or to what conſiſtence you pleaſe.

LASTLY, incorporate very well with it two, or at moſt three ounces of the beſt ſaffron reduced to fine † powder.

ACCORDING to the conſiſtence you deſire to have your medicine of, you may order it ſo, as either to make it up into a maſs of pills (in which form I have cauſed it to be given,) or keep it in a liquid form; but in this later caſe the evaporation muſt have been made more ſparingly, that after the putting in of the ‡ ſaffron it may not grow too thick. In this form the doſe may be from five or ſix drops to ten or fewer, according to circumſtances; and of the pills a ſomewhat leſs quantity is required.

** (For near five pound would perhaps do better.) * (Which circumſtance the author often omits, though I do not.) || (The author ſometimes uſes half an ounce more of each ſpice) § Or ſtrain it well through a canvaſs-bag. † (Sometimes the author inſtead of the powder makes uſe of as much extract as can be obtained from that quantity of ſaffron.) ‡ (Or its extract.)

SOME

1. That I had written the confiderations and diftinctions, to which they are annexed, before I met with thefe cited paffages, which I afterwards inferted in the margent, and other vacant places of my epiftle. 2. That thefe paffages are not borrowed from books, that treat of the truth of the Chriftian religion, or of Chriftian theology at all, but are taken from authors, that write of philofophical fubjects, and are by me applied to mine, which are ufually very diftant from theirs. 3. If you then afk me, why I make ufe of their authority, and did not content my felf with my own ratiocinations? I have this to anfwer; that my defign being to convince another, who had no reafon to look upon my authority, and whom I had caufe to fufpect to have entertained fome prejudices againft any reafons, that fhould come from one, that confeffedly aimed at the defending of the Chriftian religion, I thought it very proper and expedient to let him fee, that divers of the fame things (for fubftance) that I delivered in favour of that religion, had been taught as philofophical truths by men, that were not profeffed divines, and were philofophers, and fuch ftrict naturalifts, too, as to be extraordinarily careful, not to take any thing into their philofophy upon the account of revelation. And on this occafion let me obferve to you, that there are fome arguments, which being clearly built upon fenfe, or evident experiments, need borrow no affiftance from the refutation of any of the propofers or approvers, and may, I think, be fitly enough compared to arrows fhot out of a crofs-bow, and bullets fhot out of a gun, which have the fame ftrength, and pierce equally, whether they be difcharged by a child, or a ftrong man. But then, there are other ratiocinations, which either do, or are fuppofed to depend, in fome meafure, upon the judgment and fkill of thofe, that make the obfervations, whereon they are grounded, and their ability to difcern truth from counterfeits, and folid things from thofe, that are but fuperficial ones: and thefe may be compared to arrows fhot out of a long-bow, which make much the greater impreffion, by being fhot by a ftrong and fkilful archer. And therefore when we queftion, what doctrines ought, or ought not to be thought reafonable, it does not a little facilitate a propofition's appearing (not contrary, but) confonant to reafon, that it is looked upon as fuch by thofe, that are acknowledged the mafters of that faculty.

SOME CONSIDERATIONS ABOUT THE RECONCILEABLENESS OF REASON and RELIGION.

PART I.

As to what you write in your friend's name, near the bottom of the firft page of your letter, perhaps I fhall not miftake, if I guefs, that, when he feems but to propofe a queftion, he means an objection; and covertly intimates, that I, among many others, am reduced to that pafs, that to embrace our religion, we muft renounce our reafon; and confequently, that to be a Chriftian, one muft ceafe to be a man, and much more, leave off being a philofopher.

WHAT liberal conceffions foever fome others have been pleafed to make on fuch an occafion as this, they do not concern me; who, being afked but my own opinion, do not think my felf refponfible for that of others. And therefore, that I may frame my anfwer fo, as to meet both with the obvious fenfe of the queftion,

queftion, and the intimated meaning of him, that propofes it, I fhall roundly make a negative reply, and fay, " that I do not think, " that a Chriftian, to be truly fo, is obliged to " forego his reafon; either by denying the " dictates of right reafon, or by laying afide " the ufe of it."

I doubt not, but this anfwer is differing enough from what your friend expects; and perhaps thofe grants, that have been made by the indulgence, or inadvertency of many perfons, eminent for being pious or learned, may make you yourfelf ftartle at this declaration: and therefore, though you will not, I know, expect an anfwer to what objections your friend may make, fince he has expreffed but what he thinks ought to be a Chriftian's opinion, not what he has to object againft what is fo; yet, to fatisfy thofe fcruples, that you your felf may retain, I fhall endeavour (but with the brevity, that becomes a letter) to acquaint you by themfelves, with fome of the pofitive inducements, that have led me to this opinion, and interweave fome others, in anfwering the chief objections, that I think likely to be made againft it.

AND this preamble, fhort as it is, will, I hope, ferve to keep you from miftaking my defign; which, as you may gather from what I have intimated, is not to give you the pofitive proofs of the Chriftian religion (which is not here to be expected from a bare defendant,) but to give you fome fpecimens of fuch general confiderations, as may probably fhew, that the matter (or effential doctrines) peculiar to the Chriftian religion is not fo repugnant to the principles of true natural philofophy, as that to believe them, a man muft ceafe to act like a rational man, any more than he would be obliged to do by embracing other religions, or even the tenets, that have been held without difparagement to their intellectuals, by the mere philofophers themfelves; which laft claufe I add, becaufe, I prefume, you do not expect, that I fhould be fo folicitous to vindicate the Chriftian's belief of a Deity from being irrational; fince, befides that, perhaps your friend would think himfelf affronted to be dealt with as an Atheift, without having profeffed himfelf one, the acknowledgement of a Deity blemifhes the Chriftian's reafon no more, than it does that of men of all religions, not to fay of all mankind; and imports no other contradiction to reafon, than what has been judged to be none at all by the greateft, if not by all, of the philofophers, that were famed for being guided by reafon (without revelation.) And I fhall venture to add (upon the by) that, as I do not, for my own part, think the Atheift's philofophical objections (if your friend had produced them) to be near fo confiderable for weight or number, as not only thofe few, that deny a God, but many of thofe, that believe one, are wont to think; fo the Chriftian is not reduced, as is imagined, to make the Being of a Deity a mere poftulatum; fince, befides the philofophical arguments he can alledge in common with the beft champions for a Deity, he has a peculiar hiftorical proof, that may fuffice; the miracles performed by Chrift and his followers being fuch, that if the matter of fact can be (as it may be) well evinced, they will not only prove the reft of the Chriftian religion, but in the firft place, that there muft be a God to be the Author of them.

BUT though of the two things, which my defign obliges me to endeavour the making good of, the moft natural order feems to be, that I fhould firft fhew, that no precepts of Chriftianity do command a man to lay afide his reafon in matters of religion; and then, that there is nothing in the nature of the Chriftian doctrine itfelf, that makes a man need to do fo; yet I think it not amifs in treating of thefe two fubjects to invert the order, and firft confider that difficulty, which is the principal, and which your friend and you jointly defire to have my thoughts of; namely, " Whether there be a neceffity for a Chriftian " to deny his reafon?" And then we fhall proceed to examine, whether, though he need not difclaim his reafon, it be neverthelefs his duty fo to do?

SECTION I.

TO proceed then to the confiderations, that make up the former part of this epiftle; I fhall, in the firft place, diftinguifh betwixt that, which the Chriftian religion itfelf teaches, and that, which is taught by this or that church, or fect of Chriftians, and much more by this or that particular divine or fchoolman.

I need not perfuade you, who cannot but know it fo well already, that there are many things taught about the attributes and decrees of God, the myfteries of the trinity, and incarnation, and divers other theological fubjects, about which not only private Chriftians, but churches of Chriftians do not at all agree. There are too many men, whofe ambition, or boldnefs, or felf-conceit, or intereft, leads them to obtrude upon others, as parts of religion, things, that are not only ftrangers, but oftentimes enemies to it. And there are others, who, out of an indifcrete devotion, are fo folicitous to encreafe the number, and the wonderfulnefs of myfteries, that, to hear them propofe and difcourfe of things, one would judge, that they think it is the office of faith, not to elevate, but to trample upon reafon; and that things are then fitteft to be believed, when they are not clearly to be proved or underftood. And indeed, when, on the one fide, I confider the charitable defign of the gofpel, and the candid fimplicity, that fhines in what it propofes, or commands; and on the other fide, what ftrange and wild fpeculations and inferences have been fathered upon it, not only in the metaphyfical writings of fome fchoolmen, but in the articles of faith of fome churches; I cannot but think, that if all thefe doctrines are parts of the Chriftian religion, the apoftles, if they were now alive, would be at beft but *Catechumeni*; and I doubt not, but many of the nice points, that are now much valued and urged by fome, would be as as well disapproved by St. *Paul*, as by *Aristotle*; and should be as little entertained by an orthodox divine, as a rigid philosopher. I do not therefore allow all that for gospel, which is taught for such in a preacher's pulpit, or even a professor's chair. And therefore, if scholastick writers, of what church soever, take the liberty of imposing upon the Christian religion their metaphysical speculations, or any other merely human doctrines, as matters of faith, I who, not without some examination, think metaphysicks themselves not to have been for the most part over-well understood, and applied, shall make bold to leave all such private doctrines to be defended by their own broachers or abettors; and shall deny, that it will follow, that in case of this multitude of placets, which some bold men have been pleased to adopt into the catalogue of Christian verities, any, or all, should be found inconsistent with right reason, the Christian religion must be so too. For by that name I understand only that system of revealed truths, that are clearly delivered in the scriptures; or by legitimate and manifest consequences deduced thence. And by this one declaration, so many unnecessary, and perhaps hurtful retainers to Christianity will be at once thrown off, that I doubt not, but if you consider the matter aright, you will easily discern, that by this first distinction I have much lessened the work, that is to be done by those, that are to follow it.

SECTION II.

IN the next place, among the things, that seem not rational in religion, I make a great difference between those, in which unenlightened reason is manifestly a competent judge, and those, which natural reason itself may discern to be out of its sphere.

You will allow me, that natural theology is sufficient to evince the existence of the deity; and we know, that many of the old philosophers, that were unassisted by revelation, were, by the force of reason, led to discover and confess a God, that is, a being supremely perfect; under which notion, divers of them expressly represent him. Now, if there be such a being, it is but reasonable to conceive, that there may be many things relating to his nature, his will, and his management of things, that are without the sphere of mere or unassisted reason. For, if his attributes and perfections be not fully comprehensible to our reason, we can have but inadequate conceptions of them; and since God is a Being, *toto cælo*, as they speak, differing from all other beings, there may be some things in his nature, and in the manner of his existence, which is without all example, or perfect analogy, in inferior beings. For we see, that even in man himself, the co-existence and intimate union of the soul and body, that is, an immaterial and a corporeal substance, is without all president or parallel in nature. And though the truth of this union may be proved; yet, the manner of it was never yet, nor perhaps ever will be, in this life clearly understood, (to which purpose I shall elsewhere say more.) Moreover, if we suppose God to be omnipotent, (that is, to be able to do whatever involves no contradiction, that it should be done,) we must allow him to be able to do many things, that no other agent can afford us any examples of, and some of them perhaps, such as we, who are but finite, and are wont to judge of things by analogy, cannot conceive how they can be performed. Of the last sort of things may be the recollecting a sufficient quantity of the scattered matter of a dead human body, and the contriving of it so, that (whether alone, or with some addition of other particles) upon a re-conjunction with the soul, it may again constitute a living man, and so effect that wonder we call the resurrection. Of the latter sort, is the creation of matter out of nothing, and much more the like production of those rational and intelligent beings, human souls. For as for angels (good or bad) I doubt, whether mere philosophy can evince their existence, though I think it may the possibility thereof. And since we allow the Deity a wisdom equal to this boundless power, it is but reasonable to conceive, that these unlimited attributes conspiring, may produce contrivances and frame designs, which we men must be unable (at least of ourselves) sufficiently to understand, and to reach to the bottom of. And by this way of arguing, it may be made to appear, that there may be many things relating to the Deity above the reach of unenlightened human reason. Not that I affirm all these things to be, in their own nature, incomprehensible to us, (though some of them may be so,) when they are once proposed; but that reason, by its own light, could not discover them particularly, and therefore it must owe its knowledge of them to divine revelation. And if God vouchsafes to disclose those things to us, since not only he must needs know about his own nature, attributes, &c. what we cannot possible know unless he tells us, and since we know, that whatever he tells us is infallibly true, we have abundant reason to believe rather what he declares to us concerning himself and divine things, than what we should conclude or guess about them, by analogy to things of a nature infinitely distant from his, or by maxims framed according to the nature of inferior beings. If therefore he clearly reveal to us, that there is in the Godhead, three distinct persons, and yet that God is one, we, that think ourselves bound to believe God's testimony in all other cases, ought sure not to disbelieve it concerning himself, but to acknowledge, that in an unparalleled and incomprehensible Being, there may be a manner of existence not to be paralleled in any other being, though it should never be understood by us men, who cannot clearly comprehend, how in ourselves two such distant natures, as that of a gross body, and an immaterial spirit, should be united, so as to make up one man. In such cases therefore, as we are now speaking of, there must indeed be something, that looks like captivating one's reason,

reason, but it is a submission, that reason itself obliges us to make; and he, that in such points as these, believes rather what the divine writings teach him, than what he would think, if they had never informed him, does not renounce or enslave his reason, but suffers it to be pupil to an omniscient and infallible instructor, who can teach him such things, as neither his own mere reason, nor any others could ever have discovered to him.

I thought to have here dismissed this proposition, but I must not omit to give it a confirmation afforded me by chance, (or rather providence:) for, since I writ the last paragraph, resuming a philosophical enquiry, I met, in prosecuting it, with a couple of testimonies of the truth of what I was lately telling you, which are given, not by divines or schoolmen, but by a couple of famous mathematicians, that have both led the way to many of the modern philosophers, to shake off the reverence wont to be borne to the authority of great names, and have advanced reason in a few years, more than such as *Vaninus* and *Pomponatius* would do in many ages; and have always boldly, and sometimes successfully, attempted to explain intelligibly those things, which others scrupled not, either openly or tacitly, to confess inexplicable.

THE first of these testimonies I met with in a little French treatise put out by some mathematician, who, though he conceals his name, appears, by his way of writing, to be a great virtuoso, and takes upon him to give his readers in French the new thoughts of *Galilæo*, by making that the title of his book. This writer then speaking of a paradox (which I but recite) of *Galilæo*'s, that makes a point equal to a circle, adds, *& per consequent l'on peut dire*, i. e. and consequently one may say, that all circles are equal between themselves, since each of them is equal to a point. For though the imagination be over-powered by this idea, or notion, yet reason will suffer itself to be persuaded of it. I know (continues he) divers other excellent persons, (besides *Galilæo*) who conclude the same thing by other ways; but all are constrained to acknowledge, that indivisible and infinite are things, that do so swallow up the mind of man, that he scarce knows what to pitch on, when he contemplates them. For it will follow, from *Galilæo*'s speculation, &c. which passage I have cited, to shew you, that *Galilæo* is not the only philosopher and mathematician, who has confessed his reason quite passed about the attributes of what is infinite.

Pag. 22, 23.

THE other testimony I mentioned to you, is that of the excellent *Des Cartes*, in the second part of his principles of philosophy, where, speaking of the circle to be made by matter moving through places still lesser and lesser, he has this ingenious acknowledgment; *fatendum tamen est* (says he) *in motu isto aliquid reperiri, quod mens quidem nostra percipit esse verum, sed tamen quo pacto fiat non comprehendit, nempe divisionem quarundam particularum materiæ in infinitum, sive indefinitam, atque in tot partes ut nulla cogitatione determinare possimus tam exiguam, quin intelligamus ipsam in alias adhuc minores reipsa esse divisam.* And in the close of the next paragraph, he gives this for a reason, why, though we cannot comprehend this indefinite division, yet we ought not to doubt of the truth of it, that we discern it to be of that kind of things, that cannot be comprized by our minds, as being but finite.

IF then such bold and piercing wits, and such excellent mathematicians, are forced to confess, that not only their own reason, but that of mankind, may be passed and non-pulssed about quantity, which is an object of contemplation natural, nay, mathematical, and which is the subject of the rigid demonstrations of pure mathematicks; why should we think it unfit to be believed, and to be acknowledged, that in the attributes of God, who is essentially an infinite Being, and an *ens singularissimum*, and in divers other divine things, of which we can have no knowledge without revelation, there should be some things, that our finite understandings cannot, especially in this life, clearly comprehend.

SECTION III.

TO this consideration, I shall, for affinity's sake, subjoin another, which I leave to your liberty to look upon as a distinct one, or as an enlargement and application of the former.

I consider then, that there is a great difference between a doctrine's being repugnant to the general and well-weighed rules or dictates of reason, in the forming of which rules, it may be supposed to have been duly considered; and its disagreeing with axioms, at the establishment whereof the doctrine in question was probably never thought on. There are several rules, that pass current, even among the most learned men, and which are indeed of very great use, when restrained to those things whence they took their rise, and others of the like nature; which yet ought not to overthrow those divine doctrines, that seem not consonant to them. For the framers of these rules having generally built them upon the observations they had made of natural and moral things, since (as we lately argued) reason itself cannot but acknowledge, there are some things out of its sphere, we must not think it impossible, that there may be rules, which will hold in all inferior beings for which they were made; and yet not reach to that infinite and most singular Being, called God, and to some divine matters, which were not taken into consideration, when those rules were framed. And indeed, if we consider God as the author of the universe, and the free establisher of the laws of motion, whose general concourse is necessary to the conservation and efficacy of every particular physical agent, we cannot but acknowledge, that, by with-holding his concourse, or changing these laws of motion, which depend perfectly upon his will, he may invalidate most, if not all the axioms and theorems of natural philosophy: these supposing the course of nature, and especially the established laws of motion among the parts of the universal matter, as those upon which

which all the phænomena depend. It is a rule in natural philosophy, that *causæ necessariæ semper agunt quantum possunt*; but it will not follow from thence, that the fire must necessarily burn *Daniel*'s three companions, or their clothes, that were cast by the Babylonian king's command into the midst of a burning fiery furnace, when the author of nature was pleased to withdraw his concourse to the operation of the flames, or supernaturally to defend against them the bodies, that were exposed to them. That men once truly dead cannot be brought to life again, hath been in all ages the doctrine of mere philosophers; but though this be true, according to the course of nature, yet it will not follow, but that the contrary may be true, if God interpose either to recal the departed soul, and re-conjoin it to the body, if the organization of this be not too much vitiated, or by so altering the fabrick of the matter, whereof the carcase consists, as to restore it to a fitness for the exercise of the functions of life. Agreeably to this, let me observe to you, that, though it be unreasonable to believe a miraculous effect, when attributed only to a mere physical agent; yet the same thing may reasonably be believed, when ascribed to God, or to agents assisted with his absolute or supernatural power. That a man born blind should, in a trice, recover his sight, upon the application of clay and spittle, would justly appear incredible, if the cure were ascribed to one, that acted as a mere man; but it will not follow, that it ought to be incredible, that the Son of God would work it. And the like may be said of all the miracles performed by Christ, and those apostles and other disciples of his, that acted by virtue of a divine power and commission. For in all these, and the like cases, it suffices not to make one's belief irrational, that the things believed are impossible to be true, according to the course of nature; but it must be shewn, either that they are impossible, even to the power of God, to which they are ascribed, or that the records, we have of them, are not sufficient to beget belief in the nature of a testimony; which latter objection against these relations is foreign to our present discourse. And as the rules about the power of agents will not all of them hold in God, so I might shew the like, if I had time, concerning some of his other attributes: insomuch, that even in point of justice, wherein we think we may freeliest make estimates of what may or may not be done, there may be some cases, wherein God's supreme dominion, as maker and governor of the world, places him above some of those rules; I say, some, for I say not above all those rules of justice, which oblige all inferior beings, without excepting the greatest and most absolute monarchs themselves. I will not give examples of his power of pardoning or remitting penalties, which is but a relaxing of his own right; but will rather give an instance in his power of afflicting and exterminating men, without any provocation given him by them. I will not here enter upon the controversy *de jure Dei in creaturas*, upon what it is founded, and how far it reaches. For, without making myself a party in that quarrel, I think, I may safely say, that God, by his right of dominion, might, without any violation of the laws of justice, have destroyed, and even annihilated *Adam* and *Eve*, before they had eaten of the forbidden fruit, or had been commanded to abstain from it. For man being as much and as entirely God's workmanship as any of the other creatures, unless God had obliged himself by some promise or pact, to limit the exercise of his absolute dominion over him, God was no more bound to preserve *Adam* and *Eve* long alive, than he was to preserve a lamb, or a pidgeon; and therefore, as we allow, that he might justly recal the lives he had given those innocent creatures, when he pleased, (as actually he often ordered them to be killed, and burned in sacrifice to him:) so he might, for the declaration of his power to the angels, or for other reasons, have suddenly taken away the lives of *Adam* and *Eve*, though they had never offended him. And upon the same grounds he might, without injustice, have annihilated, I say not, damned their souls; he being no more bound to continue existence to a nobler, than a less noble creature; as he is no more bound to keep an eagle, than an oyster always alive. I know, there is a difference betwixt God's resuming a being he lent *Adam*, and his doing the same to inferior creatures: but that disparity, if it concern any of his attributes, will concern some other than his justice; which allowed him to resume, at pleasure, the being he had only lent them, or lay any affliction on them, that were lesser than that good could countervail. But, mentioning this instance only occasionally, I shall not prosecute it any further, but rather mind you of the result of this and the foregoing considerations; which is, that divinely revealed truths may seem to be repugnant to the dictates of reason, when they do but seem to be so: nor does Christianity oblige us to question such rules, as to the cases they were framed for, but the application of them to the nature of God, who has already been truly said to be *ens singularissimum*, and to his absolute power and will; so that we do not reject the rules we speak of, but rather limit them; and when we have restrained them to their due bounds, we may safely admit them.

FROM men's not taking notice of, or not pondering this necessary limitation of many axioms delivered in general terms, seems to have proceeded a great error, which has made so many learned men presume to say, that this or that thing is true in philosophy, but false in divinity, or on the contrary: as for instance, that a virgin, continuing such, may have a child, is looked upon as an article, which theology asserts to be true, and philosophy pronounces impossible. But the objection is grounded upon a mistake, which might have been prevented by wording the propositions more warily and fully. For though we grant, that physically speaking, it is false, that a virgin can bring forth a child; yet that signifies no more, than that, according to the course of nature, such a thing cannot come to pass; but speaking abso-

absolutely and indefinitely, without confining the effect to mere physical agents, it may safely be denied, that philosophy pronounces it impossible, that a virgin should be a mother. For why should the author of nature be confined to the ways of working of dependent and finite agents? And to apply the answer to the divines, that hold the opinion I oppose, I shall demand, why God may not out of the substance of a woman form a man, without the help of a man, as well as at the beginning of the substance of a man he formed a woman without the concurrence of a woman? And so that iron, being a body far heavier (*in specie*, as they speak,) will, if upheld by no other body, sink in water, is a truth in natural philosophy; but since physicks themselves lead men to the acknowledgment of a God, it is not repugnant to reason, that, if God please to interpose his power, he may (as in *Elisha*'s case) make iron swim, either by with-holding his concourse to the agents, whatever they be, that cause gravity in bodies, or perhaps by other ways unknown to us; since a vigorous loadstone may, as I have more than once tried, keep a piece of iron, which it touches not, swimming in the air, though this thin body must contribute far less, than water would, to the sustaining it aloft.

That strict philosopher *Des Cartes*, who has with great wit and no less applause attempted to carry the mechanical powers of matters higher than any of the modern philosophers; this naturalist, I say, that ascribes so great a power to matter and motion, was so far from thinking, that what was impossible to them, must be so to God too, that, though he were urged by a learned adversary with an argument, as likely as any to give him a strong temptation to limit the omnipotence of God; yet even on this occasion he scruples not to make this ingenious and wary acknowledgment, and that in a private letter; "For my part, says "he, I think we ought never to say of any "thing, that it is impossible to God. For all, "that is true and good, being dependent on "his all-mightiness, I dare not so much as say, "that God cannot make a mountain without "a valley, or cannot make it true, that one "and two shall not make three; but I say "only, that he has given me a soul of such a "nature, that I cannot conceive a mountain "without a valley, nor that the aggregate of "one and of two shall not make three, &c. "and I say only, that such things imply a contradiction in my conception." And consonantly to this, in his Principles of Philosophy he gives, on a certain occasion, this useful caution,—*Quod ut satis tutò & sine errandi periculo aggrediamur, eâ nobis cautelâ est utendum, ut semper quàm maxime recordemur, & Deum autorem rerum esse infinitum, & nos omnino finitos.*

Vol. II. Lettre 6.

Parte Prima. Art. 24.

SECTION IV.

In the next place, I think we ought to distinguish between reason considered in itself, and reason considered in the exercise of it, by this or that philosopher, or by this or that man, or by this or that company, or society of men, whether all of one sect or of more.

If you will allow me to borrow a school-phrase, I shall express this more shortly by saying, I distinguish between reason *in abstracto*, and *in concreto*. To clear this matter, we may consider, that whatever you make the faculty of reason to be in itself, yet the ratiocinations it produces are made by men, either singly reasoning, or concurring in the same ratiocinations and opinions; and consequently, if these men do not make the best use of their reasoning faculty, it will not be necessary, that what thwarts their ratiocinations, must likewise thwart the principles or the dictates of right reason. For man having a will and affections as well as an intellect, though our dijudications and tenents ought indeed (in matters speculative) to be made and pitched upon by our unbiassed understandings; yet really our intellectual weaknesses, or our prejudices, or prepossession by custom, education, &c. our interest, passions, vices, and I know not how many other things, have so great and swaying an influence on them, that there are very few conclusions, that we make, or opinions, that we espouse, that are so much the pure results of our reason, that no personal disability, prejudice, or fault, has any interest in them.

About the Diversity of Religion.

This I have elsewhere more amply discoursed of on another occasion; wherefore I shall now add but this, that the distinction, I have been proposing, does (if I mistake not) reach a great deal further than you may be aware of. For not only whole sects, whether in religion or philosophy, are in many cases subject to prepossessions, envy, ambition, interest, and other misleading things, as well as single persons; but, which is more considerable to our present purpose, the very body of mankind may be embued with prejudices, and errors, and that from their childhood, and some also even from their birth, by which means they continue undiscerned, and consequently unreformed.

This you will think an accusation as bold as high; but to let you see, that the philosophers, you most respect, have made the same observation, though not applied to the same case, I must put you in mind, that Monsieur *Des Cartes* begins his principles of philosophy with taking notice, that, because we are born children, we make divers unright judgments of things, which afterwards are wont to continue with us all our lives, and prove radicated prejudices, that mislead our judgments on so many occasions, that he elsewhere tells us, he found no other way to secure himself from their influence, but once in his life solemnly to doubt of the truth of all, that he had till then believed, in order to the re-examining of his former dijudications. But I remember, our illustrious *Verulam* warrants a yet further prejudice against many things, that are wont to be looked on as the suggestions of reason. For having told us, that the mind of man is besieged with four differing kinds of idols or phantasms, when he comes to enumerate them, he teaches, that there are not only such, as men get by conver-

sation

ſation and diſcourſe one with another, and ſuch as proceed from the divers hypotheſes or theories and opinions of philoſophers, and from the perverſe ways of demonſtration, and likewiſe ſuch as are perſonal to this or that man, proceeding from his education, temperament, ſtudies &c. but ſuch as he calls *idola tribus*, becauſe they are founded in humane nature itſelf, and in the very tribe or nation of mankind; and of theſe he particularly diſcourſes of ſeven or eight; as, that the intellect of man has an innate propenſity to ſuppoſe in things a greater order and equality than it finds, and that being unable to reſt or acquieſce, it does always tend further and further; to which he adds divers other innate prejudices of mankind, which he ſolicitouſly as well as judiciouſly endeavours to remove.

Now, if not only ſingle philoſophers, and particular ſects, but the whole body of mankind be ſubject to be ſwayed by innate and unheeded prejudices and proclivities to errors about matters, that are neither divine, nor moral, nor political, but phyſical, where the attainment of truth is exceeding pleaſant to human nature, and is not attended with conſequences diſtaſteful to it: why may not we juſtly ſuſpect not only this or that philoſopher, or particular ſect; but the generality of men, of having ſome ſecret propenſities to err about divine things, and indiſpoſitions to admit truths, which not only detect the weakneſſes of our nature, and our perſonal diſabilities, and thereby offend or mortify our pride and our ambition, but ſhine into the mind with ſo clear, as well as pure and chaſte a light, as is proper both to diſcover to ourſelves and others our vices and faults, and oftentimes to croſs our deſigns and intereſts?

AND to this purpoſe we may take notice, that divers of thoſe very idols, which my lord *Bacon* obſerves to beſiege, or pervert men's judgments in reference to things natural, may probably have the ſame kind of influence (and that much ſtronger) on the minds of men in reference to ſupernatural things. Thus he takes notice, that if ſome things have once pleaſed the underſtanding, it is apt to draw all others to comport with, and give ſuffrage to them, though perhaps the inducements to the contrary belief be either more numerous or more weighty. He obſerves alſo, that man is apt to look upon his ſenſes and other perceptions as the meaſures of things, and alſo that the underſtanding of man is not ſincerely diſpoſed to receive the light of truth, but receives an infuſion as it were of adventitious colours, (that diſguiſe the light) from the will and affections, which makes him ſooner believe thoſe things, that he is deſirous ſhould be true, and reject many others upon accounts, that do no way infer their being falſe. Now if we apply theſe things to divine truths (to which it were well they were leſs juſtly applicable) and conſider, that in our youth we generally converſe but with things corporeal, and are ſwayed by affections, that have them for their objects, we ſhall not much wonder, that men ſhould be very prone, either to frame ſuch notions of divine things, as they were wont to have about others of a far different and meaner nature; or elſe to reject them for not being analogous to thoſe things, which they have been uſed to employ for the meaſures of truth and falſity. And if we conſider the inbred pride of man, which is ſuch, that if we believe the ſacred ſtory, even *Adam* in paradiſe affected to be like God, knowing good and evil: we ſhall not ſo much marvel, that almoſt every man in particular makes the notions he has entertained already, and his ſenſes, his inclinations, and his intereſts, the ſtandards, by which he eſtimates and judges of all other things, whether natural or revealed. And as *Heraclitus* juſtly complained, that every man ſought the knowledge of natural things in the microcoſm, that is, himſelf, and not in the macrocoſm, the world; ſo we may juſtly complain, that men ſeek all the knowledge, they care to find, or will admit, either in theſe little worlds themſelves, or from that great world, the univerſe; but not from the omniſcient author of them both. And laſtly, if even in purely phyſical things, where one would not think it likely, that rational beings ſhould ſeek truth with any other deſigns than of finding and enjoying it, our underſtandings are ſo univerſally biaſſed, and impoſed upon by our wills and affections; how can we admire, eſpecially if we admit the fall of our firſt parents, that our paſſions and intereſts, and oftentimes our vices, ſhould pervert our intellects about thoſe revealed truths; divers of which we diſcern to be above our comprehenſions, and more of which we find to be directly contrary to our inclinations?

SECT. V.

AND now it will be ſeaſonable for me to tell you, that I think, there may be a great difference betwixt a thing's being contrary to right reaſon, or ſo much as to any true philoſophy, and its being contrary to the received opinions of philoſophers, or to the principles or concluſions of this or that ſect of them.

FOR here I may juſtly apply to my preſent purpoſe what *Clemens Alexandrinus* judiciouſly ſaid on another occaſion, that philoſophy was neither Peripatetical, nor Stoical, nor Epicurean, but whatſoever among all thoſe ſeveral parties was fit to be approved.

AND indeed, if we ſurvey the hypotheſes and opinions of the ſeveral ſects of philoſophers, eſpecially in thoſe points, wherein they hold things repugnant to theological truths, we ſhall find many of them ſo ſlightly grounded, and ſo diſagreeing among themſelves, that a ſevere and inquiſitive examiner would ſee little cauſe to admit them upon the bare account of his being a philoſopher, though he did not ſee any to reject them upon the account of his being a Chriſtian. And in particular, as to the Peripateticks, who by invading all the ſchools of *Europe* (and ſome in *Aſia* and *Africk*) have made their ſect almoſt Catholick, and have produced divers of the famous queſtioners of Chriſtianity in the laſt age, and the first of this; the world begins to be apace undeceived, as to many of their doctrines, which were as confidently taught and believed for many ages, as those, that are repugnant to our religion; and there is now scarce any of the modern philosophers, that allow themselves the free use of their reason, who believes any longer, that there is an element of fire lodged under the supposed sphere of the moon; that heaven consists of solid orbs; that all celestial bodies are ingenerable and incorruptible; that the heart, rather than the brain, is the origine of nerves; that the torrid zone is uninhabitable; and I know not how many other doctrines of the Aristotelians, which our Corpuscularian philosophers think so little worth being believed, that they would censure him, that should now think them worthy to be solicitously confuted; upon which score I presume you will allow me to leave those, and divers others, as weak Peripatetick conceits, to fall by their own groundlessness.

BUT you will tell me, that the Epicureans, and the Somatici, that will allow nothing but body in the world, nor no author of it but chance, are more formidable enemies to religion than the Aristotelians. And indeed I am apt to think they are so, but they may well be so, without deserving to have any of their sects looked upon as philosophy itself, there being none of them, that I know of, that maintain any opinion inconsistent with Christianity, that I think may not be made appear to be also repugnant to reason, or at least not demonstrable by it. You will not expect I should descend to particulars, especially having expresly discoursed against the Epicurean hypotheses of the origine of the world in another paper; and therefore, I shall observe to you in general, that the Cartesian philosophers, who lay aside all supernatural revelation in their inquiries into natural things, do yet both think, and, as to the two first of them, very plausibly prove, the three grand principles of *Epicurus*, that the little bodies he calls atoms are indivisible, that they all have their motion from themselves, and, that there is a vacuum in *rerum naturâ*, to be as repugnant to mere reason, as the Epicureans think the notion of an incorporeal substance, or the creation of the world, or the immortality of the soul. And as for the new Somatici, such as Mr. *Hobbes* (and some few others) by what I have yet seen of his, I am not much tempted to forsake any thing, that I looked upon as a truth before, even in natural philosophy itself, upon the score of what he (though never so confidently) delivers, by which hitherto I see not, that he hath made any great discovery either of new truths, or old errors. An honourable member of the Royal Society hath elsewhere purposely shewn, how ill he has proved his own opinions about the air, and some other physical subjects, and how ill he has understood and opposed those of his adversary. But to give you in this place a specimen, how little their repugnancy to his principles of natural philosophy ought to affright us from those theological doctrines they contradict, I shall here examine the fundamental maxim of his whole physicks, that nothing is removed but by a body contiguous and moved; it having been already shewn (by the gentleman newly mentioned) that, as to the next to it, which is, that there is no vacuum, whether it be true or no, he has not proved it.

[IF no body can possibly be moved, but by a body contiguous and moved, as Mr. *Hobbes* teaches; I demand, how there comes to be local motion in the world? For, either all the portions of matter, that composed the universe, have motion belonging to their nature, which the Epicureans affirmed for their atoms; or some parts of matter have this motive power, and some have not; or else none of them have it, but all of them are naturally devoid of motion. If it be granted, that motion does naturally belong to all parts of matter, the dispute is at an end, the concession quite overthrowing the hypothesis. If it be said, that naturally some portions of matter have motion, and others not, then the assertion will not be universally true: for though it may hold in the parts, that are naturally moveless, or quiescent, yet it will not do so in the others, there being nothing, that may shew a necessity, why a body, to which motion is natural, should not be capable of moving, without being put into motion by another contiguous and moved. And if there be no body, to which motion is natural, but every body needs an outward movent, it may well be demanded, how there comes to be any thing locally moved in the world? which yet constant and obvious experience demonstrates, and Mr. *Hobbes* himself cannot deny. For if no part of matter have any motion but what it must owe to another, that is contiguous to it, and being itself in motion, impels it; and if there be nothing but matter in the world, how can there come to be any motion amongst bodies, since they neither have it upon the score of their own nature, nor can receive it from external agents? If Mr. *Hobbes* should reply, that the motion is impressed upon any of the parts of the matter by God, he will say that, which I most readily grant to be true, but will not serve his turn, if he would speak congruously to his own hypothesis. For I demand, whether this supreme Being, that the assertion has recourse to, be a corporeal or an incorporeal substance? If it be the latter, and yet be the efficient cause of motion in bodies, then it will not be universally true, that whatsoever body is moved, is so by a body contiguous and moved. For, in our supposition, the bodies, that God moves, either immediately, or by the intervention of any other immaterial being, are not moved by a body contiguous, but by an incorporeal spirit. But because Mr. *Hobbes*, in some writings of his, is believed to think the very notion of an immaterial substance to be absurd, and to involve a contradiction; and because it may be subsumed, that if God be not an immaterial substance, he must by consequence be a material and corporeal one, there being no *medium negationis*, or third substance, that is none of those

those two. I answer, that, if this be said, and so that Mr. *Hobbes*'s deity be a corporeal one, the same difficulty will recur, that I urged before. For this body will not, by Mr. *Hobbes*'s calling or thinking it divine, cease to be a true body; and consequently a portion of divine matter will not be able to move a portion of our mundane matter, without it be itself contiguous and moved; which it cannot be, but by another portion of divine matter, so qualified to impress a motion, nor this again, but by another portion.

AND besides that it will breed a strange confusion, in rendering the physical causes of things, unless an expedient be found, to teach us how to distinguish accurately the mundane bodies from the divine, (which will perhaps prove no easy task;) I see not yet, how this corporeal deity will make good the hypothesis I examine. For I demand, how this divine matter comes to have this local motion, that is ascribed to it? If it be answered, that it hath it from its own nature, without any other cause, since the Epicureans affirm the same of their atoms, or merely mundane matter, I demand, how the truth of Mr. *Hobbes*'s opinion will appear to me, to whom it seems as likely by the phænomena of nature, that occur, that mundane matter should have a congenit motion, as that any thing, that is corporeal, can be God, and capable of moving it; which to be, it must, for aught we know, have its subsistence divided into as many minute parts, as there are corpuscles and particles in the world, that move separately from their neighbouring ones. And, to draw towards a conclusion, I say, that these minute divine bodies, that thus moved those portions of mundane matter, concerning which Mr. *Hobbes* denies, that they can be moved but by bodies contiguous and moved, these divine substances, I say, are, according to the late supposition, true bodies, and yet are moved themselves, not by bodies contiguous and moved, but by a motion, which must be innate, derived or flowing from their very essence or nature, since no such body is pretended to have a being, as cannot be referred as a portion, either to the mundane, or the divine matter. In short, since local motion is to be found in one, if not in both, of these two matters, it must be natural to (at least some parts of) one of them in Mr. *Hobbes*'s hypothesis; for, though he should grant an immaterial being, yet it could not produce a motion in any body, since, according to him, no body can be moved, but by another body contiguous and moved.]

AS then to this grand position of Mr. *Hobbes*, though, if it were cautiously proposed, as it is by *Des Cartes*, it may perhaps be safely admitted, because *Cartesius* acknowledges the first impulse, that set matter a moving, and the conservation of motion once begun, to come from God; yet, as it is crudely proposed by the favourers of Mr. *Hobbes*, I am so far from seeing any such cogent proof for it, as were to be wished for a principle, on which he builds so much, (and which yet is not at all evident by its own light,) that I see no competent reason to admit it.

I expect your friend should here oppose to what I have been saying, that formerly recited sentence, that is so commonly employed in the schools, as well of divines as of philosophers: that such or such an opinion is true in divinity, but false in philosophy; or, on the contrary, philosophically true, but theologically false.

UPON what warrant those, that are wont to employ such expressions, ground their practice, I leave to them to make out; but as to the objection itself, as it supposes these ways of speaking to be well grounded, give me leave to consider, that philosophy may signify two things, which I take to be very differing.

FOR, first, it is most commonly employed to signify a system, or body of the opinions, and other doctrines of the particular sect of those philosophers, that make use of the word. As when an Aristotelian talks of philosophy, he usually means the Peripatetick, as an Epicurean does the Atomical, or a Platonist the Platonick.

BUT we may also, in a more general, and no less just acception of the term, understand by philosophy, a comprehension of all those truths or doctrines, which the natural reason of man, freed from prejudices and partiality, and assisted by learning, attention, exercise, experiments, &c. can manifestly make out, or, by necessary consequence deduce from clear and certain principles.

THIS being briefly premised, I must, in the next place, put you in mind of what I formerly observed to you, that many opinions are maintained by this, or that sect of Christians, or perhaps by the divinity-schools of more than one or two sects, which either do not at all belong to the Christian religion, or, at least, ought not to be looked upon as parts of it, but upon supposition, that the philosophical principles and ratiocinations, upon which, and not upon express or mere revelation, they are presumed to be founded, are agreeable to right reason.

AND having premised these two things, I now answer more directly to the objection; that, if philosophy be taken in the first sense above-mentioned, it's teaching things repugnant to theology, especially taking this word in the more large and vulgar sense of it, will not cogently conclude any thing against the Christian religion. But, if philosophy be taken in the latter sense for true philosophy, and divinity only for a system of those articles, that are clearly revealed as truths in the scriptures; I shall not allow any thing to be false in philosophy so understood, that is true in divinity so explained, till I see some clearer proof of it, than I have yet met with. I have had occasion, in the foregoing discourse, to say something, that may be applied to the point under debate; and in the following part of this letter, I shall have occasion to touch upon it again: and therefore I shall now say but this in short, that it is not likely, that God, being the author of reason as well as revelation, should make it men's duty to believe as as true, that, which there is just reason to reject as false.

THERE is indeed a sense, wherein the phrases, I disapprove, may be tolerated. For if by saying, that such a thing is true in divinity, but false in philosophy, it were meant, that if the doctrine were proposed to a mere philosopher, to be judged of according to the principles of his sect, or at most, according to what he, being supposed not to have heard of the Christian religion, or had it duly proposed to him, would reject it, the phrase might be allowed, or at least indulged. But then we must consider, that the reason, why such a philosopher would reject the articles of Christian faith, would not be, because they could by no mediums be possibly proved, but because these doctrines, being founded upon a revelation, which he is presumed either not to have heard of, or not to have had sufficiently proposed to him, he must, as a rational man, refuse to believe them upon the score of their prooflesness. And the same philosopher, supposing him to be a true one, though he will be very wary, how he admits any thing as true, that is not proved, if it fall properly under the cognizance of philosophy; yet he will be as wary, how he pronounces things to be false or impossible, in matters, which he discerns to be beyond the reach of mere natural reason, especially if sober and learned men do very confidently pretend to know something of those matters by divine revelation, which though he will not easily believe to be a true one, yet he will admit, in case it should be proved true, to be a fit medium to evince truths, which, upon the account of mere natural light, he could not discover or embrace. To be short, such a philosopher would indeed reject some of the articles of our faith hypothetically, i. e. upon supposition, that he need employ no other touchstone to examine them by, than the principles and dictates of natural philosophy, that he is acquainted with (upon which score I shall hereafter shew, that divers strange chemical experiments, and other discoveries would also be rejected;) but yet he would not pronounce them false, but upon supposition, that the arguments, by which they lay claim to divine revelation, are incompetent in their kind. For as he will not easily believe any thing within the sphere of nature, that agrees not with the established laws of it; so he will not easily adventure to pronounce one way or other in matters, that are beyond the sphere of nature: he will indeed, as he justly may, expect as full a proof of the divine testimony, that is pretended, as the nature of the thing requires and allows; but he will not be backward to acknowledge, that God, to whom that testimony is ascribed, is able to know and to do many more things, than we can explicate how he can discover, or imagine how any physical agent can perform.

[SINCE I proposed to you this fifth consideration, I happened to light on a passage in Princip. Philos. part. prima. Artic. 25. *Des Cartes*'s principles, which affords of what I have been discovering the suffrage of a philosopher, that is wont to be accused of excluding theology too scrupulously out of his philosophy. His words are so full to my present purpose, that I need not, to accommodate them to it, alter one of them, and therefore shall transcribe them just as they lie: *Si fortè nobis Deus de seipso, vel aliis, aliquid revelet, quod naturales ingenii nostri vires excedat, qualia sunt mysteria incarnationis & trinitatis, non recusabimus illa credere, quamvis non clarè intelligamus, nec ullo modo mirabimur, multa esse tum in immensa ejus natura, tum etiam in rebus ab eo creatis, quæ captum nostrum excedant.*]

AND let me add on this occasion, that whereas the main scruples, that are said to be suggested by philosophy against some mysterious articles of religion, are grounded upon this, that the modus, as they speak of those things, is not clearly conceivable, or at least, is very hardly explicable; these objections are not always so weighty, as perhaps, by the confidence, wherewith they are urged, you may think them. For, whereas I observed to you already, that there are divers things maintained by school divines, which are not contained in the scripture, that observation is chiefly applicable to the things we are considering; since in several of these nice points, the scripture affirms only the thing, and the schoolmen are pleased to add the modus: and as by their unwarrantable boldness, the school divines determine many things without book; so the scruples and objections, that are made against what the scripture really delivers, are usually grounded upon the erroneous or precarious assertions of the school philosophers, who often give the title of metaphysical truths to conceits, that do very little deserve that name, and to which a rigid philosopher would perhaps think, that of sublime nonsense more proper. But of this I elsewhere say enough, and therefore shall now proceed to the consideration I chiefly intended, viz. that from hence, that the modus of a revealed truth is either very hard, or not at all explicable, it will not necessarily follow, that the thing itself is irrational, provided the positive proofs of its truth be sufficient in their kind. For even in natural things philosophers themselves do and must admit several things, whereof they cannot clearly explicate or perhaps conceive the modus. I will not here mention the origin of substantial forms as an instance in this kind, because, though it may be a fit one as to the Peripatetick philosophy, yet not admitting, that there are any such beings, I will take no farther notice of them; especially because, for a clear instance to our present purpose, we need go no further than our selves, and consider the union of the soul and body in man. For who can physically explain, both how an immaterial substance should be able to guide or determine, and excite the motions of a body, and yet not be able to produce motion in it (as by dead palsies, great faintnesses, &c. it appears the soul cannot,) and, which is far more difficult, how an incorporeal substance should receive such impressions from the motions of a body, as to be thereby affected with real pain and pleasure;

pleasure; to which I elsewhere add some other properties of this union, which, though not taken notice of, are perhaps no less difficult to be conceived and accounted for. For how can we comprehend, that there should be naturally such an intimate union betwixt two such distant substances, as an (incorporeal) spirit and a body, as that the former may not, when it pleases, quit the latter, which cannot possibly have any strings or chains, that can tie, or fasten to it that, which has no body, on which they may take hold. And I there shew, that it is full as difficult, physically to explicate, how these so differing beings come to be united, as how they are kept from parting at pleasure, both the one and the other being to be resolved into the mere appointment of God. And if to avoid the abstruseness of the modus of this conjunction betwixt the rational soul and the human body, it be said, as it is by the Epicureans, that the former is but a certain contexture of the finer and most subtle parts of the latter, the formerly proposed abstruseness of the union betwixt the soul and the body will indeed be shifted off; but it will be by a doctrine, that will not much relieve us. For those, that will allow no soul in man but what is corporeal, have a modus to explain, that I doubt they will always leave a riddle. For of such I desire, that they would explain to me, (who know no effects, that matter can produce, but by local motion and rest, and the consequences of it,) how mere matter, (let them suppose it as fine as they please, and contrive it as well as they can) can make syllogisms, and have conceptions of universals, and invent speculative sciences and demonstrations, and in a word do all those things, which are done by man, and by no other animal; and he, that shall intelligibly explicate to me the modus of matters, framing theories and ratiocinations, will, I confess, not only instruct me, but surprize me too.

And now give me leave to make this short reflection on what has been said in this section, compared with what formerly I said in the first section; that if on the one hand we lay aside all the irrational opinions, that the schoolmen and other bold writers have unwarrantably fathered on Christian religion, and on the other hand all the erroneous conceits repugnant to Christianity, which the schoolmen and others have proofless fathered upon philosophy, the seeming contradictions betwixt solid divinity and true philosophy will appear to be but few, as I think the real ones will be found to be none at all.

SECTION VI.

The next consideration I shall propose, is, that a thing may, if singly or precisely considered, appear unreasonable, which yet may be very credible, if considered as a part of, or a manifest consequence from a doctrine, that is highly so.

Of this I could give you more instances in several arts and sciences, than I think fit to be here specified; and therefore I shall content my self to mention three or four.

When astronomers tell us, that the sun, which seems not to us a foot broad, nor considerably bigger than the moon, is above a hundred and threescore times bigger than the whole globe of the earth, which yet is forty times greater than the moon; the thing thus nakedly proposed seems very incredible. But yet, because astronomers very skilful in their art have, by finding the semidiameter of the earth, and observing the parallaxes of the planets, concluded the proportion of these three bodies to be such as has been mentioned, or thereabout, even learned and judicious men of all sorts, (philosophers, divines, and others,) think it not credulity to admit what they affirm.

So the relations of earthquakes, that have reached divers hundreds of miles; of eruptions of fire, that have at once overflown and burned vast scopes of land; of the blowing up of mountains by their own fires; of the casting up of new islands in the sea itself, and other prodigies of too unquestionable truth; (for I know what work ignorance and superstition have made about other prodigies:) if they were attested but by slight and ordinary witness, they would be judged incredible, but we scruple not to believe them, when the relations are attested with such circumstances, as make the testimony as strong, as the things attested are strange.

If ever you have considered, what *Clavius*, and divers other geometricians teach upon the sixteenth proposition of the third book of *Euclid*, (which contains a theorem about the tangent, and the circumference of a circle,) you cannot but have taken notice, that there are scarce greater paradoxes delivered by philosophers or divines, than you will find asserted by geometricians themselves. And though of late the learned Jesuit *Tacquet*, and some rigid mathematicians, have questioned divers of those things, yet even what some of these severe examiners confess to be geometrically demonstrable from that proposition, contains things so strange, that philosophers themselves, that are not well acquainted with that proposition and its corollaries, can scarce look upon them as other than incomprehensible, or at least incredible, things; which yet, as improbable as they are considered in themselves, even rigid demonstrators refuse not to admit, because they are legitimately deducible from an acknowledged truth.

And so also among the magnetical phænomena there are divers things, which, being nakedly proposed, must seem altogether unfit to be believed, as indeed having nothing like them in all nature; whereas those, that are versed in magnetick philosophy, even before they have made particular trials of them, will look upon them as credible, because, how great paradoxes soever they may seem to others, they are consonant and consequent to the doctrine of magnetism, whose grand axioms (from what cause soever magnetisms are to be deriv'd) are sufficiently

ciently manifest; and therefore a magnetical philosopher would not, though an ordinary philosopher would, think it unreasonable to believe, that one part of the same loadstone should draw a needle to it, and the other part drive the same needle from it; and that the needle in a seaman's compass, after having been carried many hundred leagues (through differing climates, and in stormy weather) without varying its declination, may, upon a sudden, without any manifest cause, point at some part of the horizon several whole degrees distant from that, which it pointed to before. To which might here be added divers other scarce credible things, which either others or I have tried about magnetical bodies; but I shall hereafter have occasion to take notice of some of them in a fitter place.

WHEREFORE, when something delivered in, or clearly deduced from scripture, is objected against, as a thing, which it is not reasonable to believe, we must not only consider, whether, if it were not delivered in that book, we should upon its own single account think it fit or unworthy to be believed; but whether or no it is so improbable, that it is more fit to be believed, that all the proofs, that can be brought for the authority of the scripture, are to be rejected, than that this thing, which comes manifestly recommended to our belief by that authority, is worthy to be admitted: I say, "manifestly recommended by that authority," because that, if the thing be not clearly delivered in scripture, or be not clearly and cogently deduced thence, so far as that clearness is wanting, so far the thing itself wants of the full authority of the scripture, to impose it on our assent.

[PERHAPS it will procure what I have said the better reception, if I add a couple of testimonies not of any modern bigots, no, nor of any devout fathers of the church; but of two modern authors of sects, and who in their kinds have been thought extremely subtle reasoners, and no less rigid exacters of reason in whatever they admitted.

THE first passage I shall alledge, is the confession of *Socinus*, who in his second epistle to *Andreas Dudithius*, speaks thus: *Jam verò ut rem in pauca conferam, quod ad meas aliorúmve opiniones, quæ novitatis præ se ferunt speciem, attinet, mihi ita videtur; si detur, scripturam sacram ejus esse authoritatis, ut nullo modo ei contradici possit, ac de interpretatione illius omnis duntaxat sit scrupulus*, (which he allows) *nihil, utut verisimile aut ratione conclusum videatur, afferri contra eas possit, quod ullarum sit virium, quotiescunque illæ sententiis atque verbis illius libri aut rationibus liquidò inde deductis probatæ atque assertæ fuerint.* Which confession of *Socinus* is surpassed by that of his champion *Smalcius*, to be produced elsewhere in this paper. The other passage I met with in the excellent Monsieur *Des Cartes*'s principles of philosophy, where discoursing of the either infinite or indefinite division of the particles of matter, which is necessary to make them fill exactly all the differingly figured spaces, through which various motions do sometimes make them pass; he confesses, as he well may, that the point is exceedingly abstruse, and yet concludes: *Et quamvis quomodo fiat indefinitâ ista divisio, cogitatione comprehendere nequeamus, non ideo tamen debemus dubitare, quin fiat, quia clarè percipimus illam necessario sequi ex natura materiæ nobis evidentissimè cognitâ*, &c.]

Part II. Artic. 34, 35.

AND in this place it may be seasonable, as well as pertinent, to take notice of three or four particulars, which, though they be in some measure implied in the former general consideration, yet deserve to be distinctly inculcated here, both for their importance, and because they may as well be deduced as corollaries from the foregoing discourse, as be confirmed by the proofs I shall add to each of them. Of these the first shall be this, that we must not presently conclude a thing to be contrary to reason, because learned men profess or even complain, that they are not able clearly to comprehend it, provided there be competent proof, that it is true, and the thing be primary or heteroclite.

FOR it is not always necessary to the making the belief of a thing rational, that we have such a comprehension of the thing believed as may be had, and justly required in ordinary cases; since we may be sure of the truth of a thing, not only by arguments suggested by the nature of the thing itself clearly understood by us, but by the external testimony of such a witness, as we know will not deceive us, and cannot (at least in our case) be reasonably suspected to be himself deceived. And therefore it may in some cases suffice to make our belief rational, that we clearly discern sufficient reason to believe, that a thing is true, whether that reason spring from the evidence and cogency of the extrinsick motives we have to believe, or from the proofs suggested to us by what we know of the thing believed, nay, though there be something in the nature of that thing, which does puzzle and pose our understanding.

THAT many things, that are very hard, and require a great attention, and a good judgment to be made out, may yet be true, will be manifest from what I shall, within a page or two, note about divers geometrical demonstrations, which require, besides a good stock of knowledge in those matters, an almost invincible patience, to carry so many things along in one's mind, and go through with them. That also there are other things, which, though they be as manifestly existent, as those newly mentioned can be demonstratively true, are yet of so abstruse a kind, that it is exceeding difficult to frame clear and satisfactory notions of their nature, we might learn, if we were inquisitive enough, even from some of the most obvious things; such as, for instance, matter and time: As to the former whereof, (matter,) though the world and our own bodies be made of it, yet the ideas, that are wont to be framed of it, even by the greatest clerks, are incumbered with too great difficulties (some of which I elsewhere mention) to be easily acquiesced in by considering men. And as for the latter, (time,) though that justly celebrated saying of *Augustine*, *Si nemo ex me quærat, quid sit tempus, scio; si quærenti explicare velim, nescio*; seem in the first

first part of it to own a knowledge of what time is, yet by the latter part, (wherein he confesses he cannot declare what it is,) I am not only allowed to believe, that he could not propose an intelligible idea of it, but invited to think, that, in the first part of the sentence, he only meant, that when he did not attentively consider the nature of it, he thought he understood it, or that he knew, that there is such a thing as time, though he could not explain what it is.

And indeed, though time be that, which all men allow to be, yet, if *per impossibile* (as the schools speak) a man could have no other notion or proof of time and eternity, (even such eternity as must be conceded to something,) than what he could collect from the best descriptions of its nature and properties, that are wont to be given; I scarce doubt, but he would look upon it as an unintelligible thing, and incumbered with too many difficulties to be fit to be admitted into a wise man's belief. And this perhaps you will grant me, if you have ever put yourself to the penance of perusing those confounding disputes and speculations about time and eternity, that partly in *Aristotle* and his commentators, and partly among the scoolmen, and others, are to be met with upon these abstruse subjects. And no wonder, since the learned *Gassendus* and his followers have very plausibly (if not solidly) shewn, that duration (and time is but duration measured) is neither a substance nor an accident, which they also hold of space; about which the altercations among philosophers and schoolmen are but little, if at all, inferior to those about time. And I the rather choose to mention these instances of time and space, because they agree very well with what I intimated by the expression of primary or heteroclite things.

To which may be referred some of those things, that are called spiritual or supernatural, about which the same considerations may have place, especially by reason of this affinity between them, that when we treat of either, some proofs may in certain cases be sufficient, in spite of such objections, as in other (and more ordinary cases) would invalidate arguments seemingly as strong as those proofs.

If it be here objected, that I am too bold in venturing, without the precedence or authority of learned men, to introduce so great a difference betwixt other things, and those, which I call primary and heteroclite; I answer, that I shall not solicitously enquire, whether any others have had the same thoughts, that I proposed; since, whether they be new or no, they ought not to be rejected, if they be rational.

And I have this inducement to suppose, that there ought to be in some cases a great difference between them and other things, and consequently between the judgments we make of the ways of arguing about them, and about other things; so that they are exceeding difficult, to be clearly conceived and explicated by our imperfect faculties, and by that difficulty, apt to make what men say of them, though true, to be less satisfactory and acquiesced in, than things not more true or rational, suggested upon enquiries about subjects more familiar, or which are, at least, more proportionate to our faculties: for those abstruse things, of which we have been speaking, being such, as either have no proper and clear genus, by the help of which they may be comprehended, or have not any thing in nature, that is (sufficiently, like them) by a resemblance to which we may conceive them; or being perhaps, both primary and heteroclite too, as not being derived from the common physical causes of other things, and having a nature widely differing from the rest of things; it is no wonder, that our limited and imperfect understandings should not be able to reach to a full and clear comprehension of them; but should be swallowed up with the scruples and difficulties, that may be suggested by a bold and nice enquiry into things, to which there seems to belong, in some respect or other, a kind of infinity.

Upon these, and other considerations of kin to them, I count it not irrational to think, that things primary and heteroclite, as also by a parity of reason, some things immaterial and supernatural, may be sufficiently proved in their kind, if there be such a positive proof of them, as would be competent and satisfactory, in case there were no considerable objections made against the thing proved (especially supposing, that the asserted doctrine be not incumbered with much greater inconveniencies than the contrary doctrine, or than any other, proposed concerning that subject:) nay, I know not, why we may not, in judging of primary, and of immaterial things, safely enough prefer that opinion, which has the more cogent positive proofs, though it seem liable to somewhat the greater inconveniencies; because, in such cases, our understanding is gratified with what it most requires in all cases, that is, competent positive inducements to assent; and it is not confounded by the objections, because a disability to answer them directly, and fully, may very well proceed, either from the too abstruse nature of the thing, or the limitedness and weakness of our human intellects.

And thus we may render a reason, why, when we discourse of such uncommon matters, we may sometimes reasonably acquiesce in proofs, in spight of such objections, as in ordinary cases would be prevailing ones. For the things, about which these proofs are conversant, being primary or heteroclite, or of as abstruse a nature, as if they were so, it too often happens, that, what opinion soever we choose about them, we must admit something, that is incumbered with great difficulties, and therefore will be liable to great objections, that perhaps will never be directly and satisfactorily answered. And since it may fare thus with us, where two opposite opinions are contradictory, we may conclude, that those difficulties will not cogently evince the falsity of a theological opinion, which are but such, that the same, or as great, may be objected against another, that either is manifestly or confessedly a truth, or which must necessarily be admitted to be one, if the contrary theological tenet be supposed not to be one.

2. ANOTHER corollary, that may be drawn from the discourse, that afforded us the former, may be this; that it may not be unreasonable to believe a thing, though its proof be very difficult to be understood. To manifest this, I shall need no other argument, than what may be afforded by divers geometrical and other mathematical demonstrations; some of which are fetched, by intermediate conclusions, from principles so very remote, and require so long a series of mediums to be employed about them, that not only a man, that were of *Pilate*'s temper, who having asked him, that could best tell him, what is truth? would not stay a while to be satisfied about his enquiry, would, before he reaches half way to the end of the demonstration, or perhaps of the lemma's, be quite discouraged from proceeding any further; but even sedulous and heedful perusers do find themselves oftentimes unable to carry along such a chain of inferences in their minds, as clearly to discern, whether the whole ratiocination be coherent, and all the particulars have their due strength and connection. And if you please to make a trial upon some of the demonstrations of *Vitellio*, or even of *Clavius*, that I can direct you to, I doubt they will put you to the full exercise of your patience, and quite tire your attention: and though the modern algebrists, by their excellent way of expressing quantities by symbols, have so abridged geometrical and arithmetical demonstrations, that, by the help of species, it is sometimes easy to demonstrate, that in a line, which in the ordinary way would require a whole page, (as our most learned friend Dr. *Ward* has ingeniously shewn, by giving the demonstrations of about twenty of Mr. *Hobbes*'s theorems, in less than so many lines;) yet some demonstrable truths are so abstruse, that, even in the symbolical way, men need more attention to discern them, than most men would employ in any speculation whatsoever. And *Des Cartes* himself, as famous and expert a master as he was in this way, confesses, in a letter to one of his friends, that the solution of a problem in *Pappus* cost him no less than six weeks study; though now, most mathematical demonstrations do indeed seem far shorter than they are, because that *Euclid*'s elements being generally received among mathematicians, all his propositions are so many lemmata, which need be but referred to in the margin, being known and demonstrated already. By all which it may appear, that, granting some theological truths to be complained of by many, as things so mysterious and abstruse, that they cannot readily discern the force of those proofs, that *Des Cartes*, and other subtile speculators, have proposed to evince them; yet if other learned men, that are competent estimators, and are accustomed to bring much patience and attention to the discernment of difficult and important truths, profess themselves satisfied with them, the probations may yet be cogent, notwithstanding the difficulty to have their strength apprehended. For, if such a difficulty ought to pass for a mark, that a ratiocination is not valid, no reasonings will be found fitter to be rejected or distrusted, than many of those, whose cogency has procured such a repute to mathemacal demonstrations.

3. IT may also be deduced from the foregoing discourse, that it is not always against reason to embrace an opinion, which may be incumbered with a great difficulty, or liable to an objection not easy to be solved; especially if the subject be such, that other opinions about it avoid not either the same inconveniencies, or as great ones. The first part of what is said in this consideration will often follow from the supposition made in the precedent discourse. For those things, that render a doctrine or assertion difficult to be conceived and explained, will easily supply the adversaries of it with objections against it.

AND as for the latter, viz. the clause, which takes notice, that the consideration, to which it is annexed, will chiefly take place in that sort of opinions, that are specified in it; it will need but little of distinct proof.

FOR it is manifest enough, that if the subject or object, about which the opinion proposed is conversant, be such, that not only the contradictory opinion, but others also, are obnoxious either to the same inconveniencies, or to others, that are equal or greater; the difficulties, that are urged against a theological doctrine, may (as hath been shewn already in the first corollary,) be rationally enough attributed, not to the unreasonableness of the opinion, but to somewhat else.

THE last consectary, that (as I intimated) may be deduced from the precedent discourse, is, that it is not always unreasonable to believe something theological for a truth, which (I do not say, is truly inconsistent with, but) we do not clearly discern to comport very well with something else, that we also take for a truth, or perhaps, that is one indeed; if the theological tenet be sufficiently proved in its kind, and be of that sort of things, that we have been of late, and are yet discoursing of.

THE generality of our philosophers, as well as divines, believe, that God has a foreknowledge of all future contingencies; and yet how a certain prescience can consist with the freewill of man, (which yet is generally granted him, in things merely moral or civil,) is so difficult to discern, that the Socinians are wont to deny such things, as depend upon the will of free agents, to be the proper objects of omniscience; and the head of the Remonstrants, though a very subtile writer, confesses, that he knows not, how clearly to make out the consistency of God's prescience, and man's freedom; both which he yet confesses to be truths, being compelled to acknowledge the former, (for the latter is evident,) as well by the infiniteness, that must be ascribed to God's perfections, as by the prophetick predictions, whereby such contingent events have been actually foretold. And the reconcilement of these truths is not a difficulty peculiar to the Christian religion, but concerns speculative men

men in all religions, who acknowledge the Deity to be infinitely perfect, and allow man, as they do, to be a free agent.

[But I have made this section so prolix already, that I must not enlarge on this third particular. And therefore I shall shut it up with an acknowledgment of *Des Cartes*, which may be applied not only to it, but to almost all, that has been discoursed in this section, and indeed to a great part of this letter. He then in an epistle, that came not forth, till some years after the writer's death, speaks thus to the philosophical adversary, to whom it is addressed : "As I have often said, when the "question is about things, that relate to God, "or to what is infinite, we must not consider, "what we can comprehend of them, (since "we know, that they ought not to be compre- "hended by us) but only what we can con- "ceive of them, or can attain to by any cer- "tain reason or argument.

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SECTION VII.

AND now it is time to advance to one of the main considerations I had to propose to you, concerning the subject of this letter, and it is this; that when we are to judge, whether a thing be contrary to reason or not, there is a great deal of difference, whether we take reason for the faculty furnished only with its own innate principle, and such notions, as are generally obvious, (nay, and if you please, with this or that philosophical theory;) or for the faculty illuminated by divine revelation, especially that, which is contained in the books commonly called the Scripture.

To clear and enforce this the better, I shall invite you to take notice with me of the two following particulars.

WE may then in the first place consider, that even in things merely natural, men do not think it at all irrational, to believe divers such things upon extrinsecal proofs, especially the testimony of the skilful, as, if it were not for that testimony, a man, though born with good parts, and possibly very learned in the Peripatetick, or some other particular philosophy, would look upon as irrational to be believed, and contrary to the laws of nature.

OF this I shall give you some instances in the phænomena of the loadstone, and particularly such as these; that the loadstone, though (as was above intimated) with one part it will draw, yet with another the same stone will repel the same point of the same excited needle; and yet at the same time be fit to attract either point of another needle, that never came near a loadstone before: that though it be the loadstone, that imparts an attractive virtue to the iron, yet when the load-stone is capped, as they called it, and so a piece of iron (and consequently a distance) is interposed betwixt the stone and the weight to be raised, it will take up by many times more, than if it be itself applied immediately thereunto, insomuch that *Mersennus* relates, that (if there be no mistake,) he had a load-stone, that of itself would take up but half an ounce of iron, which when armed (or capped) would lift up ten pounds, which, says he, exceeded the former weight three hundred and twenty times: that a mariner's needle, being once touched with a vigorous load-stone, will afterwards, when freely poised, turn it self north and south; and if it be by force made to regard the east and west, or any other points of the compass, as soon as it is left at liberty, it will of itself return to its former position: that a load-stone floating on water will as well come to, and follow a piece of iron, that is kept from advancing towards it, as, when itself is fixed, and the iron at liberty, it will draw that metal to it: that without any sensible alteration in the agent or the patient, the load-stone will in a trice communicate all its virtues to a piece of steel, and enable that to communicate them to another piece of the same metal: that if a load-stone, having been marked at one end, be cut longwise according to its axis, and one segment be freely suspended over the other, the halves of the marked end, that touched one another before, will not now lie together, but the lower will drive away the upper; and that, which regarded the north in the marked end of the intire load-stone, will join with that extreme of the lower half, which in the intire stone regarded the south: that (as appears by this last named property) there are the same magnetical qualities in the separated parts of a magnet, as in the intire stone; and if it be cut, or even rudely broken into a great many parts or fragments, every one of these portions, though perhaps not so big as a corn of wheat, will, if I may so speak, set up for itself, and have its own northen and southern poles, and become a little magnet, *sui juris*, or independent upon the stone, from which it was severed, and from all its other parts: that if a loadstone be skilfully made spherical, this little magnetick globe, very fitly by our *Gilbert* called a terrella, will not only, being freely placed, turn north and south, and retain that position, but have its poles, its meridians, its æquator, &c. upon good grounds designable upon it, as they are upon the great globe of the earth. And this will hold, whether the terrella be great or small.

In his little tract *De Magnetis Proprietatibus*. p. m. 350.

I might not only much encrease the number of these odd magnetical phænomena, but add others about other subjects; but these may suffice to suggest to us this reflection, that there is no doubt to be made, but that a man, who never had the opportunity to see or hear of magnetical experiments, would look upon these as contrary to the principles of nature, and therefore to the dictates of reason, as accordingly some learned Aristotelians, to whom I had occasion to propose some of them, rejected them as incredible. And I doubt not, but I could frame as plausible arguments from the mere axioms of philosophers, and the doctrine of philosophick schools against some magnetical phænomena, which experience hath satisfied me of, as are wont to be drawn from the same topicks against the mysterious articles of faith; since among the strange properties of the load-stone there are some, which are not only admirable

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and ftupendous, but feem repugnant to the dictates of the received philofophy and the courfe of nature. For whereas natural bodies, how fubtile foever, require fome particular difpofitions in the medium, through which their corpufcles are to be diffufed, or their actions tranfmitted; fo that light itfelf, whether it be a moft fubtile body, or a naked quality, is refifted by all opacous mediums, and the very effluvia of amber and other electricks will not permeate the thinneft glafs, or even a fheet of fine paper; yet the load-ftone readily performing his operations through all kind of mediums, without excepting glafs itfelf.

IF the poles of two magnetick needles do both of them regard the north, another philofopher would conclude them to have a fympathy, at leaft to be unlikely to difagree; and yet, if he bring thefe extremes of the fame denomination within the reach of one another, one will prefently drive away the other, as if there were a powerful antipathy between them.

A fomewhat long needle being placed horizontally, and exactly poifed upon the point of a pin, if you gently touch one end with the pole of a vigorous magnet, that end fhall manifeftly dip or ftoop, though you often take it off the pin, and put it on again. And this inclination of the needle will continue many years, and yet there is not only no other fenfible change made in the metal by the contact of the load-ftone; but one end has required a a durable preponderancy, though the other be not lighter, nor the whole needle heavier than before. And the inclination of the magnetick needle may be by another touch of the loadftone taken away, without leffening the weight of the part, that is deprived of it.

THE operation, that, in a trice, the loadftone has on a mariner's needle, though it makes no fenfible change in it, or weakens the load-ftone itfelf, will not be loft, though you carry it as far as the fouthern hemifphere: but it will not be the fame in all places, but in fome, the magnetick needle will point directly at the north, in others, it will deviate or decline fome degrees towards the eaft or the weft: and, which feems yet more ftrange, the fame needle in the fame place will not always regard the fame point of the compafs, but, looked on at diftant times, may vary from the true meridian, fometimes to the weft, and afterwards to the eaft.

ALL the communicable virtues of the magnet may be imparted to iron, without any actual contact of the two bodies, but barely by approaching in a convenient way the iron to the loadftone for a few moments. And the metal may likewife be deprived of thofe virtues in a trice, without any immediate contact by the fame, or another loadftone.

IF you mark one end of a rod, or other oblong piece of iron, that never came near a magnet, and hold it perpendicularly, you may at pleafure, and in the hundredth part of a minute, make it become the north or fouth pole of a magnetical body. For if, when it is held upright, you apply to the bottom of it the north-extreme of an excited and well-poifed needle, the lower end of the iron will drive away that extreme, which yet will be drawn by the upper end of the fame iron. And, if by inverting, you make this lower end the uppermoft, it will not attract, but repel the fame lilly or north point of the needle, juft under which it is to be perpendicularly held.

THOUGH *vis unita fortior* be a received rule among naturalifts; yet oftentimes, if a magnet be cut into pieces, thefe will take up, and fuftain much more iron, than the entire ftone was able to do.

IF, of two good loadftones, the former be much bigger, and on that account ftronger than the other, the greater will draw a piece of iron, and retain it much more ftrongly than the leffer; and yet, when the iron fticks faft to the greater and ftronger loadftone, the leffer and weaker may draw the iron from it, and take it quite away.

THESE phænomena, (to mention now no more,) are fo repugnant to the common fentiments of naturalifts, and the ordinary courfe of things, that, if, antecedently to any teftimony of experience, thefe magnetical properties had been propofed to *Ariftotle* himfelf, he would probably have judged them fictitious things, as repugnant to the laws of nature: neverthelefs, though it feems incredible, that the bare touch of a loadftone fhould impart to the mariner's needle a property, which, (as far as we know) nothing in the whole world, that is not magnetical, can communicate or poffefs; and fhould operate (as men fuppofe) upon it at three or four thoufand leagues diftance; yet this is believed by the Peripateticks themfelves upon the teftimony of thofe navigators, that have failed to the *Eaft* and *Weft-Indies*; and divers even of the more rigid of the modern philofophers believe more than this, upon the teftimony of *Gilbert*, *Cabæus*, *Kircherus*, and other learned magnetical writers, who have affirmed thefe things; moft of which I can alfo aver to you upon my own knowledge.

THUS the habitablenefs of the torrid zone, though (as I lately noted) upon probable grounds denied by *Ariftotle*, and the generality of philofophers for many ages; yet not only that, but its populoufnefs, is now confidently believed by the Peripatetick fchoolmen themfelves, who never were there.

AND though *Ptolomy*, and fome other eminent aftronomers, did with great care and fkill, and by the help of geometry, as well as obfervations, frame a theory of the planets fo plaufibly contrived, that moft of the fucceeding mathematicians for twelve or fourteen ages acquiefced in it; yet almoft all the modern philofophers and aftronomers, that have fearched into thefe matters, with a readinefs to believe their eyes, and allow their reafon to act freely, have been forced, if not to reject the whole theory, yet, at leaft to alter it quite, as to the number and order of the planets, though thefe laft named innovations are fometimes folely, and always mainly, built upon the phænomena, difcovered to us by two or three pieces of glafs

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placed in a long hollow cane, and honoured with the name of a telescope.

THE last of the two things, I invited you to consider with me, is this, that when we are to judge, which of two disagreeing opinions is most rational, i. e. to be judged most agreeable to right reason, we ought to give sentence, not for that, which the faculty, furnished only with such and such notions, whether vulgar, or borrowed from this or that sect of philosophers, would prefer, but that, which is preferred by the faculty, furnished, either with all the evidence requisite or advantageous to make it give a right judgment in the case lying before it, or, when that cannot be had, with the best and fullest Informations, that it can procure.

THIS is so evident by its own light, that your friend might look upon it as an affront to his judgment, if I should go about solicitously to prove it. And therefore I shall only advertise you, that, provided the information be such, as a man has just cause to believe, and perceives, that he clearly understands, it will not alter the case, whether he have it by reason, as that is taken for the faculty furnished but with its inbred notions, and the more common observations, or by some philosophical theory, or by experiments purposely devised, or by testimony human or divine, which last we call *revelation*. For all these are but differing ways of informing the understanding, and of signifying to it the same thing; as the sight and the touch may assure a man, that a body is smooth or rough, or in motion or at rest; (and in some other instances, several senses discover to us the same object, which is therefore called *objectum commune*;) and provided these informations have the conditions lately intimated, which way soever the understanding receives them, it may safely reason and build opinions upon them.

ASTRONOMERS have within these hundred years observed, that a star hath appeared among the fixed ones for some time, and having afterwards disappeared, has yet some years after that shewed itself again. And though, as to this surprizing phænomena, our experimental philosophers could have contributed nothing to the producing it, and though it is quite out of all the received systems of the heavens, that astronomers have hitherto delivered; yet the star itself may be a true celestial light, and may allow us to philosophize upon it, and draw inferences from the discoveries it makes us; as well as we can from the phænomena of those stars, that are not extraordinary, and of those falling stars, that are within our own ken and region.

THAT the supernatural things, said to be performed by witches and evil spirits, might, if true, supply us with hypotheses and mediums, whereby to constitute and prove theories, as well as the phænomena of mere nature, seems tacitly indeed, but yet sufficiently, to be acknowledged, by those modern naturalists, that care not to take any other way to decline the consequences, that may be drawn from such relations, than solicitously to shew, that the relations themselves are all, as I fear most of them are, false, and occasioned by the credulity or imposture of men.

BUT not to do any more than glance at these matters, let us proceed upon what is more unquestionable, and consider, that, since even our most critical philosophers do admit many of the astonishing attributes of magnetick bodies, which themselves never had occasion to see, upon the testimony of *Gilbert*, and others, who never were able to give the true causes of them; because they look upon those relators as honest men, and judicious enough not to be imposed upon as to the matter of fact; since, I say, such amazing things are believed by such severe naturalists, upon the authority of men, who did not know the intimate nature of magnetick bodies; and since these strange phænomena are not only assented to, as true, by the philosophers we speak of, but many philosophical consequences are without hæsitancy deduced from them, without any blemish to the judgment of those, that give their assent both to the things and the inferences; why should it be contrary to reason to believe the testimony of God, either about his nature, which he can best, and he alone can fully know, or about the things, which either he himself has done, as the creation of the world, and of man; or which he means to do, as the destroying the world, (whether the whole world, or our great vortex only, I dispute not) and the raising both of good and bad men to life again, to receive rewards and punishments, according to their demerits. For methinks that apostle argues very well, who says, "if "we receive the testimony of men, the testi- 1 John. v. 9.
"mony of God is greater;" especially about such things concerning his own nature, will, and purposes, as it is evident, that reason, by its own unassisted light, cannot give us the knowledge of.

So that we Christians, in assenting to doctrines upon the account of revelation, need not, nor do not, reject the authority of reason, but only appeal from reason to itself, i. e. from reason, as it is more slightly, to its dictates, as it is more fully informed. Of which two sorts of dictates there is nothing more rational, than to prefer the latter to the former.

AND for my part, I am apt to think, that, if what has been represented in this section, were duly considered, this alone would very much contribute to prevent or answer most of the objections, that make such of the questioners of religion, as are not resolutely vicious, entertain such hard thoughts of some articles of the Christian faith, as if they were directly repugnant to reason. For, as we were observing, that is not to be looked on as the judgment of reason, that is pronounced even by a rational man, according to a set of notions, though the inferences from these would be rational, in case there were nothing else fit to be taken into consideration by him, that judges; but that is rather to be looked upon as the judgment of reason, which takes in the most information procurable, that is pertinent to the things under consideration.

And therefore men, though otherwise learned and witty, shew themselves not equal estimators of the case of those, tha believe the articles we speak of, when they pronounce them to assent irrationally, because the things they assent to cannot be demonstrated or maintained by mere natural reason, and would probably be rejected by *Democritus*, *Epicurus*, *Aristotle*, or any other of the ancient philosophers, to whom they should be nakedly proposed, and whose judgment should be desired about them. For, although this allegation would signify much, if we pretended to prove what we believe only by arguments drawn from the nature of the thing assented to; yet it will not signify much in our case, wherein we pretend to prove what we believe, chiefly by divine testimony, and therefore ought not to be concluded guilty of an irrational assent, unless it can be shewn, either that divine testimony is not duly challenged by us for the main of our religion; or that in the particular articles we father something on that testimony, which is not contained in it, or rightly deducible from it. And to put us upon the proving our particular articles of faith, sufficiently delivered in the scriptures, and not knowable without revelation, by arguments merely natural, without taking notice of those we can bring for the proof of that revelation, on whose account we embrace those articles, is to challenge a man to a duel, upon condition he shall make no use of his best weapons; and is as unreasonable, as if a schoolman should challenge your friend to prove, that the torrid zone is inhabited, against the reasons, that the Aristotelians are wont to give to prove it uninhabitable, without allowing him to make use of the testimony of navigators, who assure us of the constant breezes, that daily ventilate the air, and qualify that heat, which otherwise would not be supported, and who furnish us with those other circumstances, whereon to build our proofs, which we, that were never there, can have but by relation.

AND indeed, the limitations, that Christian religion puts to some of the dictates of philosophy, which were wont to be admitted in a more general and unrestrained sense, and the doctrines about God and the soul, &c. that it superadds to those, which the light of nature might lead men to about the same subjects; though to some they may seem injurious to philosophy and reason, are as little unkind to either, as is the gardener to a crab-stock, or some such other wild plant, when by cutting off some of the branches, and by making a slit in the bark, that he may graft on it a pare-main, or some other choice apples, by this seemingly hard usage he brings it to bear much nobler fruit, than, if left to its own natural condition, it ever would have done.

I know not, whether to all, that hath been said in this section, I may not add thus much further, that it sometimes happens, that those very things, which at first were proposed to the understanding, and believed upon the score of revelation, are afterward assented to, by it upon the account of mere reason. To which purpose I consider, that not any of the ancient philosophers, nay, as far as I have read, even of those, that believed God to be the author of the world, dreamed, that he created matter of nothing, but only formed the world out of præ-existent matter; whereas Christian divines usually teach, as an article of faith, that besides what they call a mediate creation, as when fishes were made out of the water, or *Adam's* body was made out of the earth, there was an immediate production of matter itself out of nothing.

SECTION VIII.

AFTER what has been hitherto discoursed, it may be seasonable to consider, what kind of probation, or what degree of evidence may reasonably be thought sufficient, to make the Christian religion thought fit to be embraced.

PERHAPS I shall not need to tell you, that, besides the demonstrations wont to be treated of in vulgar logick, there are among philosophers three distinct, whether kinds or degrees, of demonstration. For there is a metaphysical demonstration, as we may call that, where the conclusion is manifestly built on those general metaphysical axioms, that can never be other than true; such as *nihil potest simul esse & non esse*; *non entis nullæ sunt proprietates reales*, *&c.* There are also physical demonstrations, where the conclusion is evidently deduced from physical principles; such as are *ex nihilo nihil fit: Nulla substantia in nihilum redigitur*, *&c.* which are not so absolutely certain as the former, because, if there be a God, he may (at least for aught we know) be able to create and annihilate substances; and yet are held unquestionable by the ancient naturalists, who still suppose them in their theories. And lastly, there are moral demonstrations, such as those, where the conclusion is built, either upon some one such proof cogent in its kind, or some concurrence of probabilities, that it cannot be but allowed, supposing the truth of the most received rules of prudence and principles of practical philosophy.

AND this third kind of probation, though it come behind the two others in certainty, yet it is the surest guide, which the actions of men, though not their contemplations, have regularly allowed them to follow. And the conclusions of a moral demonstration are the surest, that men aspire to, not only in the conduct of private men's affairs, but in the government of states, and even of the greatest monarchies and empires. And this is considerable in moral demonstrations, that such may consist, and be, as it were, made up of particulars, that are each of them but probable; of which, the laws established by God himself among his own people, as well as the practice of our courts of justice here in *England*, afford us a manifest instance in the case of murder, and some other criminal causes. For, though the testimony of a single witness shall not suffice to prove the accused party guilty of murder; yet the testimony of two witnesses, though but of equal credit

credit, that is, a second testimony added to the first, though of itself never a wit more credible than the former, shall ordinarily suffice to prove a man guilty; because it is thought reasonable to suppose, that, though each testimony single be but probable, yet a concurrence of such probabilities, (which ought in reason to be attributed to the truth of what they jointly tend to prove) may well amount to a moral certainty, *i. e.* such a certainty, as may warrant the judge to proceed to the sentence of death against the indicted party.

To apply these things now to the Christian religion: if you consider, with how much approbation from discerning men, that judicious observation of *Aristotle* has been entertained, where he says, that it is as unskilful and improper a thing, to require mathematical demonstrations in moral affairs, as to take up with moral arguments in matters mathematical; you will not deny, but that those articles of the Christian religion, that can be proved by a moral, though not by a metaphysical or physical demonstration, may, without any blemish to a man's reason, be assented to; and that consequently (by virtue of the foregoing considerations) those other articles of the Christian faith, that are clearly and legitimately deducible from the so demonstrated truths, may likewise, without disparagement, be assented to.

We may also here consider further, that the chusing, or refusing to embrace the Christian religion, which is not proposed to us only as a system of speculative doctrines, but also as a body of laws; according to which, it teaches us, that God commands us to worship him, and regulate our lives; the embracing, I say, or not embracing this religion, is an act of human choice, and therefore ought to be determined according to the dictates of prudence. Now, though in matters, that very much import us, we may wish for and endeavour after such reasons, whereby to determine our resolves, as may amount to moral demonstrations; yet prudence will not always require, that we should refuse to act upon arguments of a less cogency, than moral demonstrations. For oftentimes, in human affairs, it so falls out, that divers hazards, or other inconveniencies, will attend whatever resolution we take; and in that case, all that prudence requires, or can enable us to do, is, to take that resolution, which upon the whole matter seems to be preferable to any other; though that, which is thus preferred, may perhaps be liable to some objection, that cannot be directly answered, but only obliquely, by the preponderancy of the arguments, that persuade the choice, against which the objection is made.

But here perhaps you will tell me, that the safest way, in case of such importance, is to suspend an action, that is every way attended with difficulties, and to forbear either embracing or rejecting the Christian religion, till the truth or falseness of it come to appear evident and unquestionable.

To which I answer, that indeed in matters of bare speculation, about which our understandings only need to be conversant, the suspension of assent is not only practicable, but usually the safest way; but *Des Cartes* himself, who has been the greatest example and inculcator of his suspension, declares, that he would have it practised only about human speculations, not about human actions; *sed hæc interim dubitatio ad solam* contemplationem *veritatis restringenda*; *non quantum ad usum vitæ: quia persæpe rerum agendarum occasio præteriret, antequam nos dubiis nostris exolvere possemus. Non raro quod tantum est verisimile cogimur amplecti, vel etiam interdum, etsi è duobus unum altero verisimiliùs non appareat, alterutrum tamen eligere.* And in some of his other writings he speaks so much to shew, that it is unreasonable to expect in matters, where embracing or rejecting a course, that requires practice, is necessary, such a certainty, as he judges necessary to make a true philosopher acquiesce in reference to propositions about speculative matters, that I find by one of his letters, that he was vehemently accused for having taught, that men need not have as sure grounds for chusing virtuous and avoiding vicious courses, as for determining about things merely notional.

And here let me observe to you the difference, that I take notice of in the cases, where we are put upon deliberating, whether we will chuse or refuse a thing proposed. For it may be propounded to us, either as a proffer, on whose acceptance an advantage may be hoped, or as a duty, which, besides the advantage it promises to the performance, has a penalty annexed to the non-performance, or as an only expedient to avoid a great mischief, or obtain a great good.

Thus, when in the *Theatrum Chymicum* some of its chief authors, as *Lully*, *Geber*, *Artephius*, who pretend to have been *adepti*, i. e. possessors of the elixir, very earnestly exhort their readers to apply themselves to so noble and useful a study as alchymy, (by the help of which, the last-named *Artephius* is said to have lived 1000 years,) they make but a proposition of the first sort. For though a prosperous attempt to make the philosophers stone (supposing there be such a thing) would possess a man of an inestimable treasure; yet, if he either refuse to believe these writers, or, if he do believe them, refuses to take the pains required of him, that would follow their counsel, he can only miss of the wealth, &c. they would make him hope for, but is really never a whit the poorer, or in a worse condition, than if they had not endeavoured to engage him.

But if an absolute sovereign commands something to be done by his subjects; and to enforce his command, does not only propose great recompenses to those, that shall perform what it prescribed, but threatens heavy penalties to the disobedient; this will belong to the second sort of cases above mentioned, in which, as it is evident, a man has not the same latitude allowed him as in the first.

But if we suppose, that a man by a translation of very peccant matter has got a spreading gangrene in his arm, and a skilful surgeon

geon tell him, that, if he will part with his arm, he may be recovered, and fave his life, which elfe he will certainly lofe; this cafe will belong to the laft fort above-mentioned; the patient's parting with his arm being the only remedy of the gangrene, and expedient to fave his life, and recover his health. And here alfo it is manifeft, that there are far ftronger motives, than thofe mentioned in the firft cafe, to make a pofitive and timely refolution.

To bring this home to our fubject, I need but mind you, that the Chriftian doctrine does not only promife a heaven to fincere believers, but threatens no lefs than a hell to the refractory.

THE voice of *Mofes* to the *Jews* is this, "Behold, I fet before you this day a bleffing "and a curfe; a bleffing, if ye obey the commandments of the Lord your God, which "I command you this day; and a curfe, if "ye will not obey the commandment of the "Lord your God, but turn afide out of the "way, which I command you this day."

Deutr. xi 26,27,28.

AND the commiffion, that Chrift gave his apoftles, to preach the gofpel, runs thus: "Go ye into all the world, and preach the "gofpel to every creature," i. e. to all mankind; "he that believeth, and is baptized, "fhall be faved; but he, that believeth not, "fhall be damned."

Mark xvi. 15, 16.

BY this you may perceive, that as far as there is either truth or probability in the Chriftian religion, fo far forth he, that refufes to become a difciple to it, runs a venture, not only to lofe the greateft bleffings, that men can hope, but to fall eternally into the greateft miferies that they can fear. And indeed our cafe, in reference to the Chriftian religion may not only be referred to the fecond fort of cafes lately mentioned, but to the third fort too. For as the language of the author of the Chriftian religion was to his auditors, "If ye be-"lieve not, that I am he (the Meffias) ye fhall "die in your fins; fo of the two greateft heralds of it, the one tells the Jews, that neither is there falvation in any other: for, "there is "no other name under heaven given among "men, whereby we muft be faved:" And the other tells the Theffalonians, that the "Lord "Jefus fhall be revealed from heaven with his "mighty angels in flaming fire, taking ven-"geance on them, that know not God, and "obey not the gofpel of our Lord Jefus "Chrift; who fhall be punifhed with ever-"lafting deftruction from the prefence of the "Lord, and from the glory of his power."

John viii. 24.

Acts iv 12.

2 Theff. i. 7, 8, 9.

BY all this it appears, that the Chriftian religion is not propofed barely as a proffer of heaven in cafe men embrace it, but as a law, that men fhould embrace it upon the greateft penalty, and as the only expedient and remedy to attain eternal happinefs, and efcape endlefs mifery; fo that the forbearing to fubmit our necks to the yoke of Chrift being as well a ruinous courfe, as to reject it, that, which reafon here puts us upon, is, not fo much to confider, whether or no the arguments for the Chriftian religion be demonftrations, and will enable a man to anfwer directly all objections and fcruples; (for there are divers courfes, that prudence may enjoin a man to fteer, whilft philofophy fuggefts fpeculative doubts about the grounds of fuch refolutions;) but whether it be more likely to be true, than not to be true, or rather, whether it be not more advifeable to perform the conditions it requires upon a probable expectation of obtaining the bleffings it promifes, than by refufing it to run a probable hazard of incurring fuch great and endlefs miferies, as it peremptorily threatens.

IT will perhaps be faid, that this is a hard cafe. But that is an allegation I am not here to confider; fince it properly belongs to the doctrine about the providence of God, who being the only Author and abfolute Lord of the creatures, who can receive neither laws nor benefits from them, that can oblige him to them; has a right to prefcribe them what laws he thinks fit, that are not impoffible for them to obey, and to punifh their difobedience to fuch laws; and much more has a right to annex what conditions he pleafes, to that ineftimable felicity he holds forth; the proffer of it upon any terms being a free act of his mere goodnefs, and the value of it incomparably furpaffing whatever we men can do or fuffer to obtain it; efpecially confidering, that, as he might enforce his commands, as fovereigns commonly do, by threatning penalties to the difobedient, without propofing rewards to the performers; fo he has given men fuch probable arguments to ground their expectations on, that they will be felf-condemned, if they reject the religion he propofes, and yet maintain it to be decent (if I may fo fpeak) for him to crown their faith with unvaluable bleffings. But, as I was faying, the direct and full anfwer to this allegation belongs not to this place, where it may fuffice to fay, that, whether the cafe be hard or no, yet this is the cafe. And therefore, though the proofs of the Chriftian religion did not amount (which yet I do not grant) to moral demonftrations, a man may act rationally in embracing that religion, if, all things confidered, it appear more likely to be true, than not to be true.

AND I fhall by and by fhew you, that this is not the only cafe, where prudence puts us upon making refolutions, notwithftanding contrary doubts.

I know the harfhnefs of the cafe is by moft men made to confift in this, that for a religion, whereof the truth fuppofed in its promifes and threats is not demonftratively proved, we muft refign up our pleafures, and fometimes undergo confiderable hardfhips and loffes, and confequently we muft quit what is certain, for what is uncertain. I have in another paper had occafion to fay fomething elfe to this objection, than what (to avoid repetition) fhall make up my prefent anfwer, which confifts of two parts.

THE firft whereof is, that what we are to give up to become Chriftians, is not really fo valuable in itfelf, as the objecters think, and that it is of fcarce any value at all, if compared to the goods we may acquire by parting with them. For alas! what is it, that Chrifti-

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anity requires us to forego, but small petty enjoyments? which those, that have had the most of, have found them, and pronounced them unsatisfactory, whilst they possessed them, and which manifest experience shews to be no less transitory, than they have been declared empty, since a thousand accidents may take them from us, and death will infallibly, after a short time (which can be but a moment compared to eternity) take us from them. And if it be said, that these enjoyments, such as they are, are at least the only happiness, that we can make our selves sure of; I must freely profess, that I think it therefore the more reasonable to part with them, if it be necessary, upon the hopes, that Christian religion gives us. For (especially if a man behold those things, not only with a philosophical eye, that can look through them, but with a Christian eye, that can look beyond them,) if there can be no greater happiness, I do not think so poor a thing, as men call happiness, worth being greedily desired; and if there be such a transcendent happiness as Christianity holds forth, I am sure, that deserves to be the object of my ambition. So that either the meanness of worldly happiness will make me think it no great misery to want it, or the excellency of heavenly felicity will make me think it great wisdom to part with earthly for it.

And now, in the second part of my answer, I must invite you to consider with me, that Christian religion requires not of us actions more imprudent, than divers others, that are generally looked upon as complying with the dictates of prudence, and some of them practised, by great politicians themselves, in the weighty affairs of state.

You know, what a common practice it is, in great storms at sea, for the merchants themselves to throw over-board their goods, and, perhaps too, their victuals, (as in *Paul*'s case) though they be sure to lose what they cast away, and are not certain, either that this loss will save the ship, or that the ship may not be saved without it. The wisest, and even the worldliest men, whether princes or private persons, think themselves never more so, than when they toil, and lay out their care and time, and usually deny themselves many things, to provide advantagiously for children, which they have but a woman's word for, and consequently a bare moral probability to assure them to be theirs.

In the small pox many physicians are for bleeding, and others (as most of our English practitioners) are very much against it. Supposing then (which is no very rare case) that a person invaded by that disease be told by one of his physicians, that, unless nature be eased of part of her burthen by phlebotomy, she will never be able to overcome the disease; and on the contrary, the other assures him, that, if by exhausting the treasure of life, (the blood) he further weakens nature, which is but too weak already, the disease must needs overcome her: what can a prudent man do in this case, where he can take no resolution, against which probable arguments, that are not directly and fully to be answered, may not be opposed, and where yet the suspension of his resolution may be as ruinous, as the venturing to take either of those he is invited to?

And in the formerly mentioned case, of a man, that has a spreading gangrene in his arm, if he consents, that it be cut off, which prudence often requires that he should do, he is certain to lose one of his usefullest limbs, and is not certain, but that he may save his life without that loss, nor that he shall save it by that loss.

And to give you an instance or two of a more publick nature; how many examples does history afford us of famous generals and other great commanders, who have ventured their forces and their lives to seize upon places promised to be betrayed to them by those they had corrupted with money; though the ground, upon which they run this hazard, be the engagement of some, who, if they were not traytors, that could falsify their faith, would never have been bribed to make so criminal and ignominious an engagement? How often have the greatest politicians either resolved to enter into a war, or taken courses, that they foresee will end in a war, upon the informations they receive from those they have corrupted in other prince's councils; though, to believe such intelligencers, those, who venture so much upon their informations, must suppose them faithless and perfidious men?

It were not difficult, to add other instances to the same purpose, by which, joined with what has been above discoursed, it may appear, that a man need not renounce or lay aside his reason to resolve to fulfil the conditions of the gospel, though the arguments for it were none of them demonstrative ones. For so much as a probability of obtaining by it such inestimable blessings, as it proposes, and little more than a bare probability of incurring, by rejecting it, such unspeakable miseries, as it threatens, may rationally induce a man to resolve upon fulfiling its reasonable conditions, and his prudence may very well be justified, if it do but appear, that (1.) It is more probable, that some religion should be true, than that so many well attested miracles alledged by the ancient Christians should be false; and that God, who is the author of the world, and of men, (for so much I think may be physically proved) should leave man, whom he has so fitted, and by benefits and internal laws obliged to worship him, without any express direction how to do it: and that (2.) If there be any true religion, the Christian is the most likely to be that, in regard not only of the excellency of its doctrine and promises, but of the prophecies and miracles, that bore witness to it, the records of which were made by honest plain men, who taught and practised the strictest virtue, and who knew their religion condemned lying, freely joined their doctrine and narratives with their blood: the truth of which was so manifest in the times, when they were said to be done, that the evidence seemed abundantly sufficient to convert whole nations, and among them many considerable and prudent persons, who had great opportunity, as well as concern, to examine the truth of them, and who were by their

their interest and education so indisposed to embrace Christianity, that, to make a sincere profession of it, they must necessarily relinquish both their former religion, and their former vices, and venturously expose for it not only their fortunes, but their lives.

IF it be here objected, that it is very harsh, if not unreasonable, to exact, upon so great penalty as damnation, so firm an assent, as is requisite to faith, to such doctrines, as are either obscurely delivered, or have not their truth demonstratively made out: I answer, that whatever others may think, I don't believe, that there is any degree of faith absolutely necessary to salvation, that is not suitable to the evidence, that men may have of it, if they be not wanting to themselves through laziness, prejudices, vice, passion, interest, or some other culpable defect. For considering, that God is just, and gracious, and has been pleased to promulgate the gospel, that men, whom it supposes to act as such (that is, as rational creatures) should be brought to salvation by it; I see no just cause to think, that he intends to make any thing absolutely necessary to salvation, that they may not so far clearly understand as they are commanded distinctly and explicitely to believe it; and what is not so delivered, I should, for that very reason, unwillingly admit to be necessary to salvation: and you may here remember, that I formerly told you, I was far from thinking all the tenents either of the schools, or of particular churches, to be so much as Christian verities, and therefore am very unlike to allow them here to be fundamental and necessary ones; and I take it to be almost as great as common a mistake, that all the doctrines, that concern fundamental articles, must be fundamental too; as if, because the head is a noble part of the body, and essential to life, therefore all the hair, that grows upon it, must be thought such too. But then, as to the absolute firmness of assent, that is supposed to be exacted by Christianity to the articles it delivers, I am not sure, that it is so necessary in all cases to true and saving faith, as very many take it to be. For first, the scripture itself tells us, that some of the truths it reveals, are unfathomable mysteries, and some other points are δυσνόητα, hard to be understood; and it is unreasonable to suppose, that the highest firmness of assent is to be given to such articles, or to those parts of them, as their obscurity keeps us from having so much reason to think, that we clearly understand them, as we have to suppose we understand those, that are far more plainly revealed. And, secondly, to speak more generally, it is harsh to say, that the same degree of faith is necessary to all persons, since men's natural capacities and dispositions, and their education, and the opportunities they have had of being informed, do very much, yet perhaps not culpably, dispose some more than others to be diffident, and apt to hesitate, and frame doubts. And the same arguments may appear evident enough to one man, to make it his duty to believe firmly what they persuade, which in another, naturally more sceptical, or better acquainted with the difficulties and objections urged by the opposite party, may leave some doubts and scruples excusable enough. And when either the doctrine itself is not clearly delivered, or the proofs of it, that a man could yet meet with, are not fully cogent; for that man, not to give such truths the same degree of assent, that demonstration may produce, is not, as many interpret it, an affront to the veracity of God, since he may be heartily disposed and ready to believe all, that shall appear to him to be revealed by God, and only doubts, whether the thing proposed be indeed revealed by him, or whether the diffident party rightly understands the sense of these words, wherein the revelation is contained; which is not to distrust God, but himself: and that in some cases, a degree of faith, not exempt from doubts, may, through God's goodness, be accepted, we may learn from hence, that the apostles themselves, who were so much in Christ's favour, made it their prayer to him, that he would increase their faith: and he, that begged, that if he could do any thing for his son, and cried out, "Lord, "I believe, help thou my unbelief," was so far accepted by that merciful high priest, who is apt to be touched with the sense of our infirmities, that his request was granted, though it could not be so but by having a miracle done in his favour. The disciples distressed by a storm, and crying to their master, as thinking themselves upon the very point of perishing, were saved by him, at the same time, when he gave them the epithet, "of men of "little faith:" and at another time, *Peter* walking upon the sea, though he had lost a degree of that faith, that made him first engage upon that adventure, and was reproved for it by Christ, was yet rescued from that sinking condition, which both he and his faith were in. And we are told, in the gospel, of a faith, which, though no bigger than a grain of mustard-seed, may enable a man to remove mountains: and though this passage speaks not primarily of justifying faith, yet still it may serve to shew, that degrees of assent, far short of the greatest, may be so far accepted by God, as to be owned by miraculous exertions of his power. For the faith then, that is made a necessary condition under the gospel, as the genuine fruit and scope of it is obedience; so it is not indispensably such a faith, as excludes doubts, but refusals. And though the assent be not so strong, as may be produced by a demonstration; yet it may be graciously accepted, if it be but strong enough to produce obedience. And accordingly, whereas *Paul*, in one place declares, "that in Christ Jesus neither "circumcision availeth any thing, nor uncir-"cumcision, but faith operative through "love;" we may learn his meaning from a parallel place, where varying the words, and not the sense, of the latter part of the sentence, he says, "that in Christ Jesus, neither cir-"cumcision availeth any thing, nor uncircum-"cision, but the keeping of the command-"ments of God." I readily grant, that attainment of a higher degree of faith is always a blessing, and cannot be sufficiently prized, without being sincerely aimed at; but there are

are in some virtues and graces degrees, which though to reach be a great happiness, yet it is but the endeavouring after them, that is an indispensable duty. Likewise it is true, that the firmness of assent to divine verities does, in some regard, bring much honour to God; as it is said of the father of the faithful, (who in reference to the promise made him of *Isaac*, did not consider his own age, nor *Sarah*'s long barrenness, so as to entertain any diffidence of what God had told him) that being mighty in faith, he gave glory to God: but it is true too, that in another respect a practical assent built upon a less undoubted evidence may have its preheminence; for when Christ now risen from the dead, had said to the distrustful *Didymus*, "*Thomas*, because thou hast seen "me, thou hast believed;" he immediately adds, "But blessed (that is, peculiarly and "preferably blessed) are those, that have not "seen, and yet have believed." And indeed he does not a little honour God (in that sense, wherein mortals may be said to honour him) who is so willing to obey and serve him, and so ambitious to be in an estate, where he may always do so, that upon what he yet discerns to be but a probability of the Christian religion's being the most acceptable to God, he embraces it with all its difficulties, and dangers, and upon this score venturously resolves to submit, if need be, to a present and actual dereliction of all his sins and lusts, and perhaps his interest and his life too, upon a comparatively uncertain expectation of living with him hereafter.

The Conclusion of the FIRST PART.

AND here I will put a period to my answer to your friend's question in one of the two senses of it, and so to the first part of this discourse. Against all which perhaps your friend will object, that at this rate of arguing for the Christian religion, one may apologize for any opinion, and reconcile the most unreasonable ones to right reason. But it is not difficult for me to reply, that this objection is grounded either upon a mistake of the design of this letter, or upon the overlooking of what is supposed in it. For I do not pretend, that the considerations hitherto alledged should pass for demonstrations of the truth of Christianity, which is to be proved by the excellency of the doctrines it teaches, and that of the rewards it promises, (both which are worthy of God,) and by divers other arguments, especially the divine miracles, that attest it: but that, which I was here to do, was, not to lay down the grounds, why I received the Christian religion, but to return an answer, backed with reasons, to the question, that was proposed: "whether I did not "think, that a Christian, to continue such, "must deny or lay aside his reason?" The sum of the answer is this, that the doctrines really proposed by the Christian religion, seeming to me to be by proper arguments sufficiently proved in their kind, so as that the proofs of it, whether they be demonstrative or no, are sufficient, (the nature of the things to be proved, considered) to justify a rational and prudent man's embracing it; this religion, I say, seeming to me to have such positive proofs for it, I do not think, that the objections, that are said to be drawn from reason against it, do really prove the belief of it to be inconsistent with right reason, and do outweigh the arguments alledgable in that religion's behalf. To propose some of the general grounds of this answer of mine, was the design of the considerations hitherto discoursed of; which (as I hinted to you at the beginning) could be no other than general, unless you had mentioned to me some of your friend's particular objections, which when he tells you, you will perhaps find, that I have already given you the grounds of answering them. And though to propose arguments to evince positively the truth of our religion, after the example of the excellent *Grotius*, and some other very learned writers, be not, as you see, either my task or my design; yet if you attentively consider, what I write in that short discourse, wherein I manage but that seemingly popular argument for Christianity, that is drawn from the miracles, that are said to attest it, you will perchance be invited to think, that when all the other proofs of it are taken in, a man may, without renouncing or affronting his reason, be a Christian.

BUT to proceed to the more considerable part of what I presumed your friend will object, I answer, that the considerations I have alledged in the behalf of some mysteries of the Christian religion, will not be equally applicable to the most absurd or unreasonable opinions. For these consideratians are offered as apologies for Christian doctrines, but upon two or all of these three suppositions. The first, that the truth of the main religion, of which such doctrines make a part, is so far positively proved by real and uncontrouled miracles, and other competent arguments, that nothing, but the manifest and irreconcileable repugnancy of it's doctrines to right reason, ought to hinder us from believing them. The second, that divers of the things, at which reasonable men are wont to take exception, are such, as reason itself may discern to be very difficult, or perhaps impossible for us to understand perfectly by our own natural light. And the third, that some things in Christianity, which many men think contrary to reason, are, at most, but contrary to it, as it is incompetently informed and assisted, but not when it is more fully instructed, and particularly when it is enlightened and assisted by divine revelation. And as I think these three suppositions are not justly applicable, (I say not, as the objection does, to the most absurd, or unreasonable opinions, but) to any other religion than the true, which is the Christian; so the last of these suppositions prompts me to take notice to you, that, though we ought to be exceeding wary, how we admit what pretends to be supernaturally revealed; yet if it be attended with sufficient evidence of its being so, we do very much wrong and prejudice our selves, if, out of an unreasonable jealousy, or to acquire or maintain the repute of being wiſer than others, we ſhut our eyes againſt the light it offers. For beſides that a man may as well err, by rejecting, or ignoring the truth, as by miſtaking a falſhood for it; I conſider, that thoſe men, that have an inſtrument of knowledge, which other men either have not, or, (which is as bad) refuſe to employ, have a very great advantage above others towards the acquiring of truth, and with far leſs parts than they, may diſcover divers things, which the others, with all their pride and induſtry, ſhall never attain to. As when *Galileo* alone among the modern aſtronomers was maſter of a teleſcope, it was eaſy for him to make noble diſcoveries in heaven of things, to which not only *Ptolomy*, *Alphonſus*, and *Tycho*, but even his maſters, *Ariſtarchus Samius* and *Copernicus* themſelves, never dreamed of, and which other aſtronomers cannot ſee but by making uſe of the ſame kind of inſtrument. And on this occaſion let me carry the compariſon, ſuggeſted by the teleſcope, a little further, and take notice, that if men having heard, that there were four planets moving about *Jupiter*, and that *Venus* is an opacous body, and ſometimes horned like the moon, had reſolved to examine theſe things by their naked eyes, as by the proper organs of ſight, without employing the teleſcope, by which they might ſuſpect, that *Galileo* might put ſome optical deluſion upon them; they would perhaps have aſſembled in great multitudes to gaze at *Venus* and *Jupiter*, that (ſince *plus vident oculi quam oculus*) the number of eyes might make amends for their dimneſs. This attempt not ſucceeding, they would perhaps chooſe out ſome of the youngeſt and ſharpeſt ſighted men, that by their piercing eyes that may be diſcovered, which ordinary ones could not reach. And this expedient not ſucceeding neither, they would perhaps diet their ſtar-gazers, and preſcribe them the inward uſe of fennel, and eye-bright, and externally apply collyriums and eye-waters, and thoſe to as little purpoſe as the reſt. With ſuch a pity, mixed with indignation, as *Galileo* would probably have looked on ſuch vain and fruitleſs attempts with, may a judicious Chriſtian, that upon a due examination admits the truth of the ſcriptures, look upon the preſumptuous and vain endeavours of thoſe men, who, by the goodneſs of their natural parts, or by the improvements of them, or by the number of thoſe, that conſpire in the ſame ſearch, think, with the bare eye of reaſon to make as great diſcoveries of heavenly truths, as a perſon aſſiſted by the revelations, contained in the ſcripture, can with great eaſe and ſatisfactorineſs attain. To which let me add this further improvement of the compariſon, that as a ſkilful aſtronomer will indeed, firſt ſeverely examine, whether the teleſcope be an inſtrument fit to be truſted and not likely to impoſe upon him; but being once reſolved of that, will confidently believe the diſcoveries it makes him, however contrary to the received theories of the celeſtial bodies, and to what he himſelf believed before, and would ſtill, if the teleſcope did not otherwiſe inform him, continue to believe; ſo a well qualified inquirer into religions, though he will be very wary, upon what terms he admits ſcripture, yet if he once be fully ſatisfied, that he ought to admit it, he will not ſcruple to receive upon its authority, whatever ſupernatural truths it clearly diſcloſes to him; though perhaps, contrary to the opinions he formerly held, and which, if the ſcripture did not teach him otherwiſe, he would yet aſſent to. And as the galaxy, and other whitiſh parts of the ſky, were by *Ariſtotle* and his followers, and many other philoſophers, who looked on them only with their naked eyes, for many ages reputed to be but meteors; but to thoſe, that look on them with an eye aſſiſted by the teleſcope, they plainly appear true conſtellations made up of a multitude of bright, though little, ſtars; ſo there are theological doctrines, which to philoſophers, and others, that look on them with the naked eye of natural reaſon, ſeem to be but light and fantaſtical things; which yet, when reaſon, aſſiſted and heightened by revelation, comes to contemplate, it manifeſtly ſees them to be true and celeſtial lights, which only their ſublimity keeps concealed from our weak, (naked) eyes.

SOME

SOME PHYSICO-THEOLOGICAL CONSIDERATIONS ABOUT THE POSSIBILITY OF THE RESURRECTION.

The PREFACE.

WHILST the Confiderations about Religion and Reafon, (to which the following effay is annexed) were not yet come from the prefs, the learned publifher of them falling one day into difcourfe with me about the defign they aimed at, and fome of the points they treated of, and particularly the refurrection; our difcourfe occafioned my letting him know, that I had long fince had thoughts, and perhaps imparted fome of them to my friends, about fuch fubjects; and that in particular about the refurrection I had yet by me a manufcript, wherein divers years ago I had endeavoured to fhew, that the philofophical difficulties, urged againft the poffibility of the refurrection, were nothing near fo infuperable, as they are by fome pretended, and by others granted to be. Upon this notice, the curiofity he expreffed to fee this effay, engaged me quickly to bring it him; though my being ready to go from *London* made me do it without ftaying to look it over my felf; much lefs, to add what fince occurred to me about the things treated of in it. But notwithftanding its imperfections, and my unwillingnefs to let it go abroad; efpecially without fome papers, that fhould have preceded it, the learned perufer would not be denied leave to fend it, in my abfence, unaltered to the prefs, and join it to the tract he expected thence; pofitively affirming, that I ought no longer to ftifle a difcourfe, that he judged very feafonable, and thought likely to do good. In which conjecture, if he do not prove miftaken, I hope fome more ingenious than religious men, feeing what can be eafily faid by fo incompetent a pen as mine, for one of the moft oppofed doctrines of Chriftianity, will hereby be made lefs forward to condemn all thofe for deferters of reafon, that fubmit to revelation. And I fhall hope too, on the other fide, that fome more religious, than, in this matter, well-informed men, will be induced to think, that what they call the new philofophy may furnifh us with fome new weapons for the defence of our ancienteft creed; and that corpufcularian principles may not only be admitted without Epicurean errors, but be employed againft them.

SOME

Physico-Theological CONSIDERATIONS, &c.

THE question about which my thoughts are desired being this; "whether to "believe the resurrection of the dead, "which the Christian religion teaches, be not "to believe an impossibility? I shall, before I proceed any farther, crave leave to state the question somewhat more clearly and distinctly; that, being freed from ambiguities, you may the better know in what sense I understand it in my answer; in the returning whereof, your friend need not desire me to insist but upon my own thoughts, unless he could do me the favour to direct me to some author, which I have not yet seen, that has expressly treated, upon philosophical grounds, of the question he proposes.

First then I take it for granted, that he does not mean, whether the resurrection is a thing knowable, or directly provable by the mere light of nature. For, if God had not, in the scripture, positively revealed his purpose of raising the dead, I confess, I should not have thought of any such thing; neither do I know, how to prove that it will be, but by flying, not only to the veracity, but the power of God; who having declared, that he will raise the dead, and being an almighty agent, I have reason to believe, that he will not fail to perform what he has foretold.

Nor do I (secondly) understand the question to be, whether the resurrection be possible to be effected by merely physical agents and means. For that it is not to be brought to pass according to the common course of nature, I presume; after the universal experience of many ages, which have afforded us no instances of it. And though perhaps in speculation it seems not absolutely repugnant to reason, that the scattered parts of a dead body might be reconjoined, soon after the death of the man; yet I think you will easily grant it to be morally impossible, that this should happen to any one person, and much more, that it may, nay, that it will happen to all the persons of mankind at the world's end: so that when I treat of the possibility of the general resurrection, I take it for granted, that God has been pleased to promise and declare, that there shall be one, and that it shall be effected, not by, or according to the ordinary course of nature, but by his own power. On which occasion, I remember, that when our Saviour, treating of the resurrection, silenced the Sadducees, that denied it, he conjoins, as the causes of their error, the two things I have pointed at in this observation, and in the first, that preceeded it: "You err, says he, "not knowing the scriptures, nor the power of "God." And when an angel would assure the blessed virgin, that she should bear a child without the intervention of a man, (which was a case somewhat akin to ours, since it was a production of a human body out of a small portion of human substance in a supernatural way,) he concludes his speech by telling her, "That nothing shall prove impossible to "God."

In the third place, I suppose, that the article of the resurrection, taught by the Christian religion, is not here meant by the proposer in such a latitude, as to comprize all, that any particular church or sect of Christians, much less any private doctor or other writer, hath taught about the resurrection; but only what is plainly taught about it in the holy scriptures themselves. And therefore if besides what is there so delivered, the proposer hath met with any thing, that he judges to be impossible in it's own nature, he hath my free consent to deal with the authors and abettors of such unreasonable opinions, (which I declare my self to be not only unconcerned to defend, but sufficiently disposed to reject,) as rashnesses unfriendly to the growth of Christianity.

4. And now, that I may yet further clear the way for the discourse, that is to follow, and obviate some objections and scruples, which I think it is better seasonably to prevent, than solemnly to answer; I shall desire your leave to lay down in this place a couple of considerations; of which I shall begin with this, that it is no such easy way, as at first it seems, to determine, what is absolutely necessary and but sufficient to make a portion of matter, considered at differing times or places, to be fit to be reputed the same body.

That the generality of men do in vulgar speech allow themselves a great latitude about this affair, will be easily granted by him, that observes the received forms of speaking. Thus *Rome* is said to be the same city, though it hath been so often taken and ruined by the barbarians and others, that perhaps scarce any of the first houses have been left standing, and at least very few remain in comparison of those, that have been demolished, and have had others built in their stead. Thus an university is said to be the same, though some colleges fall to ruin, and new ones are built; and though once in an age all the persons, that composed it, decease, and are succeeded by others. Thus the *Thames* is said to be the same river, that it was in the time of our forefathers, though indeed the water, that now runs under *London* Bridge, is not the same, that ran there an hour ago, and is quite other than that, which will run there an hour hence. And so the flame of a candle is said to be the same for many hours together, though it indeed be every minute a new body, and the kindled particles, that compose it at any time assigned, are continually putting off the form of flame, and are repaired by a succession of like ones.

NOR is it by the vulgar only, that the notion of identity has been uneaſy to be penetrated. For it ſeems, that even the ancient philoſophers have been puzzled about it; witneſs their diſputes, whether the ſhip of *Theſeus* were the ſame after it had (like that of Sir *Francis Drake)* been ſo patched up from time to time to preſerve it as a monument, that ſcarce any plank remained of the former ſhip, new timber having been ſubſtituted in the place of any part, that in length of time rotted. And even in metaphyſicks themſelves, I think it no eaſy taſk to eſtabliſh a true and adequate notion of identity, and clearly determine, what is the true principle of individuation. And at all this I do not much wonder; for almoſt every man, that thinks, conceives in his mind this or that quality or relation, or aggregate of qualites, to be that, which is eſſential to ſuch a body, and proper to give it ſuch a denomination; whereby it comes to paſs, that, as one man chiefly reſpects this thing, and another that in a body, that bears ſuch a name; ſo one man may eaſily look upon a body as the ſame, becauſe it retains what he chiefly conſidered in it, whilſt another thinks it to be changed into a new body, becauſe it has loſt that, which he thought was the denominating quality or attribute. Thus philoſophers and phyſicians diſagree about water and ice, ſome taking the latter to be but the former diſguiſed, becauſe they are both of them cold and ſimple bodies, and the latter eaſily reducible to the former, by being freed from the exceſſive and adventitious degree of coldneſs; whilſt others, looking upon fluidity as eſſential to water, think ice upon the ſcore of its ſolidity to be a diſtinct ſpecies of bodies. And ſo Peripateticks and chemiſts often diſagree about the aſhes and calces of burnt bodies; the firſt referring them to earth, becauſe of their permanency and fixedneſs, and divers of the Spagyriſts, taking them to be bodies *ſui generis*, becauſe common aſhes uſually contain a cauſtick ſalt, whereas earth ought to be inſipid: and the like may be ſaid of ſome wood-aſhes and lime-ſtone, and even coral, which, when well calcined and recent, have a biting taſte, beſides that ſome of them, that are inſipid, may be reduced into metals, as may be eaſily enough tried in the calces of lead and copper.

THESE difficulties about the notion of identity I have therefore taken notice of, that we may not think it ſtrange, that among the ancient Hebrews and Greeks, whoſe languages were ſo remote in ſeveral regards from ours, the familiar expreſſions employed about the ſameneſs of a body ſhould not be ſo preciſe as were requiſite for their turn, who maintain the reſurrection in the moſt rigid ſenſe. And this leads me from the firſt of my two conſiderations to the ſecond.

THAT, then, it is not repugnant or unconſonant to the holy ſcripture, to ſuppoſe, that a comparatively ſmall quantity of the matter of a body, being encreaſed either by aſſimilation or other convenient appoſition of aptly diſpoſed matter, may bear the name of the former body, I think I may reaſonably gather from the three following expreſſions, I meet with in the Old and New Teſtament.

FOR firſt, St. *Paul* in the 15th chapter of his firſt epiſtle to the *Corinthians*, where he profeſſedly treats of the reſurrection, and anſwers this queſtion; "But ſome men will ſay, "how are the dead raiſed up? and with what bo"dy do they come?" ver 35: he more than once explains the matter by the ſimilitude of ſowing, and tells them, ver 37. "That which "thou ſoweſt, thou ſoweſt not that body, that "ſhall be: but bare grain, it may chance of "wheat, or of ſome other grain. Adding, that "God gives this ſeed a body as he thought fit, "to each ſeed its own body, ver. 38." Now, if we conſider the multitude of grains of corn, that may in a good ſoil grow out of one; inſomuch, that our Saviour ſpeaking, in the parable *de Agro Dominico*, of a whole field, tells us, that the grain may well bear a hundred for one; we cannot but think, that the portion of the matter of the ſeed, that is in each of the grains (not to reckon what may be contained in the roots, ſtalk, and chaff,) muſt be very ſmall.

I will not now conſider, whether this text juſtifies the ſuppoſition of a plaſtick power in ſome part of the matter of a deceaſed body; whereby, being divinely excited, it may be enabled to take to its ſelf freſh matter, and ſo ſubdue and faſhion it, as thence ſufficiently to repair or augment itſelf; though the compariſon ſeveral times employed by St. *Paul* ſeems to favour ſuch an hypotheſis. Nor will I examine, what may be argued from conſidering, that leaven, though at firſt not differing from other dough, is by a light change of qualities, that it acquires by time, enabled to work upon and ferment a great proportion of other dough. Nor yet will I here debate, what may be ſaid in favour of this conjecture from thoſe chemical experiments, by which *Kircherus*, a *Polonian* phyſician in *Quercetanus* and others, are affirmed to have, by a gentle heat, been able to re-produce, in well-cloſed vials, the perfect ideas of plants deſtroyed by the fire: I will not, I ſay, in this place enter upon a diſquiſition of any of theſe things, both becauſe I want time to go thorough with it, and becauſe, the reſuſcitation, ſuppoſing the matter of fact, may give no ſmall countenance to our cauſe; yet I do not either abſolutely need it, or perhaps fully acquieſce in all the circumſtances and inferences, that ſeem to belong to it. But one thing there is, that I muſt not leave unmentioned in this place; becauſe I received it, ſoon after the trial was made, from two eminent perſons of my acquaintance, men of great veracity, as well as judgment; whereof one made the experiment, and the other ſaw it made in his own garden, where the trier of the experiment (for he was ſo modeſt, that he would not confeſs himſelf to be the author of it,) took ſome aſhes of a plant, juſt like our Engliſh red poppy; and having ſowed theſe alkaliſate aſhes in my friend's garden, they did, ſooner than was expected, produce certain plants larger and fairer than any of that kind, that had been ſeen in thoſe parts. Which ſeems to argue, that, in the ſaline and earthy, *i. e.* the fixed particles of a vegetable, that has been diſſipated and deſtroyed by the violence of the fire, there may remain a plaſtick power, inabling them to contrive diſpoſed matter, ſo as to re-produce ſuch a body, as was formerly deſtroyed. But to this plaſtick power, reſiding in any portion of the deſtroyed body itſelf, it will not perhaps be neceſſary to have recourſe; ſince an external and omnipotent agent can, without it, perform all that I need contend for: as I think I might gather from that other expreſſion of holy ſcripture, that I meet with in the ſecond chapter of *Geneſis*, where it is ſaid, "That "the Lord God cauſed a deep ſleep to fall up"on *Adam*, and he ſlept; and he took one of "his ribs, and cloſed up the fleſh inſtead "thereof. And the rib, which the Lord God "had taken from man, made he a woman, "and brought her unto the man, ver. 21, 22." For, ſince it cannot be pretended, that either the whole, or any conſiderable portion of *Eve*'s body was taken out of *Adam*'s, which was deprived but of a rib; and ſince it cannot be probably affirmed, that this rib had any ſpermatick faculty, both becauſe the text aſſigns the formation of the woman to God, and becauſe the ſeminal principles in animals require the commixture of male and female, the latter of which the text ſuppoſes not to have been then made; why may I not conclude, that, if it pleaſe God, by his immediate operation, to take a portion of the matter of a human body, and add to it a far greater quantity, either of newly created, or of pre-exiſtent matter; the new body ſo framed may, congruouſly enough to ſcripture-expreſſions, be reputed to be made of the former body. And accordingly *Adam* (ver. 23.) gives the reaſon why, he called his wife *Iſha*, which our tranſlation renders woman; becauſe ſhe was taken out of *Iſh*, which in our verſion is rendered man.

The other text, that I conſider, to my preſent purpoſe, is the myſtical reſurrection deſcribed in *Ezekiel*'s viſion, where all, that remain of the dead men, that were to riſe up an army of living men, was a valley full of dry bones, which being by the divine Power approached to one another, and made to join together in a convenient manner, were afterwards by the ſupernatural appoſition of either newly created, or extrinſecally ſupplied matter,
Ezekiel furniſhed with ſinews, (by which I ſuppoſe is
xxxvii. meant not only nerves, but veſſels, tendons,
Ver. 7, 8. ligaments, &c.) and fleſh covered with ſkins;
and laſt of all, a vivifying ſpirit was conveyed
Ver. 9, 10. into them, that made them ſtand upon their
feet alive, an exceeding great army. Whence I gather, that it is not unconſonant to the expreſſions of ſcripture, to ſay, that a portion of the matter of a dead body, being united with a far greater portion of matter furniſhed from without by God himſelf, and completed into a human body, may be reputed the ſame man, that was dead before. Which may appear, both by the tenor of the viſion, and particularly from the expreſſion ſet down in the tenth verſe, where God, calling for the enlivening ſpirit, names the compleated, but not yet revived bodies, *theſe ſlain*, as if he now counted them the ſame, that had formerly been killed.

These preliminary conſiderations being thus laid down, we may now proceed to examine more cloſely thoſe difficulties, which are ſaid to demonſtrate the impoſſibility of the reſurrection; the ſubſtance of which difficulties may be compriſed in this objection.

When a man is once really dead, divers of the parts of the body will, according to the courſe of nature, reſolve themſelves into multitudes of ſteams, that wander to and fro in the air; and the remaining parts, that are either liquid or ſoft, undergo ſo great a corruption and change, that it is not poſſible ſo many ſcattered parts ſhould be again brought together, and re-united after the ſame manner wherein they exiſted in a human body, whilſt it was yet alive. And much more impoſſible it is to effect this re-union, if the body have been, as it often happens, devoured by wild beaſts or fiſhes; ſince in this caſe, though the ſcattered particles of the cadaver might be recovered as particles of matter, yet having already paſſed into the ſubſtance of other animals, they are quite tranſmuted, as being informed by the new form of the beaſt or fiſh that devoured them, and of which they now make a ſubſtantial part.

And yet far more impoſſible will this redintegration be, if we put the caſe, that the dead body be devoured by Canibals; for then the ſame fleſh belonging ſucceſſively to two differing perſons, it is impoſſible that both ſhould have it reſtored to them at once, or that any footſteps ſhould remain of the relation it had to the firſt poſſeſſor.

In anſwer to this (indeed weighty) objection, I have ſeveral things to offer.

And firſt, I conſider, that a human body is not as a ſtatue of braſs or marble, that may continue, as to ſenſe, whole ages in a permanent ſtate; but is in a perpetual flux, or changing condition, ſince it grows in all its parts, and all its dimenſions, from a corpuſculum, no bigger than an inſect, to the full ſtature of man; which in many perſons, that are tall and fat, may amount to a vaſt bulk, which could not happen but by a conſtant appoſition and aſſimilation of new parts to the primitive ones of the little embryo; and ſince men, as other animals, grow but to a certain pitch, and till a certain age, (unleſs perhaps it be the crocodile, which ſome affirm to grow always till death,) and therefore muſt diſcharge a great part of what they eat and drink by inſenſible tranſpiration, which *Sanctorius*'s Statical Experiments, as well as mine, aſſure me to be ſcarce credibly great, as to men and ſome other animals, both hot and cold; it will follow, that, in no very great compaſs of time, a great part of the ſubſtance of a human body muſt be changed: and yet it is conſiderable, that the bones are of a ſtable and laſting texture, as I found not only by ſome chemical trials, but by the ſculls and bones of men, whom hiſtory records

to

to have been killed an exceeding long time ago, of which note we may hereafter make uſe.

SECONDLY, I conſider, that there is no determinate bulk or ſize, that is neceſſary to make a human body paſs for the ſame, and that a very ſmall portion of matter will ſometimes ſerve the turn; as an embryo, for inſtance, in the womb, a new-born babe, a man at his full ſtature, and a decrepit man of perhaps an hundred years old, notwithſtanding the vaſt difference of their ſizes, are ſtill reputed to be the ſame perſon; as is evident, by the cuſtom of crowning kings and emperors in the mother's belly, and by putting murderers, &c. to death in their old age, for crimes committed in their youth; and if a very tall and unweildy fat man ſhould, as it ſometimes happens, be reduced by a conſumption to a ſkeleton, as they ſpeak, yet none would deny, that this waſted man were the ſame with him, that had once ſo enormouſly big a body.

I conſider alſo, that a body may either conſiſt of, or abound with ſuch corpuſcles, as may be variouſly aſſociated with thoſe of other bodies, and exceedingly diſguiſed with thoſe mixtures, and yet retain their own nature; of this we have divers inſtances in metalline bodies: thus gold, for example, when diſſolved in aqua regis, paſſes for a liquor, and when dexterouſly coagulated, it appears a ſalt or vitriol: by another operation, I have taken pleaſure to make it part of the fuel of a flame: being dexterouſly conjoined to another mineral, it may be reduced to glaſs: being well precipitated with mercury, it makes a glorious tranſparent powder: being precipitated with ſpirit of urine, or oil of tartar *per deliquium*, it makes a fulminating calx, that goes off very eaſily, yet is far ſtronger than gun-powder: being precipitated with a certain other alkali, the fire turns it to a fixed and purple calx. And yet in ſpite of all theſe, and divers other diſguiſes, the gold retains its nature; as may be evinced by chemical operations, eſpecially by reductions. Mercury alſo is a greater Proteus than gold, ſometimes putting on the form of a vapour; ſometimes appearing in that of an almoſt inſipid water; ſometimes aſſuming, in that condition, the form of a red powder; ſometimes that of a white one, and of a yellow one, or of a chryſtalline ſalt, of a malleable metal; of what not? And yet all theſe are various dreſſes of the ſame quickſilver, which a ſkilful artiſt may eaſily make it put off, and re-appear in its native ſhape.

AND though it be true, that inſtances of the permanence of corpuſcles, that paſs under ſucceſſive diſguiſes, may be much eaſier found among metals and minerals, than vegetables and animals; yet there are ſome to be met with among theſe: for, not to mention *Hippocrates* his affirmation about purging a child with the milk of an animal, that had taken *Elaterium*, (if I miſ-remember not the drug,) not to mention this, I ſay; I remember, that when I once paſſed a ſpring in *Savoy*, I obſerved, that all the butter, that was made in ſome places, taſted ſo rank of a certain weed, that at that time of the year abounds there in the fields, that it made ſtrangers much nauſeate the butter, which otherwiſe was very good. If it be conſidered, how many, if I may ſo call them, elaborate alterations the rank corpuſcles of this weed muſt have undergone in the various digeſtions of the cow's ſtomach, heart, breaſts, &c. and that afterwards, two ſeparations, at leaſt, were ſuperadded, the one of the cream from the reſt of the milk, and the other of the unctuous parts of the cream from the ſerum or butter-milk; it will ſcarce be denied, but that vegetable corpuſcles may, by aſſociation, paſs through divers diſguiſes, without loſing their nature; eſpecially conſidering, that the eſſential attributes of ſuch corpuſcles may remain undeſtroyed, though no ſenſible quality ſurvive to make proof of it; as in our newly mentioned example the offenſive taſte did. And beſides what we commonly obſerve on the ſea-coaſt, of the fiſhy taſte of thoſe ſea-birds, that feed only upon ſea-fiſh, I have purpoſely enquired of an obſerving man, that lived upon a part of the Iriſh coaſt, where the cuſtom is to fatten their hogs with a ſhell-fiſh, which that place very much abounds with, about the taſte of their pork: to which he anſwered me, that the fleſh had ſo ſtrong and rank a taſte of the fiſh, that ſtrangers could not endure to eat it. There is a certain fruit in *America*, very well known to our Engliſh planters, which many of them call the prickle-pear, whoſe very red juice being eaten with the pulp of the fruit, whereof it is a part, doth ſo well make its way through the divers ſtrainers and digeſtions of the body, that it makes the urine red enough to perſuade thoſe, that are unacquainted with this property, that they piſs blood; as I have been ſeveral times aſſured by unſuſpected eye-witneſſes. But more odd is that, which is related by a learned man, that ſpent ſeveral years upon the Dutch and Engliſh plantations in the *Charibbe-Iſlands*, who ſpeaking of a fruit, (which I remember I have ſeen, but had not the liberty to make trial of it,) called janipa, or junipa, growing in ſeveral of thoſe iſlands, he tells us, among other things, that *au temps*, &c. which is, at the ſeaſon, when this fruit falls from the tree, the hogs, that feed on it, have both their fleſh and fat of a violent colour, as experience witneſſeth, (which colour is the ſame, that the juice dies;) and the like happens to the fleſh of parrots and other birds, that feed upon it. I ſhall by and by give you an inſtance of a vegetable ſubſtance, which, though torn in pieces by very corroſive liquors, and ſo diſguiſed as to leave no ſuſpicion of what it was, does thereby not only loſe its nature, but is in an immediate capacity of re-appearing cloathed even with the ſenſible qualities of it, as colour, taſte, and ſmell.

HAVING thus ſhewn, that the particles of a body may retain their nature under various diſguiſes, I now proceed to add, that they may be ſtripped of thoſe diſguiſes, or, to ſpeak with-

without a metaphor, be extricated from those compositions, wherein they are disguised, and that sometimes by such ways, as those, that are strangers to the nicer operations of nature, would never have thought upon, nor will not perhaps judge probable, when proposed. It is not unknown to expert chemists, that, in despite of all the various shapes, which that Proteus, mercury, may be made to appear in, as of a cristalline sublimate, a red precipitate, a yellow turbith, a vapor, a clear water, a cinaber, &c. a skilful method of reduction will quickly free it from all, that made it impose upon our senses, and re-appear in the form of plain running mercury. And though vitrification be looked upon by chemists, as the ultimate action of the fire, and powerfulest way of making inseparable conjunctions of bodies; yet even out of glass of lead, for instance, (made of sand, and the ashes of a metal,) though the transmutation seems so great, that the dark and flexible metal is turned into a very trasparent and brittle mass; yet even from this have we recovered opacous and malleable lead. And though there be several ways, besides precipitations, of divorcing substances, that seem very strictly, if not unseparably united; (which though I may, perhaps, have practised, it is not now convenient I should discourse of;) yet, by precipitation alone, if a man have the skill to choose proper precipitants, several separations may not only be made, but be easily and thoroughly made, that every one would not think of: for, it is not necessary, that in all precipitations, as is observed in most of the vulgar ones, the precipitant body should indeed make a separation of the dissolved body from the mass, or bulk of that liquor, or other adjunct, whereto it was before united, but should not be able to perform this without associating its own corpuscles with those of the body it should rescue, and so make in some sense a new and further composition. For, that some bodies may precipitate others, without uniting themselves with them, is easily proved by the experiment of refiners, separating silver from copper; for, the mixture being dissolved in aqua fortis, if the solution be afterwards diluted, by adding fifteen or twenty times as much common water, and you put into this liquor a copper-plate, you shall quickly see the silver begin to adhere to the plate, not in the form of a calx, as when gold is precipitated to make *aurum fulminans*, or tin-glass to make a fine white powder for a *Fucus*; but in the form of a shining metalline substance, that needs no farther reduction to be employed as good silver. And by a proper precipitant, I remember, I have also in a trice (perhaps in a minute of an hour) reduced a pretty quantity of well disguised mercury into running quicksilver. And if one can well appropriate the precipitants to the bodies they are to recover, very slight and unpromising agents may perform great matters in a short time; as you may guess by the experiment I lately promised you: which is this, that, if you take a piece of camphire, and let it lie a-while upon oil of vitriol, shaking them now and then, it will be so corroded by the oil, as totally to disappear therein, without retaining so much as its smell, or any manifest quality, whereby one may suspect there is camphire in that mixture; and yet, that a vegetable substance, thus swallowed up, and changed by one of the most fretting and destroying substances, that is yet known in the world, should not only retain the essential qualities of its nature, but be restorable to its obvious and sensible ones, in a minute, and that by so unpromising a medium as common water, you will readily grant, if you pour the dissolved camphire into a large proportion of that liquor, to whose upper parts it will immediately emerge white, brittle, strong-scented, and inflammable camphire, as before.

One main consideration I must add to the foregoing ones, namely, that body and body being but a parcel, and a parcel of universal matter mechanically different; either parcel may successively put on forms in a way of circulation, if I may so speak, till it return to the form, whence the reckoning was begun, having only its mechanical affections altered.

That all bodies agree in one common matter, the schools themselves teach, making what they call the *materia prima* to be the common basis of them all, and their specifick differences to spring from their particular forms: and since the true notion of body consists either alone in its extension, or in that, and impenetrability together, it will follow, that the differences, which make the varieties of bodies we see, must not proceed from the nature of matter, of which, as such, we have but one uniform conception; but from certain attributes; such as motion, size, position, &c. that we are wont to call mechanical affections. To this it will be congruous, that a determinate portion of matter being given, if we suppose, that an intelligent and otherwise duly qualified agent do watch this portion of matter in its whole progress, through the various forms it is made to put on, till it come to the end of its course, or series of changes; if, I say, we suppose this, and withal, that this intelligent agent lay hold of this portion of matter cloathed in its ultimate form, and extricating it from any other parcels of matter, wherewith it may be mingled, make it exchange its last mechanical affections for those, which it had, when the agent first began to watch it; in such case, I say, this portion of matter, how many changes and disguises soever it may have undergone in the mean time, will return to be what it was; and if it were before part of another body to be re-produced, it will become capable of having the same relation to it, that formerly it had.

To explain my meaning by a gross example; suppose a man cut a large globe, or sphere, of soft wax, in two equal parts or hemispheres, and of the one make cones, cylinders, rings, screws, &c. and kneading the other with dough, make an appearance of pie-crust, cakes, vermicelli, (as the Italians call paſte, ſqueezed through a perforated plate into the form of little worms,) wafers, biſkets, &c. it is plain, that a man may by diſſolution, and other ways, ſeparate the wax from the dough or paſte, and reduce it in a mould to the ſelf-ſame hemiſphere of wax it was before, and ſo he may deſtroy all, that made the other part of the wax paſs for ſeveral bodies, as cones, or cylinders, or rings, &c. and may reduce it in a mould to one diſtinct ſemi-globe, fit to be re-conjoined to the other, and ſo to re-compoſe ſuch a ſphere of wax, as they conſtituted, before the biſection was made. And to give you an example to the ſame purpoſe, in a caſe, that ſeems much more difficult; if you look upon precipitate, carefully made *per ſe*, you would think, that art has made a body extremely different from the common mercury; this being conſiſtent like a powder, very red in colour, and purgative, and for the moſt part vomitive in operation, though you give but four or five grains of it; and yet, if you but preſs this powder with a due heat, by putting the component particles into a new and fit motion, you may re-unite them together, ſo as to re-obtain, or re-produce the ſame running mercury you had, before the precipitate *per ſe* was made of it.

HERE I muſt beg your leave to recommend more fully to your thoughts that, which, ſoon after the beginning of this diſcourſe, I did (purpoſely) but touch upon, and invite you to conſider with me, that the Chriſtian doctrine doth not aſcribe the reſurrection to nature, or any created agent, but to the peculiar and immediate operation of God, who has declared, that, before the very laſt judgment, he will raiſe the dead. Wherefore, when I lately mentioned ſome chemical ways of recovering bodies from their various diſguiſes, I was far from any deſire it ſhould be imagined, that ſuch ways were the only, or the beſt, that can poſſibly be employed to ſuch an end. For, as the generality of men, without excepting philoſophers themſelves, would not have believed or thought, that, by eaſy chemical ways, bodies, that are reputed to have paſſed into a quite other nature, ſhould be reduced or reſtored to their former condition; ſo, till chemiſtry, and other parts of true natural philoſophy, be more throughly underſtood, and farther promoted, it is probable, that we can ſcarce now imagine, what expedients to re-produce bodies a further diſcovery of the myſteries of art and nature may lead us mortals to. And much leſs can our dim and narrow knowledge determine, what means, even phyſical ones, the moſt wiſe author of nature, and abſolute governor of the world, is able to employ to bring the reſurrection to paſs, ſince it is a part of the imperfection of inferior natures to have but an imperfect apprehenſion of the powers of one, that is incomparably ſuperior to them. And even among us, a child, though endowed with a reaſonable ſoul, cannot conceive, how a geometrician can meaſure inacceſſible heights and diſtances, and much leſs how a coſmographer can determine the whole compaſs of the earth and ſea, or an aſtronomer inveſtigate, how far it is from hence to the moon, and tell many years before, what day and hour, and to what degree, ſhe will be eclipſed. And indeed in the *Indies*, not only children, but rational illiterate men, could not perceive, how it was poſſible for the Europeans to converſe with one another by the help of a piece of paper, at an hundred miles diſtance, and in a moment produce thunder and lightening, and kill men a great way off, as they ſaw gunners and muſketeers do, and much leſs foretel an eclipſe of the moon, as *Columbus* did to his great advantage; which things made the Indians, even the chiefeſt of them, look upon the Spaniards, as perſons of a more than human nature. Now among thoſe, that have a true notion of a Deity, which is a Being both omnipotent and omniſcient; that he can do all, and more than all, that is poſſible to be performed by any way of diſpoſing of matter and motion, is a truth, that will be readily acknowledged, ſince he was able at firſt to produce the world, and contrive ſome part of the univerſal matter of it into the bodies of the firſt man and woman. And that his power extends to the re-union of a ſoul and body, that have been ſeparated by death, we may learn from the experiments God has been pleaſed to give of it both in the Old Teſtament and the New, eſpecially in the raiſing again to life *Lazarus* and Chriſt; of the latter of which particularly, we have proofs cogent enough to ſatisfy any unprejudiced perſon, that deſires but competent arguments to convince him. And that the miraculous power of God will be, as well as his veracity is, engaged in raiſing up the dead, and may ſuffice, if it be ſo, we may not difficultly gather from that excellent admonition of our Saviour to the Sadducees, where he tells them, (as I elſewhere noted) that the two cauſes of their errors are, their not knowing the ſcriptures, wherein God hath declared, he will raiſe the dead; nor the power of God, by which he is able to effect it. But the engagement of God's omnipotence is alſo in that place clearly intimated by St. *Paul*, *Acts* xxvi. 8. where he aſks king *Agrippa*, and his other auditors, why they ſhould think it a thing not to be believed, (ἄπιϛον,) that God ſhould raiſe the dead. And the ſame truth is yet more fully expreſſed by the ſame apoſtle, where ſpeaking of Chriſt returning in the glory and power of his father, to judge all mankind, after he has ſaid, that this divine judge ſhall transform, or transfigure (μεlασχηματίζειν) our vile bodies (ſpeaking of his own, and thoſe of other ſaints,) to ſubjoin the account on which this ſhall be done, he adds, "That it will be according to the "powerful working, (ἐνέργειαν) whereby he is "able even to ſubdue all things to himſelf, "*Phil.* iii. 21."

AND now, it will be ſeaſonable to apply, what has been delivered in the whole paſt diſcourſe, to our preſent purpoſe.

SINCE then a human body is not ſo confined to a determinate bulk, but that the ſame ſoul, being united to a portion of duly organized

nized matter, is ſaid to conſtitute the ſame man, notwithſtanding the vaſt differences of bigneſs, that there may be, at ſeveral times, between the portions of matter, whereto the human ſoul is united:

SINCE a conſiderable part of the human body conſiſts of bones, which are bodies of a very determinate nature, and not apt to be deſtroyed by the operation, either of earth or fire:

SINCE, of the leſs ſtable, and eſpecially the fluid parts of a human body, there is a far greater expence made by inſenſible tranſpiration, than even philoſophers would imagine:

SINCE the ſmall particles of a reſolved body may retain their own nature, under various alterations and diſguiſes, of which it is poſſible they may be afterwards ſtripped:

SINCE, without making a human body ceaſe to be the ſame, it may be repaired and augmented by the adaptation of congruouſly diſpoſed matter to that, which pre-exiſted in it:

SINCE, I ſay, theſe things are ſo, why ſhould it be impoſſible, that a moſt intelligent agent, whoſe omnipotency extends to all that is not truly contradictory to the nature of things, or to his own, ſhould be able ſo to order and watch the particles of a human body, as, that partly of thoſe, that remain in the bones, and partly of thoſe, that copiouſly fly away by inſenſible tranſpiration, and partly of thoſe, that are otherwiſe diſpoſed of upon their reſolution, a competent number may be preſerved or retrieved? ſo, that ſtripping them of their diſguiſes, or extricating them from other parts of matter, to which they may happen to be conjoined, he may re-unite them betwixt themſelves, and, if need be, with particles of matter fit to be contexed with them, and thereby reſtore or reproduce a body; which, being united with the former ſoul, may, in a ſenſe conſonant to the expreſſions of ſcripture, recompoſe the ſame man, whoſe ſoul and body were formerly disjoined by death.

WHAT has been hitherto diſcourſed, ſuppoſes the doctrine of the reſurrection to be taken in a more ſtrict and litteral ſenſe, becauſe I would ſhew, that, even according to that, the difficulties of anſwering what is mentioned againſt the poſſibility of it, are not inſuperable; though I am not ignorant, that it would much facilitate the defence and explication of ſo abſtruſe a thing, if their opinion be admitted, that allow themſelves a greater latitude, in expounding the article of the reſurrection, as if the ſubſtance of it were, that, in regard the human ſoul is the form of man, ſo that whatever duly organized portion of matter it is united to, it therewith conſtitutes the ſame man, the import of the reſurrection is fulfilled in this; that, after death there ſhall be another ſtate, wherein the ſoul ſhall no longer perſevere in its ſeparate condition, or, as it were, widowhood, but ſhall be again united, not to an aetherial, or the like fluid matter, but to ſuch a ſubſtance as may, with tolerable propriety of ſpech, notwithſtanding its differences from our houſes of clay, (as the ſcripture ſpeaks) be called a Job iv. 19.
human body.

THEY, that aſſent to what has been hitherto diſcourſed of the poſſibility of the reſurrection of the ſame bodies, will, I preſume, be much more eaſily induced to admit the poſſibility of the qualifications the Chriſtian religion aſcribes to the glorified bodies of the raiſed ſaints. For, ſuppoſing the truth of the hiſtory of the ſcriptures, we may obſerve, that the power of God has already extended itſelf to the performance of ſuch things, as import as much as we need infer, ſometimes by ſuſpending the natural actings of bodies upon one another, and ſometimes by endowing human and other bodies with preternatural qualities. And indeed lightneſs, or rather agility, indifferent to gravity and levity, incorruption, tranſparency and opacity, figure, colour, &c. being but mechanical affections of matter, it cannot be incredible, that the moſt free and powerful author of thoſe laws of nature, according to which, all the phænomena of qualities are regulated, may (as he thinks fit) introduce, eſtabliſh, or change them in any aſſigned portion of matter, and conſequently in that, whereof a human body conſiſts. Thus, though iron be a body above eight times heavier, bulk for bulk, than water, yet, in the caſe of *Eliſha*'s helve, its native gravity was rendered ineffectual, and it emerged from the bottom to the top of the water: and the gravitation of St. *Peter*'s body was ſuſpended, whilſt his maſter commanded him, and by that command enabled him to come to him walking on the ſea. Thus the operation of the activeſt body in nature, flame, was ſuſpended in *Nebuchadnezzar*'s fiery furnace, whilſt *Daniel*'s three companions walked unharmed in thoſe flames, that, in a trice, conſumed the kindlers of them. Thus did the Iſraelites manna, which was of ſo periſhable a nature, that it would corrupt in little above a day, when gathered in any day of the week but that, which preceded the ſabbath, keep good twice as long, and, when laid up before the ark for a memorial, would laſt whole ages uncorrupted. And to add a proof, that comes more directly home to our purpoſe, the body of our Saviour, after his reſurrection, though it retained the very impreſſions, that the nails of the croſs had made in his hands and feet, and the wound, that the ſpear had made in his ſide, and was ſtill called in the ſcripture his body, as indeed it was, and more ſo, than, according to our paſt diſcourſe, it is neceſſary, that every body ſhould be, that is rejoined to the ſoul in the reſurrection: and yet this glorified body had the ſame qualifications, that are promiſed to the ſaints in their ſtate of glory; St. *Paul* informing us, "That our vile "bodies ſhall be transformed into the like-"neſs of his glorious body," which the hiſtory of the goſpel aſſures us, was endowed with far nobler qualities than before its death. And whereas the apoſtle adds, as we formerly noted, that this great change of ſchematiſm, in the ſaints bodies, will be effected by the irreſiſtible

irreſiſtible power of Chriſt, we ſhall not much ſcruple at the admiſſion of ſuch an effect from ſuch an agent, if we conſider, how much the bare, ſlight, mechanical alteration of the texture of a body, may change its ſenſible qualities for the better. For, without any viſible additament, I have ſeveral times changed dark and opacous lead into finely coloured tranſparent and ſpecifically lighter glaſs. And there is another inſtance, which, though becauſe of its obviouſneſs it is leſs heeded, is yet more conſiderable: for who will diſtruſt, what advantageous changes ſuch an agent as God can work, by changing the texture of a portion of matter, if he but obſerve, what happens merely upon the account of ſuch a mechanical change in the lighting of a candle, that is newly blown out, by the applying another to the aſcending ſmoke. For, in the twinkling of an eye, an opacous, dark, languid and ſtinking ſmoke loſes all its ſtink, and is changed into a moſt active penetrant and ſhining body.

A CONJECTURE CONCERNING THE BLADDERS of AIR, that are found in FISHES,

Communicated by *A. J.*

And illuſtrated by an EXPERIMENT.

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REFLECTING on that queſtion, whether liquids gravitate upon bodies immerſed or not? I came to a reſolution in my own thoughts, that they do gravitate; and one of the greateſt inſtances, that did occur to me was, that a bubble of air, riſing from the bottom, does dilate itſelf all the way to the top; which is cauſed by the leſſening of the weight or preſſure of the incumbent water, the nearer it is to the top. Upon conſideration of that inſtance, the following conjecture preſented itſelf to my thoughts; that fiſhes, by reaſon of the bladder of air, that is within them, can ſuſtain or keep themſelves in any depth of water. For the air in that bladder is like the bubble, more or leſs compreſſed, according to the depth the fiſh ſwims at, and takes up more or leſs ſpace; and conſequently the body of the fiſh, part of whoſe bulk this bladder is, is greater or leſs according to the ſeveral depths, and yet retains the ſame weight. The rule *de inſidentibus humido* is, that a body, that is heavier than ſo much water, as is equal in quantity to the bulk of it, will ſink; a body, that is lighter, will ſwim; a body of equal weight will reſt in any part of the water.

Now by this rule, if the fiſh in the middle region of the water be of equal weight to the water, that is, commenſurate to the bulk of it, the fiſh will reſt there without any tendency upwards or downwards: and if the fiſh be deeper in the water, the bulk of the fiſh becoming leſs by the compreſſion of the bladder, and yet retaining the ſame weight, it will ſink and reſt at the bottom: and on the other ſide, if the fiſh be higher than that middle region, the air dilating it ſelf, and the bulk of the fiſh conſequently encreaſing, but not the weight, the fiſh will riſe upwards, and reſt at the top of the water.

PERHAPS the fiſh by ſome action can emit air out of this bladder, and afterwards out of its body, and alſo, when there is not enough, take in air and convey it to this bladder; and then it will not be wondered, that there ſhould be always a fit proportion of air in the bodies of all fiſhes, to ſerve their uſe, according to the depth of water they are bred and live in: perhaps by ſome muſcle the fiſh can contract this bladder beyond the preſſure of the weight of water: perhaps the fiſh can by its ſides, or ſome other defence, keep off the preſſure of the water, and give the air leave to dilate itſelf. In theſe caſes the fiſh will be helped in all intermediate diſtances, and may riſe or ſink from any region of the water without moving one finn.

IT were worth obſerving, what fiſhes want bladders, and if the bladders of ſeveral fiſhes are not of different ſhapes or bigneſs, and how they are in ſea-fiſhes, that live in great depths, and whether any amphibious creatures have them, or any thing analogous; as the lungs may be, or other cavities. By an inquiry into theſe, and other particulars, this conjecture may be either fortified or refuted.

[So far this conjecture: in reference to which, when it was propounded to the honourable *Robert Boyle*, he, reflecting upon the manner how a fiſh comes to riſe or ſink in water, ſoon bethought himſelf of an experiment probably to determine, whether a fiſh makes thoſe motions by conſtricting or expanding himſelf? the experiment by him ſuggeſted was; to take a bolthead with a wide neck, and having filled it almoſt full with water, to put into it ſome live fiſh of a convenient ſize, that is, the biggeſt, that can be got in, as a roch, perch, or the like; and then to draw out the neck of the bolthead as ſlender as you can; and to fill that alſo with water: whereupon the fiſh lying at a certain depth in the water of the glaſs, if upon his ſinking you perceive the water at the ſlender top does ſubſide, you may infer, he contracts himſelf, and if, upon his riſing, the water be alſo raiſed, you may conclude, he dilates himſelf.]

A NEW ESSAY INSTRUMENT, Together with the USES thereof.

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SECION. I.

Shewing the occaſion of making this new eſſay-inſtrument, together with the hydroſtatical principles it is founded on.

TO give you now a more explicite and particular account, than I had then time to do, of the inſtrument, which you ſaw tried at the Royal Society, I ſhall inform you, on what grounds I deviſed it, and then annex ſome obſervations about the fabrick and the uſes of it.

You may remember, that many years ago I ſhewed you a little glaſs-inſtrument, conſiſting of a bubble, furniſhed with a long and ſlender ſtem, which was to be put into ſeveral liquors, to compare and eſtimate their ſpecifick gravities, and which I made uſe of to ſome purpoſes, for which it is not, that I know, as yet employed. But afterwards conſidering this little inſtrument ſomewhat more attentively, I thought the application of it might eaſily be, as it were, inverted, and that, whereas it was employed but to diſcover the differing gravities of ſeveral liquors, by its various degrees of immerſion in them, it might be employed to diſcover the ſpecifick gravities of ſeveral appended ſolids, by its being more or leſs depreſſed by them in the ſame liquor. For it is clearly deducible from the grounds of the hydroſtaticks, that any ſolid body, heavier than water, looſes in the water as much of the weight it had in the air, as water of equal bulk to the immerſed ſolid would weigh in the air; and conſequently, ſince gold is by far the moſt ponderous of metals, a piece of gold, and one of equal weight of copper, braſs, or any other metal, being propoſed, the gold muſt be leſs in bulk, than the copper or braſs. And by this means, if both of them be weighed in the water, the gold muſt looſe in that liquor leſs of its former weight than the braſs or copper; becauſe the baſer metal, as well as the gold, grows higher by the weight of a bulk of water equal to it; and the baſer metal being the more voluminous, the correſpondent water muſt weigh more than that, which is equal to the gold.

THIS hydroſtatical principle may be evidently proved from what has been demonſtrated in a mathematical way, by the moſt ſubtile Archimedes *de inſidentibus humido*, and his commentators; and thoſe, that are either unacquainted with, or diſtruſtful of ſuch ratiocinations, may find the principle made out in a phyſical and experimental way in another paper. Whence I concluded, that I might ſafely infer, that the floating inſtrument abovementioned would be made to ſink deeper by an ounce, for inſtance, of gold hanging at it under water, than by an ounce of braſs or any other metal, which by reaſon of its greater bulk than gold, looſing more of its weight by the immerſion, muſt needs retain leſs, and ſo have leſs power to depreſs the inſtrument it was faſtened to. Which concluſion you will eaſily believe the event did upon trial exactly juſtify; and I preſume you will as little doubt, that the concluſion will alſo hold (though the diſparity be not ſo great and conſpicuous) in reference to other metals, as lead and tin, that differ in ſpecifick gravity.

V. Hydroſt. Paradox.

To give at once an inſtance of the truth and uſe of this notion, I was included to fit the inſtrument, that was grounded on it, for the examination of guineas, which are by far the moſt uſual gold-coins, that paſs in *England*. And though the exactneſs and diligence of our ingenious friend Mr. *Slingſby* allows us to expect, that no injury, that care and ſkill can prevent, ſhall be done to that coin; yet becauſe ſome goldſmiths and others retain fears of being deceived by the fradulent and ſubtile artifices of falſe coiners, I thought it might not be amiſs to furniſh them with an eaſy and practical way of diſtinguiſhing a true guinea from a counterfeit. And though I hope I need not tell you, that I look not upon the inſtrument I ſhewed you at *Greſham-College*, as capable of examining gold and other metals with as much nicety, as by other methods one may hydroſtatically do; yet this little trifle may on ſome occaſions be preferable, ſince the inſtrument, which is not dear, being once fitted, there is no need to have either exact ſcales, or ſkill in hydroſtaticks, or any knowledge of arithmetick, and yet the difference of a true guinea from a counterfeit will not only be ſufficiently, but conſpicuouſly made to appear, and the operation will be much ſooner performed than in the other way, and very much ſooner and cheaper than by the methods commonly employed by goldſmiths and refiners. For, in our way the coin is not defaced or injured by cutting, punching, &c. nor is there any need of touch-ſtones, or aqua-fortis, and yet the trial is ſo quickly made, that perhaps near twenty guineas may be examined, one by one, in about a quarter of an hour: I ſay, one by one, becauſe, that if the inſtrument be deſigned and fitted for ſuch a purpoſe, many guineas may be tried at once. But whether the goldſmiths will make uſe of this way, I leave them to determine; it being ſufficient for me, to have gratified ſuch virtuoſi, eſpecially the diſciples of *Vulcan*, as have given occaſion to expect this trifle will be acceptable unto them; and to add this inſtance to thoſe I have elſewhere given by way of proof, that by the knowledge of cauſes men may employ exceedingly differing means to produce the ſame effects (as in our caſe, gold, that chemiſts and ſay-maſters are fain to examine by the fire, we examine by water) and, that philoſophical truths, and particularly hydroſtatical ones, are not lightly to be deſpiſed, as airy and empty ſpeculations, ſince they may be ſometimes applied to practical uſes, to which at firſt ſight they ſeem to have no relation at all.

SECTION. II.

Deſcribing the conſtruction of this inſtrument.

I PROCEED now to the conſtruction of the inſtrument itſelf, in which are to be conſidered the matter and the form.

THE matter may be glaſs, copper, ſilver, or almoſt any other ſolid body, that is, or may be made, fit to float in the water, with a guinea hanging at it, and of a texture cloſe enough to keep out the water. For, if any of that ſhould, by ſoaking or otherwiſe, get in, it may alter the gravity of the inſtrument, and render it deceitful.

MY firſt trials were made with bubbles of glaſs, furniſhed with ſlender ſtems, hermetically ſealed at the top; and theſe, when one can procure an artificer, that can blow them well, are both the gentileſt and the cheapeſt, and for ſome of the uſes, that may hereafter be mentioned, they are almoſt the only ones, that can be fitly employed. But beſides that it is not eaſy to meet with artificers, that can give glaſs the right bigneſs and ſhape, thoſe, as all other inſtruments of glaſs, being very frail and ſubject to be broken; the ſafeſt way and more durable is, to make them of ſome metal, eſpecially either copper or ſilver, (of which the former is far more cheap, and the other more gentile, but either will ſerve well;) in regard they are leſs heavy, and, being more ſtiff, will maintain their figure better than gold or lead. Copper and ſilver will alſo ſuffer themſelves to be beaten into plates thin and yet ſtrong enough, and are not ſo ſubject to ruſt, as iron and ſteel. But in ſome caſes, eſpecially in want of metal in inſtruments, we may make uſe of well ſeaſoned wood, laid over with ſome china varniſh, or ſome other, that is very cloſe.

As to the form of the inſtrument, it conſiſts of three parts; the ball or globulous part; the ſtem or pipe; and that, which holds the coin.

THE ball or round part conſiſts of two thin concave plates of copper, or other metal, exactly ſodered together in the middle; and at the diſtanteſt parts from the commiſſure there ought to be left two oppoſite holes, one in each plate, for the two other parts of the inſtrument. This middle part, though for brevity ſake we name it the ball, ſhould not be exactly round, but, for the conveniency of ſwimming, of an almoſt eliptical or oval form, or rather ſomewhat inclining to that of a very deep double convex glaſs; or it may be of any other ſhape, that ſhall be found fittteſt to make the inſtrument keep its erect poſture ſteadily in the water. The bigneſs of it muſt be ſomewhat greater or leſs, as the plate is made thicker or thinner. But the general rule for its capacity is, that it ſhould contain as much air, as may ſerve to keep the whole inſtrument, when furniſhed, if need be, with its ballaſt and clogged with a guinea, from ſinking beneath the top of the ſtem, which ſtem is the next part to be taken notice of.

IF the inſtrument be to have its ballaſt (if I may ſo call it) within its cavity, it will be convenient, if not neceſſary, that it ſhould be hollow, like a pipe, exactly cloſed at the upper end; but where the ballaſt is to be placed without, the pipe ſhould be made ſolid, as of a piece of wire, or a little cylinder of ſome lighter matter, that will not ſoak in water: but, whether it be hollow or no, it ought to be made very ſlender, that the different depreſſions of the inſtrument in the water may be the more notable. And for the ſame reaſon it ought not to be too ſhort, eſpecially if it be to be applied to other uſes than the examining of guineas.

THE inſtrument, I moſt uſe merely for guineas, hath its ball about the bigneſs of a ſmall hen-egg, or rather leſs, and the pipe between four and five inches long, being ſodered on to the ball at the uppermoſt of the two holes abovementioned; at the undermoſt of which is inſerted and ſodered the undermoſt part of the inſtrument, which I call the ſcrew, or the ſtirrup, becauſe ſometimes it is made of a piece of wire, that a little beneath the bottom of the ball is bent round, ſo as to ſtand horizontally, that the guinea being laid on it, it may be ſupported by it, as the foot is by a ſtirrup; and in this way a piece of coin is the moſt readily put on and taken off. But the more ſecure way is, inſtead of the bent wire, to employ a very ſhort piece of braſs with a broad ſlit in it, capable of receiving the edge of the guinea, which with one turn or two of a ſmall and ſlight lateral ſcrew may be kept faſt in it, and readily, the operation being ended, taken out again.

IF you deſire to examine not only guineas, but greater gold coins and metalline mixtures, it would be convenient, that the undermoſt ſtem and the ſcrew be made by itſelf, that it may be at pleaſure thruſt upon the ſtem and taken off again. For by this means, if the ball of the inſtrument be made large enough, you may have room to put on, as occaſion ſhall require, one, two, or three flat and round pieces of copper, lead, &c. with each of them a hole in the middle, fitted to the size of the stem, so that they may be put on as near the lower part of the ball, as you think fit, and then the screw may be thrust on after them, not only to take hold of the coin or metalline mixture to be examined, but to support the plate, if need be; and by a variety of such plates, which which may be taken off and put on at pleasure, the same instrument if (as I was saying) the ball be competently large, may be adjusted sometimes to a guinea, sometimes to a coin of gold or silver, or to a metalline mixture twice or thrice as heavy as a guinea in the air.

THE instrument being made of a convenient bigness and shape; to adjust it for the use of examining guineas, you must by the help of the stirrup or screw, hang, at the bottom of it, a piece of that coin, which you know to be genuine, and having carefully stopped the orifice of the stem, if it be a pipe, (that no water may get in at it,) immerse the instrument leisurely and perpendicularly into a vessel full of clean water, until it be depressed almost to the top of the stem, and then letting it alone, if being settled it continue in the same station and posture, your work is done, but if it sink quite under water, you must lighten it either with a file, or by scraping or grating off a little of the ballast-plate above mentioned; or, if you have put any weight into the cavity to poise it, by taking out some of that, until you have made it light enough: but if, when you leave the instrument to itself, it emerge, you must then add a little weight to it, either by putting into the stem, if it be hollow, some dust-shot, filings of lead, or some other minute and heavy body, or else by putting on the short stem abovementioned, that comes out beneath the ball, a flat, round, and perforated piece of lead, of weight, sufficient to enable the guinea to depress the weight, as low as it is desired: which being done, a mark is to be made just at the place, where the surface of the water touches the stem, and then taking out your instrument, substitute in the place of your guinea a little round plate of brass, of the same weight, or a grain or two heavier, in the air; and putting the instrument into the water as before, suffer it to settle, and make another mark at the intersection of the stem and the horizontal surface of the water.

ABOUT this way of adjusting our instrument, the following particulars may be noted:

IF a screw be employed to sustain the guinea, the coin ought to be so placed, that one half, according to the estimate of the eye, may be on the right hand, and the other on the left hand of the screw; that the instrument being depressed may continue in an erected posture, and not swerve to an inclined.

THOUGH, when the stem is hollow, and the instrument too light, it may seem the better to add quick-silver than any other weight, because of its fluidness, and great specifick gravity; yet, unless the instrument be of glass, it is not safe to employ mercury, because it is apt to dissolve the soder.

IF the marks be made of a white colour, they will be so much the more conspicuous: and these marks may be made, if the pipe be hollow, by making round impressions with a small file, and encompassing them with little circles of fine wire of silver, gold, &c. And, if the stem be solid, it may then be either quite perforated at the requisite places, and have the holes filled with chawed mastic, or some such white substance, that dissolves not in water, or else have little holes, that pierce not quite through, stuck into it; and these may likewise be filled with the same substance, which, if further distinction be desired, may have some parts of it differingly coloured, before they be employed.

IT will be requisite to employ in adjusting the instrument one of the heaviest guineas you can get, to depress the instrument as low as it is like to be by any piece of that coin, lest otherwise meeting with one considerably heavier than that you made use of, the instrument may be thereby made to sink to the very bottom of the water.

THE reason why it is above prescribed, that the instrument be immersed almost, not quite, to the apex of the stem, is, because I have found, that guineas are not all precisely of the same weight, nor all waters neither; and therefore it is safest, to leave a small part of the stem, as an eighth, or, in longer instruments, a quarter of an inch, extant above the water, that we may secure the instrument from being by a heavier guinea made quite to sink.

I foresee, it may be hence objected, that these contingencies may make our instrument useless: to which it is not difficult to answer, that, though some guineas weigh a grain or two more than others, it is not that will frustrate the use of our instrument, and less will the difference of our waters do it, since (as I have observed in another paper, where I mention some trials of this kind) having examined and compared together the specifick gravities of (common) pump-water, Thames-water, and rain-water, I found the difference far more inconsiderable, than one would have thought, and consequently unable to keep hydrostatical trials of metals from being accurate enough for practice, and more exact than those troublesome and chargeable ones, that are commonly relied on.

THESE answers to the recited objections will be made good by this, that it is not a doubtful or inconsiderable difference, that appears upon the differing depressions of the instrument, that are made by a true guinea, and by a piece of brass or of copper, of the same weight with it in the air. For, in the instrument lately described, though smaller than most, that I have employed, the distance betwixt the mark, to which the gold, and that, to which the other metal, though copper, depressed it, was, by measure, about an inch and three quarters; so, that it is not every small variation of circumstances, that can make it doubtful to him, that employs our instrument, whether a guinea be true or counterfeit.

BUT philosophical candor forbids me to conceal, that there may, (though it is like there very ſeldom will,) happen a caſe, wherein, though the principle, our inſtrument is framed on, will hold good, yet the practical application may be unſecure. For, if a falſifier of money have the ſkill, by waſhing or otherwiſe, to take off much of the quantity or ſubſtance of the guinea, without altering or impairing either the figure or the ſtamp, the piece of coin will not be able to depreſs our inſtrument to the uſual mark, and may thereby make it be judged counterfeit, when it is indeed but too light.

BUT on this occaſion it is to be conſidered, that neither the touch-ſtone, nor aqua-fortis, nor antimony, nor the cupel, can ſhew us, whether a piece of coin propoſed have its juſt weight, but only, whether the metal be true gold; and therefore our inſtrument need not pretend to do more, than diſcover the genuineſs of the metal: but whether the coin have the juſt weight the law requires, is to be judged by the balance; as each ſingle piece is wont to be in moſt of the gold coins of *Europe*, and is in *England*, in reference to angels and twenty-ſhilling pieces, and all the other coins of broad gold, as they are now called. And yet it may be further conſidered, that our inſtrument does more than it need pretend to: for, without a pair of ſcales, it preſently ſhews, that the propoſed guinea, if it be not counterfeit, is otherwiſe abuſed; and though it does not clearly determine, whether that likewiſe proceed from the want of ſpecifick gravity in the metal, or from the coins having been waſhed, or otherwiſe fraudulently leſſened; yet it probably reſolves the doubt, becauſe, if the want of weight appear by the inſtrument to be very great, as it uſually does, where the piece has been robbed of ſome of its ſubſtance, (eſpecially if it be ſo much, as is reported, of ſome guineas, that of late are ſaid to have been found wanting to the value of near four ſhillings;) it is a ſtrong preſumption, that it is rather waſhed, &c. than counterfeited. For men will ſcarce venture their lives to ſteal but three or four grains from a true guinea, and much leſs from a falſe one. And they, that counterfeit, are not wont to be ſo ſparing as to make their coins too light. However, our inſtrument will in theſe caſes be ſure to prompt him, that uſes it, to employ the balance, which will preſently aſſiſt him to reſolve his doubt. For, if the ſuſpected coin have in the air its due weight, it will argue, that the greateſt lightneſs of it in the water proceeds from the metal's not being true gold, or, at leaſt, of its not being of the requiſite fineneſs; and, if it want much of its due weight in the air, it is very probable, for the reaſon above-intimated, that it is waſhed, &c. rather, than of another metal than gold; and however may be lawfully refuſed to be taken in payments, and perhaps afford a juſt ground of queſtioning him, that utters it. And if one would, for curioſity, be further ſatisfied, whether the metal be gold or no, one may add to the coin (as will be hereafter taught) as much ſterling-gold, as will make it, in the air, of the weight of a guinea, and then examining it by the weight in the water, he will preſently diſcover, whether it be gold or not.

THERE comes into my thoughts another poſſible way of counterfeiting guineas; but becauſe it is very likely, that coiners will not light upon it, and it cannot be practiſed on any of the guineas already coined, the fear of teaching bad men a ſkill, that probably they will not otherwiſe acquire, makes me forbear to mention it, though the fraud may be quickly diſcovered, ſometimes by the bare eye, and always by our inſtruments and the balance; whereof publick advertiſements may be given, if there ſhall appear need of it.

AND now I have this to add about the conſtruction of this inſtrument, that perhaps it would not be very difficult to propoſe a much more accurate and elaborate contrivance, if it were thought fit to propound any, that would require an extraordinary ſkill in the artificer to make it, and ſome conſiderable ſkill or dexterity in the perſon, that is to uſe it: but the ſlight conſtruction, hitherto diſcribed, ſeemed to ſuit better with my principal aim, which was, to propoſe at preſent an inſtrument, as ſimple, cheap, and eaſy, to be employed and kept in order, as I could well examine guineas with; little doubting, but that the principle, upon which this is framed, being well underſtood and conſidered, will, if it be found uſeful, be further improved by new applications and more artificial contrivances.

Explications of the figures.

IN fig. 1. A B, the ſtem or pipe.

C E, the two parts of the ball ſodered together.

B C D E, the ball itſelf.

F, the ſcrew.

G, the ſtirrup, ſomewhat repreſented out of its place.

H, the mark to which a copper-plate, of equal weight in the air with the guinea, depreſſes the inſtrument.

I, the mark to which a true guinea ſinks it.

FIG. 2. is the ſcrew by itſelf, to be put upon, or taken from the (ſhort) undermoſt ſtem of the inſtrument.

FIG. 3. the perforated plates of lead or other metal, to be put on as ballaſt upon the undermoſt ſtem.

FIG. 4. the undermoſt ſtem, with a perforated ballaſt-plate put upon it.

FIG. 5. the ſtirrup, that may be employed inſtead of a ſcrew.

FIG. 6. A B C, the glaſs-inſtrument.

D D D, the coin hanging at the bottom of it, and ſupported by four horſe-hairs, or ſlender ſtrings of ſilk.

FIG. 7. the undermoſt ſtem of the glaſs-inſtrument, to which, being ſtreight and ſolid, a ſcrew is faſtened on with horſe-hair or otherwiſe.

FIG. 8. A B C D, the ſmall glaſs-inſtrument for eſtimating the ſpecifique gravity of liquors, (of which an account may be expected in our next.)

E E, the quick-ſilver and water, that is employed as ballaſt to ſink it in an erected poſture.

SECT.

SECTION III.

Representing the uses of this instrument, as relating to metals.

THERE is in the nature of the thing ſuch a connection between the fabrick and uſe of our inſtrument, that I could not well deſcribe it without plainly intimating the principal uſes of it. Wherefore I ſhall here but ſummarily repeat thoſe, that are delivered already, and make a more explicit mention of thoſe few, that have been either omitted, or but lightly touched.

USE I.

THE firſt uſe, and that, which was mainly intended, is, eaſily and cheaply to diſcriminate true guineas from counterfeit, without defacing, or any ways injuring the coin. But of this uſe I have ſpoken largely enough already, and therefore ſhall advance to the next.

USE II.

ANY other kind of gold-coin, that is near about the weight of a guinea, may be examined by our inſtrument after the manner above delivered; but more eaſily, if it want of the weight of a guinea, than if it exceed it. For in caſe it be heavier, as is a twenty-ſhilling piece of broad gold, the ballaſt, whether internal or external, of the inſtrument muſt be taken off, that ſo heavy a coin may not quite ſink it; whereas, if the coin propoſed be lighter than a guinea, one may add as much gold (of the ſame alloy) beaten into thin plates, as, with the coin propoſed, will make up in the air the weight of a guinea. For then this aggregate, being examined, as if it were a guinea, will diſcover in the water, whether the coin be right or counterfeit. I ſhall add, that if the piece, to be examined, be not much heavier than a guinea, it may be convenient to paſs a very ſmall perforated plate of copper or lead over the upper ſtem (or pipe,) ſo as to make it reſt upon the ball before the inſtrument is adjuſted. For, by this means, nothing need be altered beneath the ball; and ſuch pieces of metal (of which ſeveral differingly heavy may be eaſily provided) being thin and light, will not (as trial has ſhewn) make the inſtrument top heavy, though one of them be placed above the center of gravity, and may be very readily taken off, and (if need be) ſcraped or filed to lighten the inſtrument, when an extraordinarily heavy guinea, or a coin ſomewhat more weighty than a guinea, is to be examined.

BUT to return to what I was ſaying about adding a weight of gold to a piece of propoſed coin; in order to this uſe it will be neceſſary, that the ſlit or aperture at the bottom of the inſtrument, which is to be ſhut and opened by the lateral ſcrew, be made (as it eaſily may without inconvenience) wide enough to receive double the thickneſs of a guinea, that ſo different coins, as *Engliſh*, *French*, *Spaniſh*, &c. and the grain-weights, neceſſary to bring them to the weight required (in the air,) may be ſecurely faſtened to the inſtrument by the ſcrew.

IF the ball be large, and the pipe well proportioned to it, coins, that do not much exceed the weight of a guinea, may be examined without much altering the weight of our inſtrument, provided it be at firſt adjuſted ſo, as that a guinea will not depreſs it ſo far as not to leave a conſiderable part of the pipe above water, that the coin heavier than a guinea may not be able to draw it quite under water.

ACCORDING to the method above deſcribed, may half guineas be examined. For, if the inſtrument be good, it will ſhew a manifeſt difference, if inſtead of an entire guinea, you faſten in the ſcrew a half guinea, that you know to be true, and that, which is ſuſpected to be counterfeit; adding a grain-weight or two of gold, in caſe the propoſed coin needs it; I ſay, a grain-weight of gold, becauſe, if it be of braſs, of which the grain-weights, commonly uſed, are made, it will looſe in water more than it ſhould of the weight it had in the air; and therefore it will be uſeful to ſuch, as intend to try ſeveral ſorts of Engliſh coins, as angels, two and twenty ſhilling pieces, double guineas, &c. to have by them a numerous ſet of grains, (about whoſe ſhape, by the way, one need not be curious, that not being material) made of a thin plate of ſterling gold.

USE III.

IF the inſtrument be ſkilfully fitted for ſuch a purpoſe, it may be made to ſerve to examine ſome ſorts of white money leſs heavy than half crowns. And, becauſe it may be uſeful to know in general, what coins may, and what may not, be examined by this or that particular inſtrument propoſed, I ſhall here add a general way, that is not difficult for finding this out; namely, firſt by weighing the piece of gold or ſilver in the air, and afterwards in the water, and ſubſtracting the latter from the former, to obtain the difference of the two weights: and next, by weighing alſo in the air and in water a piece of copper, or braſs, if this be the likelieſt to be employed in counterfeiting the coin, and obſerving likewiſe the difference between thoſe weights. For, the leſſer of theſe differences being ſubſtracted from the greater, the remains will ſhew, how much the true piece of coin will outweigh the other in the water; and conſequently if ſo many grains, as this reſidue amounts to, being added to the weight of the lighter metal, do make a ſufficiently manifeſt depreſſion of it below the mark it would ſtay at without that addition, one may probable conclude, that the difference between a true and counterfeit piece of coin propoſed will be diſcoverable by the inſtrument.

THE cheapneſs of theſe ſlight inſtruments being conſidered, it may be expedient for goldſmiths and others, that have frequent occaſions to examine various ſorts of coin, to have a ſeveral inſtrument adjuſted for each of them, to ſave themſelves ſome pain and trouble. But if the ball be made large, and fitted with a ſtem ſlender and long enough, one may quickly by changing the ballaſt-plates, as occaſion requires, fit the ſame inſtrument to examine coins of differing metals, and of very differing weights. For one of theſe, made of copper, ſerves me to examine both guineas and crown-pieces of ſilver, and half crowns too; and it may be eaſily made to ſerve alſo for divers foreign coins.

USE IV.

It is a great complaint of pewterers, that the tin they buy of the miners or merchants, is often adulterated with lead, as they find to their prejudice, when they have made veſſels of it. And many others, that are buyers, complain much more of divers pewterers for putting too much lead into their pewter, becauſe lead is by many times cheaper than tin. On theſe accounts, I ſhall add, to the other uſe of our inſtrument, ſomething, that relates to tin and pewter. Though I muſt take notice, that ſome tin may perhaps be found a little heavier in ſpecie than ordinary, although no fraud intervene; becauſe I have obſerved ſome tin (as I elſewhere relate) to contain ſome, though but a very little, proportion of gold or ſilver. But this being no uſual caſe, I ſhall proceed to ſay, that the pewterer may judge, whether the miner or merchant have deceived him; if, taking a piece of tin, that he knows to be pure, and is of a convenient weight, he obſerves, how much it depreſſes the pipe, and then makes the like obſervation with an equal piece of the tin ſuſpected to have lead or ſome other metal in it. For if this depreſſes the inſtrument much lower than the other, it will juſtify the ſuſpicion; ſince as gold, being the heavieſt of metals, cannot be allayed by any other, that will not depreſs our inſtrument leſs than gold can do; ſo tin, being the lighteſt of metals, cannot be mixed with any other, that will not ſink it lower than unmixed tin, (ſtill ſuppoſing the weights to be the ſame in the air.)

And as for the buyers of pewter, it will be eaſy for them (if they think it worth while) to find by our inſtrument, if there be too much, or but enough of lead mixed with the tin in an aſſigned portion of pewter of convenient weight to be examined by it. For, having once obſerved, how much the inſtrument is depreſſed by a piece of two, three, or four drams, or even an ounce weight of pewter, which is known to be good, and to contain ſuch a proportion of lead in reference to the tin, if you load the inſtrument with an equally heavy piece of any other maſs of pewter propounded, if the inſtrument ſink deeper, it will be a ſign, that the former proportion of lead may be very probably argued to exceed in the mixture; I ſay, probably, becauſe perhaps it is poſſible to embaſe pewter by mixing not only lead, but other mineral ſubſtances, whoſe ſpecifick gravity is not well known: but yet I ſay very probably, becauſe the addition of too much lead is the moſt gainful way of adulterating pewter. And the other things, that ſome employ, as regulus of antimony, tin-glaſs, copper, and ſpeltar, are ſeldom uſed in great quantities; and if I thought it worth the while, I could facilitate the diſcovery even of theſe by adding, what I have obſerved of their differing ſpecifick gravities, and ſome other things, that I think fitter to be here omitted than to have time and words ſpent upon them.

USE V.

The laſt uſe, I ſhall now mention of our inſtrument, in reference to metals, is, that it may aſſiſt us to eſtimate the quality of metalline mixtures, whether in coins or other maſſes, and to gueſs at the proportion of the ingredients, that compoſe them. For, ſince we have formerly ſeen, that the ſame inſtrument, employed to examine guineas, ſerved alſo for crown-pieces of ſilver, that wanted of an ounce leſs than a twentieth part of that weight, it will be eaſily granted, that the ſame inſtrument, and more eaſily, that a larger one, may be ſo fitted, as to help goldſmiths, chemiſts, and others, that are not acquainted with hydroſtaticks, to make ſuch an eſtimate, as will not much deceive them, of the fineneſs of gold and its differing allays with ſilver, or ſome its other determinate metal.

In order to this, the inſtrument may be be fitted to ſink to the tip of the pipe, with ſome determinate weight of the fineſt gold, as of 24 carats, as they call that, which is moſt pure and fine. But it will be convenient, that this metal in the air be juſt an ounce, or half an ounce, or ſome ſuch determinate weight, that is commodiouſly diviſible into many aliquot parts. Then you may make a mixture, that contains a known proportion of the metal wherewith you allay the gold; as if it hold 19 or 15 parts of gold, and one of ſilver; and, letting the inſtrument ſettle in the water, mark the place, where the ſurface of the water cuts the ſtem or pipe. And then putting in another mixture, wherein the ſilver has a new and greater proportion to the gold; as if the former be an eighteenth or a fourteenth part of the latter, you may obſerve, how much leſs than before this depreſſes the inſtrument, and ſo you may proceed with as many mixtures or degrees of allays, as you think fit, or can be diſtinguiſhed conveniently on the ſtem; being always careful, that, whatever be the proportion of the two ingredients, the weight of the maſs in the air be juſt the ſame with that of the pure gold, which we have lately ſuppoſed to be one ounce, or half an ounce.

By the ſame method may be examined the differing alloys of pure ſilver upon the admixture of ſuch and ſuch determinate proportions of copper, or any other metal, lighter in ſpecie than ſilver; and by the ſame way, with a ſlight variation, it will not be difficult to eſtimate, how much divers coins, whether of ſilver or gold, are more or leſs embaſed by by the known ignobler metal, that is mixed in the piece propofed.

And though this way of determining the alloys of metals, be not fo exact, as is poffible to be propofed by the help of hydroftaticks and calculation; yet it may be very ufeful to chemifts, goldfmiths, refiners, and others, that are unacquainted with hydroftatical matters, to make without trouble or fupputation eftimates, that will not much deceive them, and perhaps will come nearer the truth, not only than the eftimates wont to be made by the touchftone, but perhaps too than fome of thofe, that divers make with trouble, and inconvenience, and charge. And indeed I was chiefly invited to communicate this trifle, and fpend fo many words about it, by the requeft of fome ingenious difciples of *Vulcan*, who thought they perceived, that by this way they could oftentimes make better eftimates of the fuccefs of their graduating, and fome other operations upon metals, than otherwife they fhould be able; this way greatly accommodating them by this particular advantage, that they may from time to time try the degrees of purity, and fome other confiderable alterations of their mixtures, without at all deftroying or injuring them, though they have not yet attained the pitch they aim at and expect; whereas, if they happen to be too forward, as often they are, in examining the productions of their labours by the cupel or fevere cementations, what they would try may be deftroyed or fpoiled in its way to a perfection, which otherwife, in their opinion, it might in due time be brought to.

Perhaps it may not be amifs, on this occafion, to add, as an improvement of this fifth ufe of our inftrument, that it may be employed to examine other mixtures befides allayed coins, and that if the inftrument be adjufted to an ounce, for inftance, of pure copper, it may help men to make an eftimate of the alloy of tin, or the quantity of it, that is oftentimes added to copper, to make differing forts of bell-metal, and of thofe metalline fpecula, whether plain or concave, that are called fteel glaffes, as alfo of foders confifting of certain proportions of filver and brafs, or copper; in all which, and divers others, the difcovery of the proportion of the ingredients may, on fome occafions, be ufeful to tradefmen, as well as defirable by virtuofi. And though I have obferved, that by mixture, tin and copper acquire a fpecifick gravity fomewhat differing from what their ingredients promife; yet, fince the inftrument is to be fitted for fuch eftimates, not by calculation, but by trials, the eftimates may be made near enough to the truth.

NEW EXPERIMENTS

ABOUT THE

Weakened SPRING, and fome unobferved Effects of the AIR.

First publifhed in the Philosophical Transactions, N°. CXX. p. 467, for *December* 27, 1675.

As for the not yet communicated trials, that I made in profecuting my defign of difcovering or obferving fome latent qualities of the air, I will not deny you fome of them, as imperfect as yet they are, but will venture to fend them you, as my notes or my memory fuggefts them to me; not only, becaufe without being compleated they may be fit enough to countenance fufpicions (for you know, that I do not call them fo much as opinions,) but for a weightier inducement, to be told you at the end of this paper.

The two chief things aimed at in the imperfect attempts I now fend you, were to difcover; firft, whether, as fome corrofions of bodies do in clofe veffels increafe the fpring of the air (as I long fince noted them to do,) fo fome other corrofions may not, by a contrary, or fome other way, weaken the fpring of the air; and next, whether in fome folutions and precipitations the air on the account of fome unobferved quality may not be found to produce fome phænomena not yet taken notice of.

In order to each of thefe inquiries, I fhall mention a few trials, though without curioufly forting them, becaufe fometimes in the fame experiment both thofe attempts were jointly profecuted.

You may remember, that in fome of my formerly publifhed trials I acquainted you with an odd phænomenon of the change of colour producible in folutions of copper by the operation of the air: I ſhall now add, what further phænomena my memory or notes ſupply me with, about the ſubject of that and the like experiments.

EXPERIMENT I.

WE took filings of crude copper, and put them into a chryſtalline glaſs of a conical ſhape, into which we poured ſome ſtrong ſpirit of ſalt, (that was fitted for our peculiar purpoſe) to the height of about a finger's breadth above the filings; and then cloſing the veſſel with a glaſs-ſtopple exquiſitely fitted to it, we ſuffered it to continue unmoved in a window for ſome days, untill the liquor had both obtained a high and darkiſh brown colour by the ſolution of ſome of the copper, and loſt that colour again, growing clear like common water, (which is itſelf a ſomewhat odd phænomenon;) and then taking out the ſtopple, (without ſhaking the liquor) and thereby giving acceſs to the outward air, we perceived, (as we had conjectured) that the upper ſurface of the liquor did in a few minutes re-acquire a darkiſh brown colour, which penetrating deeper and deeper, at the end of about a quarter of an hour the whole body of the liquor appeared to be likewiſe tinged. The conical glaſs being again well ſtopped, the menſtruum did again in very few days let fall, or otherwiſe looſe its tincture; which, the ſtopple being taken out, it re-gained as before. Nor were theſe two the only trials I made with the like ſucceſs for the main; but afterwards being deſirous by a further trial to reſolve a doubt I had, I kept the glaſs yet longer in the ſame place with the ſame filings and menſtruum in it for (if I miſ-remember not) a month or two together; but obſerved not, that the liquor would any more grow clear.

EXPERIMENT II.

HAVING taken ſuch a glaſs, as is mentioned in the firſt experiment, wherein the liquor was grown clearer than is uſual, and had probably been ſo a good while before (for the veſſel, having been hid by others, which ſtood before it, had been for ſome weeks forgotten;) we took out the ſtopple, and left it open for about half an hour, but did not perceive the liquor to have acquired any colour ſo much as at the top. Whether this proceeded from the long debarring of commerce with the freſh air, or from ſome other cauſe, being unable to wait the event as long as would perhaps be requiſite, I thought fit to try, whether the air had already had ſome operation upon the liquor, though it did not yet appear; and accordingly putting in the ſtopple, I left the veſſel cloſed for two or three hours, and at my return to viſit it, I perceived, that it had acquired a faint colour tending to a green; wherefore taking out the ſtopple again, I opened its commerce with the outward air, leaving the glaſs unſtopped for 20 or 24 hours, but found, that in all that time it had not regained its wonted dark colour, but was only arrived at a green, deep enough, but not true, nor very tranſparent.

THIS obſervation being made in the ſame veſſel, that had been formerly employed, ſuggeſted to us an enquiry, whether the advanced time of the year, which was the middle of *October*, might not have an intereſt in the ſlow and imperfect ſucceſs of this trial.

EXPERIMENT III.

SOME ſtrong ſpirit of ſalt having been kept upon filings of copper, until the ſolution was come to be of a dark brown colour, about three ſpoonfuls of it by gueſs was put into a receiver, that might hold eight or ten times as much: being kept in vacuo (if the time be rightly remembered) about half a year, it retained its colour, but the veſſel being opened and the external air permitted a free acceſs to it, the ſolution in about an hour was turned into a fine tranſparent green, though no precipitation of any muddy ſubſtance appeared by any ſediment to be made.

EXPERIMENT IV.

IN one of that ſort of conical glaſſes, that has been already more than once deſcribed, we had put upon ſome filings of copper, a convenient quantity of our ſpirit of ſalt; and though we obſerved, that for a great while it would not part with its deep and ſomewhat muddy tinctrue; yet we left it in the window for many weeks longer, and at length, towards the latter end of *December*, we found it to have loſt its tincture, ſo much, that the liquor appeared like common water. Upon which obſervation, though the time of the year were unpromiſing, I thought fit to try, whether the air in that ſeaſon would not have ſome, though perhaps but a ſlow operation on the ſaline ſpirit, and accordingly taking out the glaſs-ſtopple to give free acceſs to the outward air, we obſerved, that in ſome hours its operation on the liquor was ſcarce ſenſible, but within about 24 hours the menſtruum had acquired not juſt its former colour, but a ſomewhat faint and moderately tranſparent green: ſo that this tincted menſtruum, as it had been very ſlow in loſing its colour, ſo it did but ſlowly and imperfectly re-acquire it.

" I HAVE not in the foregoing experiments " made mention of any phænomena of " them relating to the Spring of the included " Air, becauſe I do not remember, that they " were ſuch, as invited me to draw any poſitive " concluſion from them, and my ſilence on " this occaſion may be the more allowable, " becauſe the way of further making ſuch ob- " ſervations may be ſufficiently deduced from " the enſuing trials; in reciting of which I " alter very little, and in ſome of them not at " all, the expreſſions I find them regiſtered " in, though more than once the phænomena, " that relate to the air's elaſtic power, be " mingled in the ſame experiment with the " mention of its operations upon colours.

" THE ſpring of the air and its variations, " by the ways now known to many of the cu- " rious, being things, that manifeſtly appear " to have a notable intereſt in divers phæno-

" mena of nature, whose causes, if not them-" selves also, were unknown to former philo-" sophers; it seemed an attempt, though not " very promising, yet worth the making, to " try, whether the spring of the air, which may " divers ways, as by heat, compression, &c. " be increased, may not by some other way " than cold and dilatation be weakened: and " having often found menstrums, that corrode " metals, so as to produce bubbles to invi-" gorate the strength of the spring of the air " included in the vessels, wherein the solution " was made, I thought fit to try, whether in " some metalline dissolutions, wherein I had " observed, that few or no visible bubbles at " all were produced, the spring of the neigh-" bouring and included air would not be de-" bilitated; and in order to this were made " the following trials."

EXPERIMENT V.

[We took some filings of copper, and putting them together with a mercurial gage * in a conical glass fitted with an exactly ground-stopple of the same matter, (which was chrystalline) we poured on the filings, as much rectified spirit of fermented urine made *per se*, as sufficed to swim an inch or better above them; then carefully stopping the glass, coming to look on it many hours after, we perceived, that the mercury in the sealed leg was considerably depressed, and gently drawing out the stopple to let in the outward air, we perceived that access to have a manifest effect upon the mercury.]

But this will be better understood by the more circumstantial experiment, that ensues.

EXPERIMENT VI.

[We took a crystal glass of an almost conical shape, and capable of containing between five and six ounces of water, and furnished with a stopple of the same matter, that by grinding was exactly fitted to it. Into this we put a convenient quantity of clean filings of good copper, on which we poured as much strong spirit of (fermented, or rather, putrified urine, as served to swim about an inch above the copper, and having let down a mercurial gage, so that it leaned upon the bottom and side of the glass, we closed it very well with a stopple, and set it in a quiet and well enlightened place, having taken good notice at what mark the quicksilver rested in the open leg of the gage. This done, we let the menstrum alone to work upon the filings; which it did, as we foresaw, somewhat slowly and very calmly, without producing any noise or sensible bubbles, acquiring by degrees a very pleasant blue colour, and the glass being kept quiet in the same place for two or three days longer, the liquor, as I conjectured would happen, began to lose of the intensness of its colour, which by degrees grew fainter and fainter, until at the end of three or four days the liquor was grown very pale, and left me little doubt but that, if I would have staid some days longer, it would have lost the remaining eye of blue, and have looked almost like common water. But being unwilling to tarry so long, I took out the stopple, that the air without the glass might have access to that within; and leaving the vial in the same place and posture, my expectation was somewhat answered by finding, that within four or five minutes, if not less, the upper part of the liquor, that was contiguous to the air, had acquired a fine blue colour, which descending deeper and deeper, before the end of the tenth minute had diffused itself, but somewhat weakened, through the liquor, whose colour was suffered to deepen for a while longer; so that in less than a quarter of an hour from the first unstopping of the vial, the liquor was grown to be throughout of a rich ceruleous colour, which grew almost too opacous within a few minutes longer: when carefully closing the vial again with the same stopple as before, we set it aside in the same place, where, the included air being denied all commerce with the external, the liquor began again, within two or three days, to lose of its colour, and, to be short, afforded me the opportunity of making a second experiment, much like the former. And the like success I had, for the main, in a trial or two made in another glass with another portion of the same spirit of urine, put upon the filings of copper; so that the experiment was, in all, made divers times, as well when I was not, as when I was alone: and particularly, once to be sure, that the diurnal air as such had not any great interest in the phænomenon, I made the trial successfully about nine a clock at night, in the presence of so well known a witness, as the learned secretary of the Royal-Society.

See the reference in the foregoing experiment.

One circumstance I forgot to take notice of, which was, that in most of these experiments I forbore to shake the glass, lest it should be suspected, that the agitation of the liquor might have raised some little fine powder, that might have been supposed to have been precipitated out of the tincture, and, being thus mingled with the liquor again, restore it to its former colour; but in truth, I did not perceive any such powder to be precipitated. And though, to obviate the objection, I forbore to shake the vial; yet I justly supposed, that if, by the agitation of the liquor, more parts of it should be quickly exposed to the action of the air, the coloration would be hastened, which upon trial appeared to be true.].

EXPERIMENT VII.

[Experience have made me think it likely, that strong spirit of sal armoniac, made without quick-lime, would operate more nimbly and more powerfully on that metal than our spirit of urine had done; we took such a conical glass, as has been lately described, and covering the bottom of it with a convenient quantity of filings of good copper, we poured on them as much strong spirit of sal armoniac, as served to swim about a finger's breadth above them; and, having let down such a mercurial gage, as is formerly mentioned, so that it leaned upon the bottom and side of the glass, we closed it very well with a stopple, and

* About such glasses, see Experiment XVII. in the continuation of our New Physico Mechanical Experiments.

and ſet it in a quiet and well enlightened place, having taken good notice at what mark the quick-ſilver reſted in the open leg of the gage: this done, we let the menſtruum alone to work upon the filings, which it did, as we foreſaw, ſomewhat ſlowly and very calmly, without producing any noiſe or ſenſible bubbles, acquiring by degrees a very pleaſant blue colour, and afforded us alſo the phænomenon we chiefly looked after; which was, that repairing from time to time to the window to ſee what paſſed, we perceived, that for two or three days together the mercury in the ſealed leg of the gage did, though very ſlowly, deſcend, until it appeared to be near a quarter of an inch lower than at firſt; and probably the depreſſion might have been greater, if ſome indiſcreet body or other had not, by tampering with the glaſs, diſturbed the experiment; whoſe event yet ſeemed ſufficiently to argue, that the ſpring of the air, contained in the cavity of the glaſs, and communicating with that in the open leg of the gage or ſyphon, was weakened in compariſon of that in the cloſed leg, which by the hermetic ſeal on one ſide, and the quickſilver on the other ſide, was kept from ſuch communication.

AND becauſe I thought it might be ſuſpected, that the phænomenon might be referrable to ſome inequality in the preſſure of the air, occaſioned by the greater operation of the heat of the day on the more impriſoned air of the gage, than on that more immediately included in the cavity of the vial; I was careful to obſerve, whether the depreſſion did not continue at differing times of the day, and found it to do ſo, as well at night, as at noon, though at this laſt named time, the ſun ſhined hot upon the place and veſſels too.

THIS experiment was made, in all, four or five times, though not always with equal, yet ſtill with ſome ſucceſs, the mercury in the ſealed leg of the gage being ſometimes more and ſometimes leſs, but always manifeſtly depreſſed; which phænomenon was confirmed by the obſervation we more than once made of the ſudden return of the quickſilver to its former ſtation, upon the unſtopping of the glaſs, to give free admiſſion to the outward air.

EXPERIMENT VIII.

CONSIDERING, whilſt I was about theſe trials, that ſpirit of vinegar, though in working upon coral and ſome other bodies, it not only produces ſtore of bubbles, but alſo, as I have elſewhere delivered*, a ſomewhat odd kind of elaſtical ſubſtance, yet being put upon minium it was wont, in my obſervation, to work calmly and without producing froth; I thought fit to make trial, whether this calm and ſilent ſolution of minium would be accompanied with a permanent change of the air's ſpring: the event I find thus ſet down:

[A pretty quantity of ſpirit of vinegar being put upon minium in a conical glaſs, furniſhed with a glaſs ſtopple and a mercurial gage, continued divers days without any ſenſible depreſſion of the mercury in either leg, nor did any change appear in the gage, upon the removal of the ſtopple, though it was evident by the great ſweetneſs acquired, that it had made a ſolution of a great portion of the minium.] But to return to our trials upon copper.

EXPERIMENT IX.

[WE took ſome filings of copper, and in a vial capable of holding ſome two or three ounces of water, we poured on them ſtrong ſpirit of ſal armoniac, made without quicklime, till the liquor reached near an inch above them. This was done about the twentieth of *Auguſt*, on the *Friday* before noon, and the following *Monday*, preſently after dinner, it had acquired a deep blue tincture, and loſt again ſo much of it, that it was pale, almoſt like common water: then, to ſatisfy a virtuoſo, I unſtopped the vial, deſiring him to place his eye level with the ſurface of the liquor, which, in a minute of an hour, or leſs, appeared, to his ſurpriſe and wonder, to have acquired a deep blue tincture, that reached downwards to the thickneſs of the back of a knife, the whole liquor becoming of the like colour in four or five minutes more, and the glaſs being preſently ſtopped again, and left where it was before, appeared not at the end of nine days, to have loſt its tincture; though now and then, within that time, it ſeemed manifeſtly paler than when the vial was ſtopped.]

" NONE of the former trials with ſpirit of " ſal armoniac having been made in an her- " metically ſealed glaſs, it will not be amiſs " both to diverſify and to confirm our experi- " ments, by ſetting down the ſucceſs of one " made in ſuch a veſſel."

EXPERIMENT X.

[WE took a round vial, holding about eight ounces of water, and having put into it filings of copper and a mercurial gage, we poured on the metal ſtrong ſpirit of ſal armoniac, till it reached to a good height in the vial, which then being hermetically ſealed up, was ſet by in a ſouth-window, where it

* To the better underſtanding of this, the enſuing trial may much conduce; and therefore is tranſcribed out of another paper, to which it properly belongs.

A mercurial gage having been put into a conical glaſs whoſe bottom was covered with beaten coral, ſome ſpirit of vinegar was poured in, and then the glaſs ſtopple, which was very well ground, cloſing the neck exactly, we obſerved, that upon the working of the menſtruum on the coral, ſtore of bubbles were for a good while produced, which ſucceſſively broke in the cavity of the veſſel, and their acceſſion ſo conſtipated the air, that they compreſſed the air impriſoned in the cloſed leg of the gage three marks or diviſions, which I gueſſed to amount to about the third part of the extent it had before: but ſome hours after the corroſion had ceaſed, the compreſſion made by this newly generated air grew manifeſtly fainter, and the impriſoned gage air drove down the mercury again, till it was depreſſed within one diviſion of its firſt ſtation; and thereabouts, or little lower, continued five or ſix days; ſo that in this operation there ſeemed to have been a double compreſſive power exerciſed; the one tranſient, by the briſk agitation of vapours or exhalations, and the other durable, from the aerial and ſpringy particles, either produced or extricated by the action of the ſpirit of vinegar upon the coral.

it quickly acquired a deep blue tincture: there it stood about twelve days, before that tincture, which decayed but slowly, did, little by little, grow so diluted, that the liquor was pale and almost like water: during this stay of the glass in the window, the mercury in the open leg appeared to be impelled up, and when after nine o' clock at night, (which time I chose to try, whether the nocturnal air, as nocturnal, would have any thing to do with the phænomenon,) the hermetic seal was broken off; immediately upon which there was produced a noise, and the mercury in the shorter and closed leg was briskly impelled up, by our guess, near three eights of an inch, and though the orifice, at which the air had access, was scarce wide enough to admit a middle-sized pea, yet, within a minute and half, the surface of the liquor being held between the eye and the candle, appeared to have acquired a very lovely and fair colour, which reached downwards a quarter of an inch; so that the vial seemed to contain two very differing liquors swimming on one another, and the coloration piercing deeper and deeper within five minutes in all, the whole liquor had attained a rich blue colour.]

"WITH this experiment I shall conclude "this paper: for though I made several other "trials, with the same design, that I made the "foregoing ones, as with spirit of nitre, and "minium, spirit of vinegar and copper; "yet a present want of time hinders me from "troubling you with them, which I the rather "forbear to do, because I fear, they would "prove less satisfactory than those I have set "down, which themselves must, to a less discerning eye than yours, appear very imperfect, notwithstanding that prolixity in reciting some of them, which I was obliged to "by my not yet knowing, in such odd attempts, what circumstances might safely be "omitted. But such as they are, I send them "you, who, by your diffused correspondency, "have great opportunity to get them made, "if you think them worth it, by curious persons in several countries, various manners, "and differing seasons of the year: and however the things I send you be but trifles, yet "their novelty may, perhaps, excite the industry of others, and give rise to further enquiries."

AN EXPERIMENTAL DISCOURSE OF QUICKSILVER growing hot with GOLD.

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The INTRODUCTION of the PUBLISHER.

THOUGH the following discourse was by the author of it made part of a short Examen of the supposed sympathy between gold and quicksilver, (which itself belongs to another treatise;) yet the worthiness of the subject, and the great curiosity, that is observed among many virtuosi, (not only chemists, but others,) about mercurial preparations and experiment, made me think I might do them an acceptable piece of service, if I could prevail with the author to sever them from the papers, whereto he had annexed them, (but to which they seemed not absolutely necessary) though upon the conditions he judged requisite to insist on.

AND since I venture to impart before the time these things unto the curious, I hope and desire, they will be so equitable, as to indemnify me to the author, and not fruitlessly endeavour to put a person, that has already given so many proofs of his propensity to gratify ingenious men, upon making unseasonable anſwers to any verbal or epiſtolary queſtions about things, wherein ſome conſiderations, that he thinks are not to be diſpenſed with by him, do as yet injoin him ſilence.

Now, to gratify the curious among ſtrangers, as well as thoſe of our own nation, the publiſher was not unwilling to give this diſcourſe in Latin, as the author hath been pleaſed to impart it in Engliſh.

Follows the DISCOURSE itſelf.

Of mercury growing hot with gold.

1. —BUT that what I have hitherto ſaid, may not be drawn to the diſparagement or diſcouragement of thoſe Spagyriſts, that poſſeſs or aſpire to the nobler arcana of gold and mercury, I muſt mind you to take notice, that what I have objected againſt the ſuppoſed ſympathy of gold and quickſilver, is ſpoken only of common mercury, that being it, whoſe ſympathy with gold is wont to be celebrated. And though perhaps, a good part of the things I have alledged will be found applicable even to true running mercuries; yet I would not be thought to deny, that there may be a quickſilver more ſubtile and penetrant than that which is common, and that thoſe chemiſts, that ground the ſympathy of gold and mercury upon the operations of a more philoſophical mercury, may likewiſe argue for it more ſpeciouſly, than vulgar mercury will enable them to do. And to let you ſee on this occaſion, that I am not unkind to the chemiſts, I will annex part of a paper, written to a friend to give him my opinion about mercury's incaleſcence with gold.

2. —AND now I ſhall abruptly begin this ſection with the conſideration of a problem much agitated among the curious, eſpecially thoſe, that pretend, whether truly or vainly, to have more than ordinary inſight into chemiſtry: among whom I find it hotly diſputed, whether or no there be any ſuch thing, as a mercury, that will heat with gold, that is, which by being barely mingled with that metal reduced to fine parts, will, without the help of external heat, produce upon the commixture of thoſe two bodies very ſenſible heat.

3. THE affirmative of this queſtion is poſitively aſſerted by ſome writers and others, that pretend to the tranſmutation of metals: for, among theſe, I have met with ſome, that aſcribe this virtue of incaleſcence with gold to the mercuries extracted, as they ſuppoſe, from ſome complete metals, which are therefore in their phraſe ſtiled *mercurii corporum*, or the mercuries of the metalline bodies.

4. BUT the negative part of the queſtion is more generally maintained, being not only embraced by far the greateſt number of philoſophers and phyſicians, but aſſented to by many of the more learned Spagyriſts themſelves, eſpecially the modern, divers of whom have reckoned this ſort of mercuries among the chimæra's and *non-entia* of bragging chemiſts. And I have the leſs wondered to find many learned men ſo averſe from believing this incalſcence of mercury and gold, becauſe, having purpoſely enquired of ſeveral prying alchemiſts, that have ſpent much labour, and many trials, to find out things of this kind, and have of

De Mercurio cum auro incaleſcente.

—VERÙM *enimverò, ne quæ hactenus diſſerui eò torqueantur, ac ſi laudes animoſque viris illis Spagyricis demere velimus, qui nobiliora auri & argenti vivi arcana poſſident ambiuntve, monendus es mihi, ut advertas, me quod contra ſuppoſitam auri & mercurii ſympathiam objeci, de vulgari duntaxat mercurio dictum velle, cum ille ſit, cujus cum auro ſympathia celebrari ſueverit. Et quamvis fortè magna à me dictorum pars, conſultâ experientiâ, ad nativum etiam mercurium currentem extendi poſſit; non tamen cenſere lectorem velim, negare me, dari argentum vivum poſſe vulgari ſubtilius & penetrantius, iſtoſque chymicos, qui auri & mercurii ſympathium niti voluut mercurii magis philoſophici operationibus, contendere etiam pro ea multò ſpecioſius poſſe, quàm ſi vulgaris duntaxat mercurius adhibeatur. Atque ut hac occaſione teſtatum faciam, me viros chymiæ addictos neutiquam averſari; ſubjungam hîc ſcripti mei partem, ad amicum quendam idc rcò exarati, ut meam ipſi de mercurii cum auro incaleſcentia opinionem depromerem.*

— NUNC *verò abruptè ſectionem hanc ordiar problematis cujuſdam diſcuſſione, quod diu multumque inter curioſos fuit agitatum, eos imprimis, qui, ſive verè ſive falsò, obtendunt, ſe intimiores, quàm vulgo conceſſum eſt, chymiæ receſſus adiiſſe: inter quos id calidè diſceptari reperio, utrum ejuſmodi detur mercurius, qui incaleſcat cum auro, id eſt, qui, dum nudè metallo iſti, ad minutas admodùm partes redacto, commiſcetur, citra externi caloris adminiculum, factâ ſolummodò duorum illorum corporum cramate, ſenſibilem valdè calorem pariat.*

HUJUS *quæſtionis affirmativam mordicùs tenent nonnulli authores, aliique, qui metallorum tranſmutationem ſibi vendicant; inter hos quippe nonnullos videre mihi licuit, qui hanc incaleſcendi cum auro virtutem mercuriis adſcribunt, ex perfectis quibuſdam corporibus, ut autumant, elicitis; quos idcircò* mercurios corporum, *ſive mercurios corporum metallicorum, nuncupare ſolent.*

AT *negativam tuentur multò plures, iique non modò philoſophi & medici, ſed & ex ipſis Spagyricis doctrinâ clariores, imprimis, ex neotericis & modernis, quorum non pauci hanc mercuriorum familiam chimæris & non-entibus grandiloquentium chemiſtarum accenſent. Atque eò minùs mirâbar, complures viros doctos adeò eſſe ab hoc mercurii cum auro incaleſcentiæ aſſenſu alienos, quia, conſultò quæſiti à me plures ex alchymiſtis ſagacioribus, qui multum impenderant operæ, plurimaque experimenta peregerant ad hujus generis arcana depromenda, quique per aliquot annos noviſſimos varias* Europæ *partes permearant, ut aliorum, qui tranſmutationes me-*

of late years, travelled into many parts of *Europe*, to pry into the secrets of other seekers of metalline transmutations, they have apart ingenuously confessed to me, that they never actually saw any incalescent mercury, though they sometimes heard it boasted of by all alchemists, whose bold pretensions had the less weight with me in this matter, because I had long taken notice, how great a confidence, fraud, or ignorance (for I would not think all those cheats, that are mistaken,) can give to some of that sort of men, that I am speaking of. Insomuch that one of them having imposed upon an honest chemist, well known, and much employed, with a pretended incalescent mercury, they had the confidence to bring it me to convince me of the experiment; but upon due trial, I found not any sensible degree of that great heat, that was promised. Which miscarriage was vainly pretended to be salved by I know not what unsatisfactory excuses.

metallicas vestigant, secreta rimarentur, illi, inquam, singuli seorsim à me rogati ingenuè apud me fassi sunt, se revera nunquam incalescentem ullum mercurium vidisse, licèt id quandoque jactatum ab alchymistis audivissent; quorum jactabundi obtentus eò minùs apud me in hoc negotio valebant, quòd à longo jam tempore notaveram, quantam fraus vel ignorantia (non enim omnes illos haberi impostores velim, qui hallucinantur) in nonnullis hujusmodi, de quibus loquor, viris fiduciam parere possint; quæ sanè tanta erat, ut illi, cùm eorum unus, bonæ frugis chymistam, multis notum multisque operam suam locantem, supposito mercurio incalescente fefellerat, eò fiduciæ abriperentur, ut apud memet se sisterent, de experimento illo me convicturi. At, re, ut par erat, exploratâ, nullum percepi sensibilem illius caloris gradum, quem promiserant.

5. BUT, notwithstanding all this, having, for the reasons I have long since expressed in other papers (and for some other considerations, that I have not judged fit to mention) looked upon mercury as a body, which is not necessarily so homogeneous, as it is supposed, the opinion I most liked of was that of a possibility of an incalescent mercury. For, notwithstanding the vulgarly supposed simular nature of quicksilver, which I willingly confess to be great enough to be admirable, it was yet congruous to my principles, that a liquor, which in weight, colour, total volatility, &c. was answerable to all the essential properties for which a body is called mercury, might yet have an internal constitution of parts, that might make it in some unobserved things considerably differing from common mercury. And, among these differing qualites, I did not know but one might well be, that of growing hot with gold. And this opinion I judged the more reasonable, because, having devised two ways (unpractised, that I know of, by any chymist) the one, to discover, whether a clean and carefully distilled mercury might not be a compounded body, and have in it parts, that were not mercurial; and the other, out of such a fine distilled mercury to separate parts, and that in no despicable number, that are plainly heterogeneous; I found, upon trial, that both the methods I had thought on would succeed, which warranted me to think it possible, that a mercury very fine and clean, and even purged by sublimations and distillations, may, by art, have been made to assume and incorporate with it a multitude of heterogeneous corpuscles, not to be discovered, much less separated, (as those of tin, lead, &c. may be) but by a skilful artist.

VERÙM *enimverò, his omnibus nequicquam obstantibus, cùm ex rationibus dudum in alio scripto à me expositis, aliisque de causis hìc non memorandis, argentum vivum corpus reputem, quod non necessariò tam sit homogeneum, ac passim habetur; illa mihi opinio præ cæteris allubuit, quæ mercurii incalescentis possibilitatem adstruit. Etenim, non obstante vulgò suppositâ mercurii (ut sic dicam) similaritate, quàm adeò eximiam esse puto, ut parere admirationem possit, meis tamen principiis consonum erat, liquorem quendam, qui pondere, colore, totali volatilitate, &c. omnes referebat proprietates essentiales, quarum respectu corpus aliquod mercurii nomine venit, habere tamen posse internam ejusmodi partium diathesin, quæ in nonnullis hactenus non observatis insignem illi à mercurio vulgari discrepantiam conciliare queat: atque has inter qualitates differentes nesciebam, annon ea recenseri meritò posset, quâ incalescit cum auro commixtus. Atque hanc opinionem rationi eò magis consentaneam arbitrabar, quòd, excogitatâ à me duplici methodo (hactenus à chymicorum nullo, quòd sciam, in praxin versâ,) unâ quidem, ut manifestum redderem, essetne purus curatéque distillatus mercurius corpus compositum, partesque contineret non mercuriales; alterâ verò, ex purificato ejusmodi & distillato mercurio partes separandi non paucas manifestò heterogeneas; experiundo comperi, utramque illam methodum à me inventam successu gaudere: id quod auctoramentum mihi haud leve erat, ut possibile existimarem, mercurium valdè defæcatum, quin & per sublimationes & distillationes repurgatum, arte posse eò redigi, ut assumat secúmque conflet heterogeneorum corpusculorum multitudinem, quæ nonnisi à perito artis filio detegi, multò minus segregari queant (ut fieri de stanneis, plumbeis, &c. corpusculis potest.)*

6. THIS, in the general, may suffice to make me suspend my judgment about the problem formerly proposed, and to engage me to make trials, whether some of these heterogeneous particles, that I found reducible with mercury into a lasting mercurial flux, might not so alter it, as to dispose it to heat with gold. But this was not sufficient to determine me to an

HOC *generatim suffecerit, ut meum de problemate suprà proposito judicium suspendam, & ad experimenta sumenda properem, quibus palàm reddatur, annon aliquæ ex particulis illis heterogeneis, quas cum mercurio in durabilem fluxum mercurialem reduci posse deprehendi, ita alterare eum possint, ut ad incalescendum cum auro ipsum disponant. At non erat hoc satis ad eliciendum à me assensum;*

an aſſent; for to oblige me to admit incaleſcent mercuries, it ought not to ſuffice, that it is poſſible, or even probable, that there may be ſuch, but there was neceſſary ſome poſitive proof, that there are ſuch; and that alſo, through God's bleſſing, my trials afforded me about the year 1652.

7. SOME years after I was in poſſeſſion of this mercury, I found in ſome of their books, that chemiſts call philoſophers, ſome dark paſſages, whence I then gueſſed their knowledge of it, or of ſome other very like it; and in one of them I found, though not all in the very ſame place, an allegorical deſcription of it, the greateſt part of which was not very difficult for me to underſtand; but not finding there any notice taken of the property of this mercury to grow hot with gold, I was induced to ſuſpect, that either they had not the knowledge of it, or judged it unfit to be ſpoken of. But you will, I ſuppoſe, expect from me rather narratives than conjectures. And, indeed, it is but reaſonable, that, having but mentioned to you a phænomenon, whoſe credibility is by many denied, I ſhould take notice of ſome circumſtances fit to bring credit to it. And I ſhall the leſs grudge the pains of ſetting down ſeveral particular phænomena, becauſe I preſume you have not met with them, and becauſe alſo it may gratify ſome of your chemical friends, who may have or diſcover ſome noble mercury, by helping them to examine it, and to try, whether it reſembles ours.

8. THAT I might not then be impoſed on by others, I ſeveral times made trial of our mercury, when I was all alone, For when no body was by me, nor probably dreamed of what I was doing, I took to one part of the mercury, ſometimes half the weight and ſometimes an equal weight of refined gold reduced to a calx or ſubtle powder. This I put into the palm of my left hand, and putting the mercury upon it, ſtirred it and preſſed it a little with the finger of my right hand, by which the two ingredients were eaſily mingled, and grew not only ſenſibly, but conſiderably hot, and that ſo nimbly, that the incaleſcence did ſometimes come to its height in about a minute of an hour by a minute-clock. I found the experiment ſucceed, whether I took altogether, or but half as much gold as mercury; but the effect ſeemed to be much greater when they were employed in equal weight. And, to obviate a ſuſpicion, which, though improbable, might poſſibly ariſe, as if the immediate contact of the ingredients and the ſkin produced a ſenſe of heat, which was not due to the action of the metals upon one another; I had the curioſity to keep the mixture in a paper, and found not its interpoſition to hinder me from feeling the incaleſcence, though it much abated the degree of my ſenſe of it.

9. I tried alſo the ſame mercury with refined ſilver reduced to a very fine powder; but I could not perceive any heat or warmth at all; though, I am apt to think, that if I had had a ſufficient quantity of leaf-ſilver to have made the experiment with, I ſhould, after ſome time, have

ſum; ut enim ad mercurios incaleſcentes admittendum adducerer, ſufficere non debebat, poſſibiles eos eſſe, vel etiam probabiles, ſed reverâ tales dari manifeſtâ probatione erat evincendum: & hoc ipſum quoque, favente Deo, experimenta mea, anno 1652. circiter, comprobarunt.

POST *aliquot ab eo tempore annos, quo mercurium hujuſmodi jam poſſidebam, in quibuſdam ex eorum, quos turba chymica philoſophos nuncupat, libris obſcura quædam loca inveniebam, unde tunc eorum de ipſo, vel alio aliquo perquam ei ſimili, cognitionem conjectabam; atque in ipſorum uno reperiebam (non tamen rem totam in uno planè eodemque loco) deſcriptionem ejus allegoricam, cujus pars maxima adeò difficilis intellectu mihi non erat: at cùm nihil ibi notatum viderem de illa mercurii hujus proprietate, quâ calorem cum auro acquirit, in ſuſpicionem incidi, eos vel cognitione illius fuiſſe deſtitutos, vel eam ſilentio premendam cenſuiſſe. At tu ſine dubio facti potiùs narrationes, quàm conjecturas à me exſpectas. Et ſanè æquum omninò fuerit, ut, cùm mentionem duntaxat fecerim phænomeni, cujus à multis negatur credibilitas, circumſtantias nonnullas annotem, quæ fidem ei conciliare valeant. Atque eò minùs laborem detrectabo particularia aliquot phænomena hìc tradendi, tum quòd ea tibi non occurriſſe autumem, tum quòd ea grata fore putem quibuſdam amicis tuis chymicis, nobilem quendam vel jam poſſidentibus vel paraturis mercurium, ut ſcil. hoc qualicunque ſcripto noſtro ad eum examinandum, &, an referat noſtrum, experiundum, juventur.*

ITAQUE, *ne mihi imponerent alii, pluries mercurium noſtrum, quando ſolus eram, explorabam. Etenim quando nemo mihi aderat, neque quiſquam per ſomnium quid agerem conjiceret, ſumebam unam partem illius mercurii, ad auri, in calcem vel pollinem redacti, pondus quandoque dimidium, quandoque æquale. Hoc polline volæ manus ſiniſtræ immiſſo, & mercurio ſuperinfuſo, utrumque ſimul agitabam, premebamque nonnihil digito manûs dextræ; quâ ratione duo hæc ingredientia facilè commixta, non modò ad ſenſum ſed inſigniter incaleſcebant, idque adeò properè, ut incaleſcentia interdum unius horæ circiter minuto, indicante idipſum horologio minutis inſtructo, ad ἀκμὴν perveniret. Succedebat hoc experimentum, ſive æqualem ſumerem ſive dimidiam auri quantitatem; effectus tamen multò videbatur inſignior, quando æquali pondere adhibebantur. Atque, ut ſuſpicioni, quæ, licèt improbabilis, ſubnaſci tamen poſſet, occurrerem, immediatum ſcil. ingredientium & cutis contactum producere poſſe ſenſum caloris, qui non debeatur metallorum in ſe invicem actioni, curioſitate ducebar mixturam hanc in charta ſervandi; quo facto, interpoſitionem ejus nequaquam impedire incaleſcentiæ ſenſum comperiebam, quanquam, ex natura rei, intenſiorem illius gradum remitteret.*

PORRÒ *mercurium eundem cum repurgato argento, ad ſubtilem valdè pulverem redacto, exploravi; at nullum omnino calorem percipere potui; quanquam eò ferar, ut exiſtimem, ſi ſufficiens argenti foliati quantitas, ad peragendum experimentum, mihi ſuppetiiſſet, me poſt aliquot temporis* have produced an incalescence, though much inferior to what the same quantity of mercury would produce with gold; but this only upon the by. I shall now add, that to the end I might not be thought to impose upon myself, I did not make trial in my own hand, when it was in different tempers, as to heat and cold, but I did it in the hands of others, who were not a little surprised and pleased at the event. And this I did more than once or twice; by which means I had, and still have, divers witnesses of the truth of the experiment, whereof some are noted persons, and especially him, to whom I last shewed it, which you will easily believe, when I tell you it is the learned secretary of the Royal Society; to whom having given the ingredients, I desired him to make the experiment in and with his own hands, in which it proved successful within somewhat less than a minute of an hour *.

10. And that, which makes this incalescence the more considerable is, that being willing to husband my mercury, a great part of which had been, as I guessed, stolen from me before I employed it, I made these trials but with a drachm at a time, which scarce amounts in quantity to the bigness of half a middle-sized bean; whereas, if I could have made the experiment with a spoonful or two of quicksilver, and a due proportion of gold, it is probable the heat would have been intense enough, not only to burn one's hand, but perchance to crack a glass vial; since I have sometimes had of this mercury so subtle, that when I employed but a drachm at a time, the heat made me willing to put it hastily out of my hand.

11. These things being matters of fact, I scruple not to deliver them; but I would much scruple to determine thence, whether those, that are *mercurii corporum*, and were made, as chemists presume, by extraction only from metals and minerals, will each of them grow hot with gold, as, if I much mistake not, I found antimonial mercury to do. And much less would I affirm, that every metalline mercury (though never so disposed to incalescence) or even that of silver or gold itself, is the same with that, which the chrysopæan writers mean by their philosophic mercury, or is near so noble as this. Nay, I would not so much as affirm, that every mercury, obtained by extraction even from the perfect metals themselves, must needs be more noble and fit (as alchemists speak) for the philosophic work, than that, which may with skill and pains be at length obtained from common mercury skilfully freed from its recrementitious and heterogeneous parts, and richly impregnated with the subtle and active ones of congruous metals or minerals. These and the like points I should, as I was saying, much scruple at offering to determine in this place, where what I designed to deliver was historical, though I have not thought it impertinent to glance at the points lately mentioned, because those glances may intimate things conducive to the better understanding of what I have said, and have to say in this paper.

* Since this was written, the noble and judicious president of the Royal Society, the lord viscount *Brouncker*, made the same experiment with some of the same mercury, in his own hand with good success.

poris spatium incalescentiam suscitaturum fuisse, quamvis multò inferiorem eo, quem eadem mercurii quantitas cum auro produceret: at hoc nonnisi in transitu. Adjiciam nunc, me, ne mihimet imposuisse censerer, non tantùm rem hanc explorasse in manu mea, quando variè erat pro caloris & frigoris ratione temperata, sed & in manibus aliorum, quos non parùm attonitos habebat, juvabatque eventus. Atque hoc ipsum pluries quàm semel bisve feci; unde mihi testes suppetunt experimenti veritatis assertores, probatæ fidei viri, quorum unus erat eruditus Societatis Regiæ secretarius, quem, exhibitis ei ingredientibus, rogabam, ut suismet manibus experimentum caperet; in quibus & optatum successum minori quàm unius minuti spatio sortiebatur †.

Atque, *quod incalescentiam hanc insigniorem reddit, est, quòd, cùm parcè uti mercurio meo cuperem, quippe cujus magna pars (ut conjicio) surrepta mihi fuerat, priusquam eum adhiberem, experimenta singula nonnisi cum una drachma peragebam, quæ vix fabæ mediocris dimidiæ magnitudinem æquat, cùm, si copia mihi fuisset capiendi experimentum cum cochleari uno alteróve mercurii pleno, supparique quantitate auri, probabile sit, calorem inde oriturum fuisse satis intensum, ut non modò ureret manum, sed forsan & in phiala vitrea rimas ageret; quandoquidem interdum hujus generis mercurium habui adeò subtilem, ut, adhibente me singulis vicibus nonnisi drachmam unam, calor me adegerit, ut properè è manibus mixturam deponerem.*

Hæc, *cùm sint res facti, tradere non dubito; at valdè ambigerem exinde determinare, num, qui appellantur mercurii corporum, paranturque, ut jactant chymici, sola extractione ex metallis & fossilibus, eorum quilibet calorem acquirat cum auro, quemadmodum, ni multum fallor, mercurium antimonialem acquirere comperi. Multóque minus affirmarem, quemvis mercurium metallicum (quantumcumque ad incalescentiam dispositum,) quin & mercurium argenti aurive ipsius eundem esse cum eo, quem scriptores chrysopæi per mercurium suum philosophicum intelligunt, vel præstantiâ suâ ad hunc accedere. Quin imò, ne quidem assererem, quemlibet mercurium, extractione etiam ab ipsis perfectis metallis impetratum, nobiliorem esse oportere, & (ut loquuntur alchymistæ) ad philosophicam operatiorem magis idoneum, quàm illum, qui, peritiâ & industriâ comite, obtineri tandem potest à mercurio vulgari, à partibus suis recrementitiis heterogeneisq; purgato, subtilibusque & efficacibus metallorum mineraliumve congruorum partibus uberrime fœto: hæc, inquam, & similia hoc loco affirmare admodum vererer; cùm hîc nonnisi ea tradere instituerim, quæ ad rei historiam faciunt; quanquam præter rem non existimaverim, jamjam indigitatos rei hujus apices innuere, quòd stricturæ istæ ea possint lectori ingerere, quæ ad meliorem tum dictorum tum dicendorum intelligentiam conducere queant.*

† *Ex quo tempore hoc literis fuit consignatum, illustrissimus & judiciosissimus Regiæ Societatis præses, Dom. Vicecomes* Brouncker, *idem experimentum suâ cum ejusdem mercurii portione manu cum successu peregit.*

12. I doubt not but what I have related and hinted has given you a curiosity to know somewhat further of this mercury: and I confess, that if there be any truth in what some of the most approved Spagyrists have delivered about a solvent of gold, that seems of kin, and perhaps is not much nobler than one, that I had; it seems allowable to expect, that even ours should be of more than ordinary use, both in physick and alchemy. But the misfortune I had to have lost a considerable quantity of it, being afterwards increased by the almost sudden death of the only operator I trusted in the making of it; I was altogether discouraged from repeating such a troublesome preparation, especially being diverted by business, removes, sickness, and more pleasing studies. And though I have not forgot some not despicable trials, that I made with our mercury, yet since they are not necessary to the question, that occasioned this paper, I shall pass them over in silence, and only observe some few things I had almost forgot to tell you; namely, first, that whereas it is usual to take four, five, or six, nay eight or ten parts of common quicksilver, to make an amalgame with one of gold, even when both are heated by the fire; I found our mercury so congruous to that metal, that it would presently imbody with no less than an equal weight of it, and produce a pretty hard amalgame or mixture, in which the mercury was so diffused, that the gold had quite lost its colour. Secondly, I shall add what, for aught I know, has not been yet observed, that this power of penetrating gold and growing hot with it, is so inherent, not to say radicated, in our mercury, that after it had been distilled f om gold again and again, I found it to retain that property. And, lastly, whereas it may be suspected, that this faculty may be quickly lost, (as that of the prepared Bononian stone to receive light, has been complained of as not durable) I found by trial, that a single drachm of mercury, made after a certain manner, did, the third or fourth year after I had laid it by, grow so hot with gold, that I feared it would have burnt my hand.

THUS far the author to his friend: but when he sent me the paper, he accompanied it with the following lines;

13. I have little at present to say to you about the papers, which this sheet accompanies, save that one of the chief reasons, that makes me backward to have the foregoing observations communicated to the curious, is, that I fear, we may thereby procure divers queries and perhaps requests, (relating to this mercury) which I would by all means avoid, for divers reasons, and particularly for this, that a great weakness of that part disables me to write with my own hand, and I know, you will not think it fit I should, about such a subject, employ that of an amanuensis. And therefore I cannot consent, this paper should go out of your hands, unless you can think on some likely course to secure me from trouble, and from the unwelcome necessity of disobliging some, whilst I endeavour to gratify others. If this precaution be used, I may safely learn, by means of

NON *dubito, quin hactenus à me enarrata indigitataque curiositatem in te pepererint, aliquid amplius de hoc mercurio cognoscendi: & fateor, si quid veri subest ei, quod quidam ex probatissimis spagyricis de quodam auri dissolvente, quod affine videtur nostro, nec eo fortè multò est nobilius, tradiderunt; exspectare fas fuerit, ipsissimum hoc nostrum in insignem, cum in medicina, tum in alchymia, usum cedere posse. Verùm cùm infortunium illud, quo insigniori quantitate ejus fui privatus, stipatum fuerit subitâ morte operatoris unici, cui in eo parando penitùs fidebam, mentem planè alienam ab iteranda tam molestâ præparatione sensi; maximè cùm occupationes, migrationes, adversa valetudo, studiaque gratiora aliorsum me traherent; & licèt experimenta quædam non spernenda, cum mercurio nostro peracta, memoriâ meâ non exciderint; cùm tamen ad quæstionem illam, quæ scriptum hoc peperit, non sint necessaria, silentio ea involvam, paucula duntaxat annotaturus, quæ commemorare propemodum fuissem oblitus. Quorum primum est, quòd, cùm solenne sit capere mercurii vulgaris partes quatuor, 5 vel 6, imò 8 vel 10, ad amalgama faciendum cum una parte auri, etiam tùm, quando utrumque incaluit igne; ego adeò congruum deprehenderim cum metallo illo mercurium nostrum, ut non minus quàm æquale illius pondus intimè statim pervaderet, satisque durum amalgama cramáve produceret, in quo adeò diffusus erat mercurius, ut aurum colorem suum penitus amitteret. Secundum est, (quod hactenus observatum fuisse haud putem,) vim scil. hance, aurum penetrandi, cúmque eo incalescendi, mordicùs adeò inhærere mercurio nostro, ne dicam ita in eo radicatam esse, ut postquam iterum atque iterum ab auro esset distillatus, proprietatis illius tenacem eum deprehenderim. Et denique, cùm suspicio incessere lectorem possit, facultatem hanc citò deperdi, (ut de præparato ad hauriendam lucem lapide Bononiensi queruntur authores) experiundo didici, unicam drachmam mercurii, certo modo parati, post tertium quartúmve à quo seposuerum annum adeò cum auro incaluisse, ut ne adureret manum meam, timerem.*

HACTENUS *author noster ad amicum suum: sed cùm mihi chartas illas mitteret, voluit eas sequenti mantissâ locupletare;*

NON *diu te morabor differendo de chartis hîc junctis: dicam solummodò, unam ex præcipuis rationibus, quæ in vulgandis prægressis observationibus cunctabundum me faciunt, hanc esse, quòd vereor, nos hoc ipso variis circa mercurium hunc quæstionibus & fortè sollicitationibus ansam daturos, quas omni studio præcavere velim, cum ob alias, tum hanc ob causam, quòd magna manuum mearum debilitas me impedit, quò minus meamet manu id consignare literis valeam, quod conscribi amanuensis operâ consultum haud judicaveris. Proindéque concedere haud possum, scriptum hoc è manibus tuis dimitti, nisi rationem suggeras probabilem, quâ securum me præstes à molestia, atque ab ingrata necessitate repulsam dandi nonnullis, dum aliis obsecundare studeo. Hac cautelâ si utaris, potero amplissimæ tuæ consuetudinis beneficio citra molestiam edoceri, quid ii, qui tantâ peritiâ tantoque judicio valent,* your diffused acquaintance, what those, that are skilful and judicious enough to deserve to be much considered in such an affair, will think of our mercury, and whether, in case they have an esteem of it approaching to that of divers eminent chemists (some of which importune me to impart it;) they judge the good, that the preparations of it (such as precipitats and turbiths of divers kinds, mercurius dulcis, cinaber made of the sulphur of antimony, and with gold, &c.) may do in physick, is likely much to exceed the political inconveniencies, that may ensue, if it should prove to be of the best kind, and fall into ill hands. The knowledge of the opinions of the wise and skilful about this case will be requisite to assist me to take right measures in an affair of this nature. And, till I receive this information, I am obliged to silence.

14. ONLY, in the mean while, I shall, for the sake of the enquiries into the mercurial arcana, make bold to add a secret, which, I think, will to divers philalethists and other students of the chemical philosophers books seem a paradox, if not an untruth; namely, that a mercury, qualified to heat with gold, and perhaps with other powders, may be made by more ways than one or two; experience having assured me (whatever authorities or theories may be urged to the contrary) that such a mercury may be (I say not, easily or speedily, but successfully) prepared, not only by employing antimony and solid metals, as mars, but without any such metal at all, or so much as antimony itself.

15 HERE I purposed to conclude: but, because I am, as you know, very averse (which I declare myself to be on this occasion also) from making any promise to the publick, I think fit in this place to give you an advertisement, and obviate a scruple. I shall therefore admonish those inquisitive Spagyrists, that may be desirous to try, whether their purified mercury be incalescent, that they be not too hasty to conclude it is not so; nor to reject it, unless they have made the trial with gold duly prepared. For I have found, that my mercury did not grow hot with the smallest filings of gold I could make (though indeed within a few hours after it did, without the help of fire, imbody with it into a hard amalgama,) which argued, that the corpuscles of the metal were not yet small enough to be suddenly penetrated by the quicksilver: nor will every calx of gold serve our turn, as I have found by employing, without success, a very fine and spongy calx made after an uncommon way, the golden particles having, as it seemed, some extremely fine, though unobserved dust of the additament sticking to them, which hindered the adhæsion of the mercurial ones. Now, the calx of gold, that I most used, as finding it still to do well, was that made by quartation *, as alchemists call it. But because it is not so easy, as even chemists, that have not tried, imagine, to make good

* That is, by melting together one part of fine gold, and three or four parts of cuppelled silver, and then putting the mass, wherein the metals are mixed, almost *per minima*, into purified aqua-fortis, which dissolving the silver only, leaves the gold in the form of a fine calx.

valent, ut in hoc negotio magni fieri mereantur, de mercurio nostro sentiant; adhæc utrum, si æstimationem de eo foveant illi supparem, quam præcellentium chymicorum complures (quorum nonnulli me urgent ad eum communicandum) præ se ferunt, verisimile censeant, utilitatem, quam præparationes ipsius (cujusmodi sunt præcipitata & turbithi diversorum generum, mercurius dulcis, cinnabaris ex antimonio & auro cum parata, &c.) afferre possint rei medicæ, longè superaturam esse incommoda illa politica, quæ nascitura forent, si fortè de præstantissima esset indole, atque in maleferiatas manus incideret. Sapientum & peritorum hoc in casu opiniones cognoscere, necessarium mihi fuerit, ut recto tramite in istiusmodi negotio incedere mihi detur. Atque, donec edoctus id fuero, silentii sacra colere teneor.

INTERIM *in eorum gratiam, qui arcana mercurialia scrutantur, subjungere ausim secretum aliquod, quod philalethis compluribus, aliisque, qui chymicorum philosophorum libris meditandis incumbunt, paradoxum, quin & falsum fortè videbitur: mercurium scil. ad incalescendum cum auro aliisve pulveribus idoneum, modis uno binóve pluribus parari posse; cùm per experientiam certò mihi constet, (quicquid in contrarium obtendant authoritates & theoriæ) talem mercurium posse, (non dicam facilè properéve, sed cum successu) parari, non modò antimonium solidáque metalla, putà martem, &c. adhibendo, sed citra ullius omnino metalli, quin vel ipsius antimonii, usum.*

HIc *statueram finem huic sermoni imponere: at cùm ægerrimè, ut nosti, tum aliàs, tum hac imprimis occasione, promissi fidem publico obstringam, visum est mihi hoc loco monitum aliquod suggerere, & scrupulo cuidam obviam ire. Prius quod attinet, curiosos illos Spagyricos, quos fortè tentandi cupido incesserit, sitne purgatus ipsorum mercurius incalescendi qualitate instructus, monebo, ne nimis festinantur concludant ipsum eâ præditum non esse, néve eum rejiciant, nisi experimentum fecerint cum auro ritè præparato. Comperi quippe, mercurium meum non incalescere cum ramentis auri, omnium quas conficere poteram minimis, (quanquam reverâ intra paucas exinde horas, sine ignis adminiculo, cum ipso in durum amalgama conflaretur;) quod argumento erat, metalli illius corpuscula necdum exigua satis fuisse, ut properè à mercurio penetrarentur: neque quævis auri calx rem nostram conficiet; ut comperi, dum perquàm subtilem spongiosamque calcem, modo non vulgari paratam, citra successum adhibui, in qua, ut videtur, apprimè tenuis sensumque fugiens additamenti pulvis adhærebat particulis aureis, & mercurialium adhæsionem præpediebat. Jam verò calx auri, quâ plerumque utebar successu ejus inductus, illa erat, quæ quartationis † (ut vocant) beneficio paratur. At quia non adeò facilè est, ut ipsi chymici, qui manum operi non admoverunt, sibi imaginantur, bonæ notæ calces auri parare, cumque in*

† *Hoc est, per fusionem conflando unam partem auri puri, & tres quatuórve partes argenti cupellati, ut vocant, & tunc immittendo massam, in qua metalla miscentur quasi per minima, in purgatam aquam fortem, quæ solum argentum dissolvens, aurum in forma calcis relinquit.*

good calces of gold, and that in the way newly mentioned, there needs fusion of gold and of silver (for which many chemists want conveniencies,) and they are often imposed on by common refiners, who here usually sell in wires such silver for fine (which indeed it is comparitively,) as I have found not to be without mixture; I shall add, that by making an amalgama, the common way, with pure gold and vulgar mercury, and dissolving the mercury in good aqua-fortis, there will remain a powder, which, being well washed with fair water to dulcify it, and kept a while in a moderate fire to dry it thoroughly without melting it, will become a calx, which I have more than once used with our mercury with good success. It is true, both in this way and in that (by quartation) aqua-fortis, which is a corrosive liquor, is employed to bring the gold to powder, and therefore in a diffident mind some suspicion may arise, that the incalescence may proceed only from the action of the acid particles of the menstruum, which yet adhering to the corpuscles of the gold works upon the quicksilver, as aqua-fortis is known to do: but, to omit those answers, that cannot be given in few words, after I have taken notice, that, if the effect depends not on our mercury (as prepared) but only on the calx, it appears not, why this should not grow hot with common mercury, as well as with ours; I shall need to add, for the removal of this subtile scruple, no more than this plain experiment, (which I twice or thrice made,) namely, that taking, instead of a calx of gold, a competent number of leaves of gold, such as book-binders and the apothecaries use, this gold, that was without the help of salts reduced by beating to a sufficient thinness (insosomuch that seventy odd leaves did not weigh a scruple,) I found (more than once) upon putting two or three times the weight of our mercury to them, that a smart heat was presently produced in my hand.

in methodo jamjam memorata requiratur fusio auri & argenti (cujus peragéndæ commoditate non pauci chymici destituuntur,) cùm etiam crebrò à vulgaribus metallorum purgatoribus fallantur, qui hîc passim, filorum formâ, ejusmodi argentum pro puro venditant (quale, comparatè loquendo, reverâ est,) quod non esse mixturæ expers deprehendi; adjiciam, quòd, dum communi more amalgama conficitur cum auro puro & mercurio vulgari, mercuriusque dissolvitur bonâ aquâ forti, remansurus sit pulvis, qui cum aqua pura, ad conciliandam ei, quam vocant, dulcedinem, probè elotus, & aliquandiu in temperato igne, ad eum penitus exiccandum citra fusionem, asservatus, talem calcem præbebit, quâ pluries cum mercurio nostro feliciter usus fui. Fateor equidem, tum in hac methodo, tum in illa, quæ instituitur per quartationem, adhiberi aquam fortem, liquorem scil. corrosivum, ad aurum in pulverem redigendum, unde scrutanti genio suboriri suspicio poterit, incalescentiam illam soli actioni acidarum particularum menstrui acceptam ferendam esse, quod hærens etiamnum auri corpusculis, in mercurium operetur, solenni aquæ fortis more. Verùm, (ut eas responsiones sileam, quæ paucis tradi non possunt,) postquam notavi, quòd, si effectus hic non dependet à mercurio nostro (ritè præparato,) sed à sola calce, non pateat, quare hæc non incalescat æquè cum mercurio vulgari ac nostro; opus haud fuerit, aliud quicquam ad scrupulum hunc eximendum, quàm obvium hoc experimentum, quod sequitur, quodque bis térve à me paractum fuit, adjicere: sumpsi, inquam, calcis auri loco, sufficientem numerum foliorum auri, qualibus utuntur bibliopegi & aurifabri; hoc aurum, quod citra salium opem tundendo redactum erat ad tenuitatem sufficientem (adeò ut ultra septuaginta folia vix unius scrupuli pondus æquarent,) hoc, inquam, aurum comperi (unâ vice pluries,) cùm binum trinumve mercurii nostri pondus ipsi commiscerem, insignem in manu mea calorem mox peperisse.

EXPERIMENTS, NOTES, &c.

ABOUT THE

MECHANICAL ORIGIN OR PRODUCTION

OF

Divers Particular QUALITIES:

Among which is inferted

A Difcourfe of the IMPERFECTION of the CHEMIST'S Doctrine of QUALITIES;

TOGETHER WITH

Some REFLECTIONS upon the HYPOTHESIS of ALKALI and ACIDUM.

The PUBLISHER to the READER.

TO keep the reader from being at all furprized at the date of the title-page, I muft inform him, that a good part of the enfuing tracts were printed off, and in my cuftody the laft year; and the reft had come out with them divers months ago, if the noble author had not been hindered from committing them to the prefs by the defire and hope of being able in a fhort time to fend them abroad more numerous, and by his being hindered to do fo, partly by remove, partly by the want of fome papers, that were oddly loft or fpoiled, and partly by the ficknefs of himfelf, and divers of his near relations. And fome of thefe impediments do yet fupprefs what the author intended fhould have made a part of the book, which now he fuffers to be publifhed without them, though divers of his papers about fome other particular qualities have been written fo long ago, as to have lain for many years neglected among other of his old writings: which that he may have both leifure and health to review and fit for publication, is the ardent wifh of the fincere lovers of real knowledge, who have reafon to look on it as no mean proof of his conftant kindnefs to experimental philofophy, that in thefe tracts he perfeveres in his courfe of freely and candidly communicating his experiments and obfervations to the publick, notwithftanding the liberty, that hath been too boldly taken to mention them as their own by later writers; as particularly by the compiler of the treatife, entitled *Polygraphice*, who in two chapters hath allowed himfelf to prefent his reader with above fifty experiments, taken out of our author's book of colours, without owning any of them to him, or fo much as naming him or his book in either of thofe chapters, nor, that I remember, in any of the others. Nor did I think this practice juftified by the confeffion made in the preface, importing, that the compiler had taken the particulars he delivered from the writings of others. For this general and perfunctory acknowledgement neither doth right to particular authors, nor, by naming them, enables the reader to know, whether the things delivered come from perfons fit to be credited or not; and therefore, fince it is but too likely, that fuch concealment of the names, if not ufurpation of the labours of the benefactors to philofophy, will prove much more forbidding to many others to impart their experiments, than as yet they have to our generous author; it feems to be the intereft of the commonwealth of learning openly to difcountenance fo difcouraging a practice, and to shew, that they do not think it fit, that possessors of useful pieces of knowledge should be strongly tempted to envy them to the publick, to the end only that a few compilers should not be put upon so reasonable and easy a work, as by a few words or names to shew themselves just, if not grateful.

BUT not to keep the reader any longer from the perusal of these tracts themselves, I shall conclude with intimating only, that what our author saith in one of them concerning the insufficiency of the chemical hypothesis for explaining the effects of nature, is not at all intended by him to derogate from the sober professors of chemistry, or to discourage them from useful chemical operations; forasmuch as I had the satisfaction, some years since, to see in the author's hands a discourse of his about the Usefulness of Chemistry for the Advancement of Natural Philosophy; with which also it is hoped he will ere long gratify the publick.

ADVERTISEMENTS relating to the following TREATISE.

TO obviate some misapprehensions, that may arise concerning the ensuing notes about particular qualities, it may not be improper to add something in this place to what has been said in another paper * in reference to those notes, and consequently to premise to the particular experiments some few general advertisements about them.

AND I. we may consider, that there may be three differing ways of treating historically of particular qualities. For either one may in a full and methodical history prosecute the phænomena; or one may make a collection of various experiments and observations, whence may be gathered divers phænomena to illustrate several, but not all of the heads or parts of such an ample or methodical history; or, in the third place, one may in a more confined way content one's self to deliver such experiments and observations of the production, or the destruction or change of this or that quality, as being duly reasoned on, may suffice to shew, wherein the nature of that quality doth consist, especially in opposition to those erroneous conceits, that have been entertained about it. Of the first of these three ways of treating of a quality I pretend not to have given any complete example; but you will find, that I have begun such histories in my specimens about fluidity and firmness, and in the experiments, observations, &c. that I have put together about cold. The second sort of historical writings I have given an instance of in my experiments about colours; but in these ensuing notes, the occasion I had to make them, having obliged me chiefly to have an eye to the disproval of the errors of the peripateticks and the chemists about them, I hope I shall not be thought to have fallen very short in my attempt, if I have (here and there) performed, what may be required in the third way of writing historically of a quality; my present design being chiefly to give an intelligent and historical account of the possible mechanical origination, not of the various phænomena of the particular qualities succinctly mentioned in these notes; though my secondary end being to become a benefactor to the history of qualities by providing materials for myself or better architects, I have not scrupled to add to those, that tend more directly to discover the nature or essence of the quality treated of, and to derive it from mechanical principles, some others (which happened to come in my way) that acquaint us but with some of the less luciferous phænomena.

II. THAT you may not mistake what is driven at in many of the experiments and reasonings delivered or proposed in the ensuing notes about particular qualities, I must desire you to take notice with me, what it is, that I pretend to offer you some proofs of. For if I took upon me to demonstrate, that the qualities of bodies cannot proceed from (what the schools call) substantial forms, or from any other causes but mechanical, it might be reasonably enough expected, that my argument should directly exclude them all. But since, in my explications of qualities, I pretend only, that they may be explicated by mechanical principles, without enquiring, whether they are explicable by any other; that, which I need to prove, is, not that mechanical principles are the necessary and only things, whereby qualities may be explained, but that probably they will be found sufficient for their explication. And since these are confessedly more manifest and more intelligible, than substantial forms and other scholastic entities (if I may so call them) it is obvious, what the consequence will be of our not being obliged to have recourse to things, whose existence is very disputable, and their nature very obscure.

THERE are several ways, that may be employed, some on one occasion, and some on another, either more directly to reduce qualities (as well as divers other things in nature) to mechanical principles; or, by shewing the insufficiency of the Peripatetick and chemical theories of qualities, to recommend the Corpuscularian doctrice of them.

FOR further illustration of this point, I shall add on this occasion, that there are three distinct sorts of experiments (besides other proofs) that may be reasonably employed, (though they be not equally efficacious) when we treat of the origin of qualities. For some instances may be brought to shew, that the proposed quality may be mechanically introduced into a portion of matter, where it was not before.

Other

* See Tracts about Cosmical Qualities, &c. to which is prefixed an Introduction to the History of Particular Qualities.

Other inſtances there may be to ſhew, that by the ſame means the quality may be notably varied as to degrees, or other not eſſential attributes. And by ſome inſtances alſo it may appear, that the quality is mechanically expelled from, or aboliſhed in, a portion of matter, that was endowed with it before. Sometimes alſo by the ſame operation the former quality is deſtroyed, and a new one is produced. And each of theſe kinds of inſtances may be uſefully employed in our notes about particular qualities. For as to the firſt of them, there will be ſcarce any difficulty. And as to the ſecond, ſince the permanent degrees, as well as other attributes of qualities are ſaid to flow from (and do indeed depend upon) the ſame principles, that the quality it ſelf does; if, eſpecially in bodies inanimate, a change barely mechanical does notably and permanently alter the degree or other conſiderable attribute; it will afford, though not a clear proof, yet a probable preſumption, that the principles, whereon the quality it ſelf depends; are mechanical. And laſtly, if, by a bare mechanical change of the internal diſpoſition and ſtructure of a body, a permanent quality, confeſſed to flow from its ſubſtantial form, or inward principle, be aboliſhed, and, perhaps, alſo immediately ſucceeded by a new quality mechanically producible; if, I ſay, this come to paſs in a body inanimate, eſpecially, if it be alſo, as to ſenſe ſimilar, ſuch a phænomenon will not a little favour that hypotheſis, which teaches, that theſe qualities depend upon certain contextures, and other mechanical affections of the ſmall parts of the bodies, that are endowed with them, and conſequently may be aboliſhed when that neceſſary modification is deſtroyed. This is thus briefly premiſed to ſhew the pertinency of alledging differing kinds of experiments and phænomena, in favour of the corpuſcular hypotheſis about qualities.

WHAT has been thus laid down, may, I hope, facilitate and ſhorten moſt of the remaining work of this preamble, which is to ſhew, though but very briefly, that there may be ſeveral ways, not impertinently employable to recommend the corpuſcularian doctrine of qualities.

FOR firſt, it may ſometimes be ſhewn, that a ſubſtantial form cannot be pretended to be the neceſſary principle of this or that quality; as will, for inſtance, hereafter be made manifeſt in the aſperity and ſmoothneſs of bodies, and in the magnetical virtue, reſiding in a piece of iron, that has been impregnated by a loadſtone. It is true, that the force of ſuch inſtances is indirect, and that they do not expreſly prove the hypotheſis, in whoſe favour they are alledged; but yet they may do it good ſervice, by diſproving the grounds and concluſions of the adverſaries, and ſo (by removing prejudices) making way for the better entertainment of the truth.

SECONDLY, we may ſometimes obtain the ſame, or the like quality, by artificial and ſometimes even temporary compoſitions, which, being but factitious bodies, are by learned adverſaries confeſſed, not to have ſubſtantial forms, and can indeed reaſonably be preſumed to have but reſulting temperaments: as will be hereafter exemplified in the production of green by compounding blue and yellow, and in the electrical faculty of glaſs; and in the temporary whiteneſs produced by beating clear oil and fair water into an ointment, and by beating water into a froth, and, more permanently, in making coral white by flawing it with heat; and in divers other particulars, that will more properly be elſewhere mentioned.

THIRDLY then, in ſome caſes the quality propoſed may be either introduced, or varied, or deſtroyed, in an inanimate body, when no change appears to be made in the body, except what is mechanical, and what might be produced in it, ſuppoſing ſuch a parcel of matter were artificially framed and conſtituted as the body is, though without any ſubſtantial form, or other ſuch like internal principle. So when a piece of glaſs, or of clarified roſin, is, by being beaten to powder, deprived of its tranſparency, and made white, there appears no change to be made in the pulverized body, but a comminution of it into a multitude of corpuſcles, that by their number, and the various ſituations of their ſurfaces are fitted copiouſly to reflect the ſincere light ſeveral ways, or give ſome peculiar modification to its rays; and hinder that free paſſage of the beams of light, that is requiſite to tranſparency.

FOURTHLY, as in the caſes belonging to the foregoing number there appears not to intervene in the patient or ſubject of the change, any thing but a mechanical alteration of the mechanical ſtructure or conſtitution; ſo in ſome other caſes it appears not, that the agent, whether natural or factitious, operates on the patient, otherwiſe than mechanically, employing only ſuch a way of acting, as may proceed from the mechaniſm of the matter, which itſelf conſiſts of, and that of the body it acts upon. As when goldſmiths burniſh a plate or veſſel of ſilver, that having been lately boiled looked white before, though they deprive it of the greateſt part of its colour, and give it a new power of reflecting the beams of light and viſible objects, in the manner proper to ſpecular bodies; yet all this is done by the intervention of a burniſhing tool, which often is but a piece of ſteel or iron conveniently ſhaped; and all that this burniſher does, is but to depreſs the little prominencies of the ſilver, and reduce them, and the little cavites of it, to one phyſically level or plain ſuperficies. And ſo when a hammer ſtriking often on a nail, makes the head of it grow hot, the hammer is but a purely mechanical agent, and works by local motion. And when by ſtriking a lump of glaſs, it breaks it into a multitude of ſmall parts, that compoſe a white powder, it acts as mechanically in the production of that whiteneſs, as it does in driving in a nail to the head. And ſo likewiſe, when the powdered glaſs, or colophony lately mentioned, is, by the fire, from a white and opacous body, reduced into a colourleſs (or a reddiſh) and tranſparent one, it appears not, that the fire, though a natural agent, need work otherwiſe

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than mechanically, by colliquating the incoherent grains of powder into one mass; wherein, the ranks of pores not being broken and interrupted as before, the incident beams of light are allowed every way a free passage through them.

FIFTHLY, the like phænomena to those of a quality to be explicated, or at least as difficult in the same kind, may be produced in bodies and cases, wherein it is plain we need not recur to substantial forms. Thus a varying colour, like that, which is admired in a pigeon's neck, may be produced in changeable taffety, by a particular way of ranging and connecting silk of several colours into one piece of stuff. Thus we have known opals casually imitated and almost excelled by glass, which luckily degenerated in the furnace. And somewhat the like changeable and very delightful colour I remember to have introduced into common glass, with silver, or with gold and mercury. So likewise merely by blowing fine crystal-glass, at the flame of a lamp, to a very extraordinary thinness, we have made it to exhibit, and that vividly, all the colours (as they speak) of the rainbow; and this power of pleasing by diversifying the light, the glass, if well preserved, may keep for a long time. Thus also by barely beating gold into such thin leaves, as artificers and apothecaries are wont to employ, it will be brought to exhibit a green colour, when you hold it against the light, whether of the day, or of a good candle; and this kind of greenness, as it is permanent in the foliated gold, so I have found by trial, that if the sun-beams, somewhat united by a burning-glass, be trajected through the expanded leaf, and cast upon a piece of white paper, they will appear there, as if they had been tinged in their passage. Nay, and sometimes a slight and almost momentary mechanical change will seem to over-rule nature, and introduce into a body the quite opposite quality to that she had given it: as when a piece of black horn is, only by being thinly scraped with the edge of a knife, or a piece of glass, reduced to permanently white shavings. And to these instances of colours, some emphatical, and some permanent, might be added divers belonging to other qualities, but that I ought not to anticipate what you will elsewhere meet with.

THERE is yet another way of arguing in favour of the Corpuscularian doctrine of qualities, which, though it do not afford direct proofs of its being the best hypothesis, yet it may much strengthen the arguments drawn from other topicks, and thereby serve to recommend the doctrine itself. For, the use of an hypothesis being to render an intelligible account of the causes of the effects, or phænomena proposed, without crossing the laws of nature, or other phænomena; the more numerous, and the more various the particles are, whereof some are explicable by the assigned hypothesis, and some are agreeable to it, or, at least, are not dissonant from it, the more valuable is the hypothesis, and the more likely to be true. For it is much more difficult, to find an hypothesis, that is not true, which will suit with many phænomena, especially, if they be of various kinds, than but with a few. And for this reason, I have set down among the instances belonging to particular qualities, some such experiments and observations, as we are now speaking of, since, although they be not direct proofs of the preferableness of our doctrine, yet they may serve for confirmation of it; though this be not the only, or perhaps the chief reason of their being mentioned. For, whatever they may be as argument, since they are matters of fact, I thought it not amiss to take this occasion of preserving them from being lost; since, whether or no they contribute much to the establishment of the mechanical doctrine about qualities, they will, at least, contribute to the natural history of them.

III. I shall not trouble the reader with a recital of those unlucky accidents, that have hindered the subjects of the following book from being more numerous; and I hope he will the more easily excuse their paucity, if he be advertised, that although the particular qualities, about which some experiments and notes, by way of specimens, are here presented, be not near half so many as were intended to be treated of; yet I was careful to choose them such as might comprehend in a small number a great variety; there being scarce one sort of qualities, of which there is not an instance given in this small book, since therein experiments and thoughts are delivered about heat and cold, which are the chief of the four first qualities; about tastes and odours, which are of those, that, being the immediate objects of sense, are wont to be called sensible qualities; about volatility and fixity, corrosiveness and corrosibility, which, as they are found in bodies purely natural, are referable to those qualities, that many physical writers call second qualities, and which yet, as they may be produced and destroyed by the chemists art, may be stiled chemical qualities, and the spagyrical ways of introducing, or expelling them, may be referred to chemical operations, of which there is given a more ample specimen in the mechanical account of chemical precipitations. And lastly, some notes are added about magnetism and electricity, which are known to belong to the tribe of occult qualities.

IV. IF a want of apt coherence, and exact method, be discovered in the following essays, it is hoped, that defect will be easily excused by those, that remember and consider, that these papers were originally little better than a kind of rhapsody of experiments, thoughts, and observations, occasionally thrown together by way of annotations upon some passages of a discourse, (about the differing parts and redintegration of nitre) wherein some things were pointed at, relating to the particular qualities, that are here more largely treated of. And though the particulars, that concern some of these qualities, were afterwards (to supply the place of those borrowed by other papers whilst these lay by me) encreased in number; yet it was not to be expected, that their accession should as well correct the form as augment the matter of our annotations. And as for the two tracts, that are inserted among these essays about qualities; I mean, the discourse of the imperfection of the chemical doctrine of them, and the reflections on the hypothesis of acidum and alcali, the occasion of their being made parts of this book, is so far expressed in the tracts themselves, that I need not here trouble the reader with a particular account of it.

V. I do not undertake, that all the following accounts of particular qualities would prove to be the very true ones, nor every explication the best, that can be devised. For besides that the difficulty of the subject, and incompleatness of the history we yet have of qualities, may well deter a man, less diffident of his own abilities than I justly am, from assuming so much to himself, it is not absolutely necessary to my present design. For, mechanical explications of natural phænomena do give so much more satisfaction to ingenious minds, than those, that must employ substantial forms, sympathy, antipathy, &c. that the more judicious of the vulgar philosophers themselves prefer them before all others, when they can be had; (as is elsewhere shewn at large,) but then they look upon them either as confined to mechanical engines, or at least, but as reaching to very few of nature's phænomena, and, for that reason, unfit to be received as physical principles. To remove therefore this grand prejudice and objection, which seems to be the chief thing, that has kept off rational inquiries from closing with the mechanical philosophy, it may be very conducive, if not sufficient, to propose such mechanical accounts of particular qualities themselves, as are intelligible and possible, and are agreeable to the phænomena whereto they are applied. And to this it is no more necessary, that the account proposed should be the truest and best, that can possibly be given, than it is to the proving, that a clock is not acted by a vital principle, (as those Chineses thought, who took the first, that was brought them out of *Europe*, for an animal,) but acts as an engine, to do more than assign a mechanical structure made up of wheels, a spring, a hammer, and other mechanical pieces, that will regularly shew and strike the hour, whether this contrivance be, or be not, the very same with that of the particular clock proposed; which may indeed be made to move either with springs or weights, and may consist of a greater or lesser number of wheels, and those differingly situated and connected; but for all this variety, it will still be but an engine. I intend not therefore by proposing the theories and conjectures ventured at in the following papers, to debar myself of the liberty either of altering them, or of substituting others in their places, in case a further progress in the history of qualities shall suggest better hypotheses or explications. And it was but agreeable to this intention of mine, that I should, as I have done, on divers occasions in the following notes, employ the word *or*, and express myself somewhat doubtingly, mentioning more than one cause of a phænomenon, or reason of an opinion, without dogmatically declaring for either; since my purpose in these notes was rather to shew, it was not necessary to betake ourselves to the scholastick or chemical doctrine about qualities, than to act the umpire between the differing hypotheses of the Corpuscularians; and, provided I kept myself within the bounds of mechanical philosphy, my design allowed me a great latitude in making explications of the phænomena I had occasion to take notice of.

OF THE MECHANICAL ORIGIN OF HEAT AND COLD.

SECTION I.

About the MECHANICAL PRODUCTION *of* COLD.

HEAT and cold being generally looked upon as the most active among qualities, from which many other qualities are deducible, and by which many of nature's phænomena, especially among the Peripateticks, are attemped to be explicated; I suppose it will be very proper to begin with instances of them to shew, that qualities may be mechanically produced or destroyed. A not useless paraphrase of which expression may be this, that a portion of matter may come to be endowed with a quality, which it had not before, or to be deprived of one, that it had, or sometimes to acquire, or lose a degree of that quality; though on the part of the matter (or, as some would speak, of the patient) there do not appear to intervene any more than a change of texture, or some other mechanical alteration; and though the agents (on their part) do not appear to act upon it otherwise, than after a mechanical manner, that is, by their bigness, shape, motion, and those other attributes, by virtue whereof mechanical powers and engines perform their operations; and this without having recourse to the Peripatetick substantial forms and elements, or the hypostatical principles of the chemists.

AND having here (as in a proper place) to avoid ambiguity, premised once for all this * summary declaration of the sense, agreeably whereunto I would have these terms understood in the following notes about the origin of particular qualities; I proceed now to set down some few examples of the mechanical production of cold and heat, beginning with those, that relate to the former, because, by reason of their paucity, they will be quickly dispatched. And I hope I shall not need to make an apology for mentioning no greater number; since I scarce remember to have met with any instances of this kind in any of the classick writers of natural philosophy.

EXPERIMENT I.

MY first experiment is afforded me by the dissolution of sal armoniac, which I have somewhat wondered, that chemists having often occasion to purify that salt by the help of water, should not have, long since, and publickly, taken notice of. For, if you put into three or four times its weight of water, a pound, or but half a pound (or even less) of powdered sal armoniac, and stir it about to hasten the dissolution, there will be produced in the mixture a very intense degree of coldness, such as will not be only very sensible to his hand, that holds the glass whilst the dissolution is making, but will very manifestly discover itself by its operation upon a thermoscope. Nay, I have more than once, by wetting the outside of the glass, where the dissolution was making, and nimbly stirring the mixture, turned that externally adhering water into real ice, (that was scraped off with a knife) in less than a minute of an hour. And this thus generated cold continued considerably intense, whilst the action of dissolution lasted; but afterwards by degrees abated, and within a very few hours ceased. The particular phænomena I have noted in the experiments, and the practical uses, that may be made of it, I reserve for another place †, the knowledge of them being not necessary in this, where what I have already related, may suffice for my present argument.

AND to shew, that not only a far more intense degree of cold may emerge in this mixture, than was to be found in either of the ingredients before they were mingled, but a considerable coldness may be begun to be produced between bodies, that were neither of them actually cold before they were put together, I will subjoin a transcript of what I find to this purpose among my *adversaria*.

EXPERIMENT II.

[I REMEMBER, that once I had a mind to try, whether the coldness produced upon the solution of beaten sal armoniac in water might not be more probably referred to some change of texture or motion resulting from the action of the liquor upon the salt, than to any infrigidation of the water made by the sudden dispersion of so many saline grains of powder, which, by reason of their solidity, may be suspected to be actually more cold than the water they are put into; I therefore provided a glass

* See more of this in the preamble.

† Divers of the phænomena &c. of this experiment were afterwards printed. Numb. 15. of the *Ph. Transact.*

glaſs full of that liquor, and having brought it to ſuch a temper, that its warmth made the ſpirit of wine in the ſealed weather-glaſs, manifeſtly, though not nimbly, aſcend; I took out the thermoſcope, and laid it in powdered ſal armoniac, warmed beforehand; ſo that the tincted liquor was made to aſcend much nimblier by the ſalt than juſt before by the water; and having preſently removed the inſtrument into that liquor again, and poured the ſomewhat warm ſal armoniac into the ſame, I found, as I imagined, that within a ſpace of time, which I gueſſed to be about half a minute or leſs, the ſpirit of wine began haſtily to ſubſide, and within a few minutes fell above a whole diviſion and a quarter below the mark at which it ſtood in the water, before that liquor or the ſalt were warmed. Nor did the ſpirit in a great while re-aſcend to the height, which it had, when the water was cold.

THE ſame experiment, being at another time reiterated, was tried with the like ſucceſs; which ſecond may therefore ſerve for a confirmation of the firſt.]

EXPERIMENT III.

HAVING a mind likewiſe to ſhew ſome ingenious men, how much the production of heat and cold depends upon texture and other mechanical affections, I thought fit to make again a ſal armoniac by a way I formerly publiſhed, that I might be ſure to know what ingredients I employed, and ſhew their effects, as well before conjunction as after it. I took then ſpirit of ſalt, and ſpirit of fermented, or rather putrified urine; and having put a ſealed weather-glaſs into an open veſſel, where one of them was poured in, I put the other, by degrees, to it, and obſerved, that as, upon their mingling, they made a great noiſe with many bubbles, ſo, in this conflict, they loſt their former coldneſs, and impelled up the ſpirit of wine in the ſealed thermoſcope: Then ſlowly evaporating the ſuperfluous moiſture, I obtained a fine ſort of ſal armoniac, for the moſt part figured not unlike the other, when being diſſolved and filtrated, it is warily coagulated. This new ſalt being gently dried, I put into a wide glaſs of water, wherein I had before placed a ſealed weather-glaſs, that the included ſpirit might acquire the temper of the ambient liquor, and having ſtirred this ſalt in the water, though I took it then off the mantle-tree of a chimney, that had had fire in it divers hours before, it did, as I expected, make the tincted ſpirit haſtily ſubſide, and fall conſiderably low.

EXPERIMENT IV.

SINCE, if two bodies, upon their mixture, acquire a greater degree of cold than either of them had before, there is a production of this additional degree of that quality, it will be proper to add, on this occaſion, the enſuing experiment.

WE took a competent quantity of acid ſpirit diſtilled from roch-allom, (that, though rectified, was but weak,) which, in the ſpirit of that ſalt, is not ſtrange. Of this we put into a wide-mouthed glaſs (that was not great) more than was ſufficient to cover the globulous part of a good ſealed thermoſcope, and then ſuffering the inſtrument to ſtay a pretty while in the liquor, that the ſpirit of wine might be cooled, as much as the ambient was, we put in, little by little, ſome volatile ſalt ſublimed from ſal armoniac and a fixed alcali, and notwithſtanding the very numerous (but not great) bubbles, and the noiſe and froth that were produced, as is uſual upon the re-action of acids and alcalies, the tincted ſpirit in the weather-glaſs, after having continued a good while at a ſtand, began a little to deſcend, and continued (though but very ſlowly) to do ſo, till the ſpirit of allom was glutted with the volatile ſalt; and this deſcent of the tincted liquor in the inſtrument being meaſured, appeared to be about an inch (for it manifeſtly exceeded ſeven eighths.) By comparing this experiment with the firſt part of the foregoing, we may gather, that when volatile and urinous ſalts or ſpirits (for the ſaline particles appear ſometimes in a dry, and ſometimes in a liquid form) tumultuate upon their being mixed with acids, neither the heat nor the cold, that enſues, is produced by a conflict with the acids preciſely as it is acid, ſince we have ſeen, that an urinous ſpirit produced an actual heat with ſpirit of ſalt, and the diſtilled ſalt of ſal armoniac, which is alſo urinous, with the acid ſpirit of roch-allom, produces not a true effervescence, but a manifeſt coldneſs: as the ſame ſalt alſo did in a trial of another ſort, which was this.

EXPERIMENT V.

WE took one part of oil of vitriol, and ſhaking it into twelve parts of water we made a mixture, that at firſt was ſenſibly warm: then ſuffering this to cool, we put a ſufficient quantity of it into a wide mouthed glaſs, and then we put a good thermoſcope hermetically ſealed, above whoſe ball the compounded liquor reached a pretty way. After ſome time had been allowed, that the liquor in the thermometer might acquire the temper of the ambient; we put in, by degrees, as much volatile ſalt of ſal armoniac, as would ſerve to ſatiate the acid ſpirits of the mixture: for, though theſe two made a notable conflict with tumult, noiſe, and froth, yet it was but a cold ebullition (if I may ſo ſtile it,) for the ſpirit in the thermoſcope deſcended about an inch beneath the mark it reſted at, when the ſeeming effervescence began.

EXPERIMENT VI.

IT is known, that ſalt-petre being put into common water produces a ſenſible coldneſs in it, as it alſo does in many other liquors: But that the ſame ſalt put into a liquor of another conſtitution may have a quite differing effect, I have convinced ſome inquiſitive perſons, by mingling eight ounces of fine ſalt-petre, powdered, with ſix ounces of oil of vitriol: For by that commixture with a ſalt, that was not only actually,

actually, but, as to many other bodies, potentially cold, the oil of vitriol, that was sensibly cold before, quickly conceived a considerable degree of heat, whose effects also became visible in the copious fumes, that were emitted by the incalescent mixture.

EXPERIMENT VII.

THIS brings into my mind, that though gunpowder seems to be of so igneous a nature, that, when it is put upon a coal, it is turned presently into flame capable of promoting the deflagration of the charcoal, and kindling divers bodies it meets with in its way; yet if some ounces of gunpowder, reduced to powder, be thrown into four or five times as much water, it will very manifestly impart a coldness to it, as experience made with, as well as without, a sealed thermoscope has assured me.

THIS and the foregoing experiment do readily suggest an enquiry into the nature of the coldness, which philosophers are wont to oppose to that, which immediately, and upon the first contact, affect the organs of sense, and which therefore they call actual or formal.

THE success of this experiment upon a second trial served to confirm it, which is the more strange, because I have found, that a small quantity of oil of vitriol, not beforehand mingled with water, would produce a notable heat in its conflict with a small portion of just such salt as I employed before (both the parcels having been, if I well remember, taken out of the same glass.) And this heat did, upon trial made with the former thermoscope, make the tincted spirit ascend much further than the lately recited experiment made it subside.

A DIGRESSION ABOUT POTENTIAL COLDNESS.

POTENTIAL coldness has been generally looked upon, and that partly perhaps upon the score of its very name, as so abstruse a quality, that it is not only rational, but necessary to derive it from the substantial forms of bodies. But, I confess, I see no necessity of believing it not to be referrible to mechanical principles. For, as to the chief instances of potential coldness, which are taken from the effects of some medicines and aliments in the bodies of men, it may be said, without improbability, that the produced refrigeration proceeds chiefly from this, that the potentially cold body is made up of corpuscles of such size, shape, &c. that, being resolved and disjoined by the menstruum of the stomach, or the fluids it may elsewhere meet with, they do so associate themselves with the small parts of the blood, and other liquors, as, by clogging them, or otherwise, to lessen their wonted agitation, and perhaps make them act in a peculiar way, as well as less briskly on the nervous and fibrous parts; and the perception of this imminution (and perhaps change) of motion in the organs of feeling, is that, which, being referred to the body, that produces it, we call it's potential coldness. Which quality appears by this account to be, as I was saying before, but a relative thing, and is wont to require the diffusion or dispersion of the small parts of the corpuscles of the agent, and their mingling themselves with the liquors, or the small parts of the body they are to refrigerate. And therefore, if it be granted, that, in agues, there is some morbifick matter, of a viscous or not easily dissipable texture, that is harboured in some part of the body, and requires such a time to be made fluid and resolvable, the cold fits of agues need not be so much admired as they usually are; since, though just before the fit the same parcel of matter, that is to produce it, were actually in the body, yet it was not, by reason of its clamminess, actually resolved into small parts, and mingled with those of the blood, and consequently could not make such a change in the motion of that liquor, as is felt in the cold fit of an ague; (for, of the further change, that occasions the hot fit, I am not here to speak.) And in some other diseases, a small quantity of matter, being resolved into minute parts, may be able to produce a great sense of coldness in some part of a body, which, by reason of the structure of that part, may be peculiarly disposed to be affected thereby; as I have known hypochondriac and hysterical women com-

complain of great degrees of coldneſs, that would ſuddenly invade ſome particular part, chiefly of the head or back, and be for a good while troubleſome there. And that, if a frigorific vapour, or matter, be exceeding ſubtile, an inconſiderable quantity of it being diſperſed through the blood, may ſuffice to produce a notable refrigeration, I have learned by enquiry into the effects of ſome poiſons; and it is not very material, whether the poiſon, generally ſpeaking, be cold or hot, if it meet with a body diſpoſed to have thoſe affections, that paſs for cold ones produced in it. For I have made a chemical liquor, that was penetrant and fiery enough to the taſte, and had acquired a ſubtlety and briſkneſs from diſtillation, with which I could, almoſt in a trice, giving it but in the quantity of about a drop, caſt an animal into that, which appeared a ſleep; and the like liquor, in a not much greater quantity, being, by I know not whoſe miſtake, applied to the aching tooth of a very ingenious perſon, did preſently, as he ſoon after told me, give him an univerſal refrigeration, and trembling, worſe than the cold paroxiſm of a quartane. And though ſcorpions do ſometimes cauſe, by their ſting, violent heats in the parts they hurt, yet ſometimes alſo the quite contrary happens, and their poiſon proves, in a high degree, potentially cold; as may be learned from the two following obſervations, recorded by eminent phyſicians. * *Famulum habui* (ſaith *Benivenius*,) *qui à ſcorpione ictus, tam ſubito ac tam frigido ſudore toto corpore perfuſus eſt, ut algentiſſimâ nive atque glacie ſeſe opprimi quereretur. Verùm cùm algenti illi ſolam theriacam ex vino potentiore exhibuiſſem, illicò curatus eſt*: thus far he: to whoſe narrative I add this of *Amatus Luſitanus*.

* *Beniven. cap. 56. Abditorum apud Schenk. lib. 7. de venen. obſerv. 24.*

Cent. 6. obſerv.

VIR *qui à ſcorpione in manus digito punctus fuit, multum dolebat, & refrigeratus totus contremebat, & per corpus dolores, cute totâ quaſi acu punctâ, formicantes patiebatur, &c.*

I cannot now ſtay to enquire, whether there may not be in theſe great refrigerations, made by ſo ſmall a quantity of poiſon, ſome ſmall concretions or coagulations made of the minute particles of the blood into little clots, leſs agile and more unweildy than they were, when they moved ſeparately: which may be illuſtrated by the little curdlings, that may be made of the parts of milk, by a very ſmall proportion of runnet, or ſome acid liquor, and the little coagulations made of the ſpirit of wine by that of urine: nor will I now enquire, whether, beſides the retardment of the motion of the blood, ſome poiſons, and other analogous agents, may not give the motion of it a new modification, (as if ſome corpuſcles, that uſually are more whirled or brandiſhed, be put into a more direct motion,) that may give it a peculiar kind of grating, or other action, upon the nervous and fibrous parts of the body. Theſe, I ſay, and other ſuſpicions, that have ſometimes come into my thoughts, I muſt not ſtay to examine; but ſhall now rather offer to conſideration, whether, ſince ſome parts of the human body are very differing from others in their ſtructure and internal conſtitution; and ſince alſo ſome agents may abound in corpuſcles of differing ſhapes, bulks, and motions, the ſame medicine may not, in reference to the ſame human body, be potentially cold, or potentially hot, according as it is applied; or perhaps may, upon one or both of the accounts newly mentioned, be cold, in reference to one part of the body, and hot, in reference to the other. And theſe effects need not be always aſcribed to the mere and immediate action of the corpuſcles of the medicine, but ſometimes to the new quality they acquire in their paſſage, by aſſociating themſelves with the blood, or other fluids of the body, or to the expulſion of ſome calorific or frigorific corpuſcles, or to the diſpoſition they give the part on which they operate, to be more or leſs permeated and agitated than before, by ſome ſubtile æthereal matter, or other efficients of heat or cold. Some of theſe conjectures about the relative nature of potentially cold bodies may be either confirmed or illuſtrated by ſuch inſtances as theſe; that ſpirit of wine, being inwardly taken, is potentially very hot; and yet, being outwardly applied to ſome burns, and ſome hot tumours, does notably abate the heat of the inflamed parts, though the ſame ſpirit, applied even outwardly to a tender eye, will cauſe a great and dolorous agitation in it. And camphire, which in the doſe of leſs than a half, or perhaps a quarter of a ſcruple, has been obſerved to diffuſe a heat through the body, is, with ſucceſs, externally applied by phyſicians and ſurgeons in refrigerating medicines.

BUT I leave the further inquiry into the operations of medicines to phyſicians, who may poſſibly, by what has been ſaid, be aſſiſted to compoſe the differences between ſome famous writers about the temperament of ſome medicines, as mercury, camphire, &c. which ſome will have to be cold, and others maintain to be hot; and ſhall only offer by way of confirming in general, that potential coldneſs is only a relative quality, a few particulars; the firſt whereof is afforded by comparing together the ſixth and the ſeventh experiment before going, (which have occaſioned this digreſſion about potential coldneſs;) ſince by them it ſeems probable, that the ſame thing may have it in reference to one body, and not to another, according to the diſpoſition of the body it operates upon, or that operates upon it. And the fumes of lead have been obſerved ſometimes (for I have not found the effect to ſucceed always) to arreſt the fluidity of mercury, which change is ſuppoſed to be the effect of a potential coldneſs belonging to the chemiſt's Saturn in reference to fluid mercury, though it have not that operation on any other liquor, that we know of.

AND laſtly, (for I would not be too prolix) though nitre and ſal armoniac be both apart and jointly cold in reference to water, and though, however nitre be thoroughly melted in a crucible, it will not take fire of itſelf, yet if, if, whilst it is in fusion, you should by degrees cast on it some powdered sal armoniac, it will take fire and flash vehemently, almost as if sulphur had been injected.

BUT our excursion has, I fear, lasted too long, and therefore I shall presently re-enter into the way, and proceed to set down some trials about cold.

EXPERIMENT VIII.

IN the first experiment we observed, that upon the pouring of water upon sal armoniac there ensued an intense degree of cold; and we have elsewhere recited, that the like effect was produced by putting, instead of common water, oil of vitriol to sal armoniac: but now, to shew further, what influence motion and texture may have upon such trials, it may not be amiss to add the following experiment: to twelve ounces of sal armoniac we put, by degrees, an equal weight of water, and whilst the liquor was dissolving the salt, and by that action producing a great coldness, we warily poured in twelve ounces also of good oil of vitriol; of which new mixture the event was, that a notable degree of heat was quickly produced in the glass, wherein the ingredients were confounded, as unlikely as it seemed, that, whereas each of the two liquors is wont, with sal armoniac, to produce an intense cold, both of them acting on it together should produce the contrary quality. But the reason I had to expect the success I met with, was this, that it was probable the heat, arising from the mixture of the two liquors, would overpower the coldness produceable by the operation of either, or both of them upon the salt.

EXPERIMENT IX.

IN most of the experiments, that we have hitherto proposed, cold is wont to be regularly produced in a mechanical way; but I shall now add, that in some sort of trials I found, that the event was varied by unobserved circumstances; so that sometimes manifest coldness would be produced by mixing two bodies together, which at another time would upon their congress disclose a manifest heat, and sometimes again, though more rarely, would have but a very faint and remiss degree of either.

OF this sort of experiments, whose events I could not confidently undertake for, I found to be, the dissolution of salt of tartar in spirit of vinegar, and of some other salts, that were not acid, in the same menstruum, and even spirit of verdigrease (made *per se*) though a more potent menstruum than common spirit of vinegar would not constantly produce near such a heat at the beginning of its operation, as the greatness of the seeming effervescence, then excited, would make one expect, as may appear by the following observation transcribed *verbatim* out of one of my *Adversaria*.

[INTO eight ounces of spirit of verdigrease (into which we had put a while before a standard-thermoscope, to acquire the like temper with the liquor) we put in a wide-mouthed glass two ounces of salt of tartar, as fast as we durst for fear of making the matter boil over; and though there were a great commotion excited by the action and reaction of the ingredients, which was attended with a copious froth and a hissing noise; yet it was a pretty while, ere the glass was sensibly warm on the outside; but by that time the salt was all dissolved, the liquor in the thermoscope appeared to be impelled up about three inches and a half.

AND yet, if my memory do not much deceive me, I have found, that by mixing salt of tartar with another salt, the texture of the fixed alkali was so altered, that upon the affusion of spirit of verdigrease, (made without spirit of vinegar and spirit of wine) though there ensued a great conflict with noise and bubbles, yet, instead of an incalescence, a considerable degree of coldness was produced.

EXPERIMENT X.

IT is very probable, that further trials will furnish us with more instances, to shew how the production of cold may, in some cases, be effected, varied, or hindered by mechanical circumstances, that are easily and usually overlooked. I remember, on this occasion, that though, in the experiment above recited, we observed, that oil of vitriol and water being first shaken together, the volatile salt of sal armoniac being afterwards put to them, produced a sensible coldness; yet I found, that if a little oil of vitriol, and of the volatile salt, were first put together, though soon after a considerable proportion of water were added, there would be produced, not a coldness, but a manifest degree of heat, which would impel up the liquor in the thermoscope to the height of some inches. And I remember too, that though salt of tartar will, as we shall see ere long, grow hot in the water, yet having distilled some salt of tartar and cinnabar in a strong fire, and put the whole *Caput mortuum* into distilled or rain water, it made indeed a hissing there, as if it had been quick lime, but produced no heat, that I could by feeling perceive. I shall add, that not only, as we have seen already, some unheeded circumstances may promote or hinder the artificial production of cold by particular agents, but, which will seem more strange, some unobserved, and perhaps hardly observable, indisposition in the patient, may promote or hinder the effects of the grand and catholick efficients of cold, whatever those be. This suspicion I represent as a thing, that further experience may possibly countenance, because I have sometimes found, that the degree of the operation of cold has been much varied by latent circumstances, some bodies being more wrought upon, and others less, than was, upon very probable grounds, expected. And particularly I remember, that though oil of vitriol be one of the firiest liquors, that is yet known, and does perform some of the operations of fire itself, (as we we ſhall elſewhere have occaſion to ſhew) and will thaw ice ſooner than ſpirit of wine, or any other liquor, as I have tried; yet having put about a pound or more, by our eſtimate, of choice rectified oil of vitriol, into a ſtrong glaſs vial proportionable to it, we found, that, except a little, that was fluid at the top, it was all congealed or coagulated into a maſs like ice, though the glaſs ſtood in a laboratory, where a fire was conſtantly kept not far from it, and where oil of vitriol very ſeldom, or never, has before, or ſince, been obſerved to congeal or coagulate ſo much as in part. And the oddneſs of our phænomenon was encreaſed by this circumſtance, that the maſs continued ſolid a good while after the weather was grown too mild to have ſuch operations upon liquors far leſs indiſpoſed to loſe their fluidity by cold, than even common oil of vitriol is. On the other ſide I remember, that about two years ago, I expoſed ſome oil of ſweet almonds hermetically ſealed up in a glaſs bubble, to obſerve what condenſation an intenſe cold could make of it, (for though cold expands water, (it condenſes common oil;) but the next day I found, to my wonder, that not only the oil remained unfrozen by the ſharp froſt it had been expoſed to, but that it had not its tranſparency troubled, though it is known, that oil will be brought to concrete, and turn opacous by a far leſs degree of cold than is requiſite to freeze water; notwithſtanding which, this liquor, which was lodged in a glaſs, ſo thin, that it was blown at the flame of a lamp, continued fluid and diaphanous in very froſty weather, ſo long till I loſt the expectation of ſeeing it congealed or concreted. And this brings into my mind, that though camphire be, as I formerly noted, reckoned by many potentially cold, yet we kept ſome oil of it, of our making, wherein the whole body of the camphire remained, being only by ſome nitrous ſpirits reduced to the form of an oil; we kept it, I ſay, in ſuch intenſe degrees of cold, that would have eaſily frozen water, without finding it to loſe its tranſparency, or its fluidity.

And here I ſhall put an end to the firſt ſection, (containing our notes about cold) the deſign of which may be not a little promoted by comparing with them the beginning of the enſuing ſection. For if it be true, that (as we there ſhew) the nature of heat conſiſts either only or chiefly in the local motion of the ſmall parts of a body mechanically modified by certain conditions, of which the principal is the vehemency of the various agitations of thoſe inſenſible parts; and if it be alſo true, as experience witneſſes it to be, that, when the minute parts of a body are in, or arrive at ſuch a ſtate, that they are more ſlowly or faintly agitated than thoſe of our fingers, or other organs of feeling, we judge them cold: theſe two things, laid together, ſeem plainly enough to argue, that a privation or negation of that local motion, that is requiſite to conſtitute heat, may ſuffice for the denominating a body cold, as coldneſs is a quality of the object, (which, as it is perceived by the mind, is alſo an affection of the ſentient:) and therefore an imminution of ſuch a degree of former motion, as is neceſſary to make a body hot as to ſenſe, and which is ſufficient to the production of ſenſible coldneſs, may be mechanically made, ſince ſlowneſs, as well as ſwiftneſs, being a mode of local motion, is a mechanical thing. And though its effect, which is coldneſs, ſeem a privation or negation; yet the cauſe of it may be a poſitive agent acting mechanically, by clogging the agile calorific particles, or deadning their motion, or perverting their determination, or by ſome other intelligible way bringing them to a ſtate of coldneſs, as to ſenſe: I ſay, coldneſs as to ſenſe; becauſe as it is a tactile quality, in the popular acception of it, it is relative to our organs of feeling; as we ſee, that the ſame luke-warm water will appear hot and cold to the ſame man's hands, if, when both are plunged into it, one of them ſhall have been newly held to the fire, and the other be benumbed with froſt. And indeed the cuſtom of ſpeaking has introduced an ambiguity into the word cold, which often occaſions miſtakes, not eaſily, without much attention, and ſometimes circumlocution alſo, to be avoided; ſince uſually by cold is meant that, which immediately affects the ſenſory of him, that pronounces a body cold, whereas ſometimes it is taken in a more general notion for ſuch a negation or imminution of motion, as though it operates not perceivably on our ſenſes, does yet upon other bodies; and ſometimes alſo it is taken (which is perhaps the more philoſophical ſenſe) for a perception, made in and by the mind, of the alteration produced in the corporeal organs by the operation of that, whatever it be, on whoſe account a body is found to be cold.

But the diſcuſſion of theſe points is here purpoſely omitted, as for other reaſons, ſo principally, becauſe they may be found expreſsly handled in a fitter place.

SECTION II.

Of the MECHANICAL ORIGIN, or PRODUCTION of HEAT.

After having diſpatched the inſtances I had to offer of the production of cold, it remains, that I alſo propoſe ſome experiments of heat, which quality will appear the more likely to be mechanically producible, if we conſider the nature of it, which ſeems to conſiſt mainly, if not only, in that mechanical affection of matter we call local motion mechanically modified, which modification, as far as I have obſerved, is made up of three conditions.

The firſt of theſe is, that the agitation of the parts be vehement, by which degree of rapidneſs the motion proper to bodies, that are hot, diſtinguiſhes them from bodies, that are barely fluid. For theſe, as ſuch, require not near so brisk an agitation, as is wont to be necessary to make bodies deserve the name of hot. Thus we see, that the particles of water, in its natural (or usual) state, move so calmly, that we do not feel it at all warm, though it could not be a liquor, unless they were in a restless motion; but when water comes to be actually hot, the motion does manifestly and proportionably appear more vehement, since it does not only briskly strike our organs of feeling, but ordinarily produces store of very small bubbles, and will melt butter or coagulated oil cast upon it, and will afford vapours, that, by the agitation they suffer, will be made to ascend into the air. And if the degree of heat be such, as to make the water boil, then the agitation becomes much more manifest by the confused motions, and waves, and noise, and bubbles, that are excited, and by other obvious effects, and phænomena of the vehement and tumultous motion, which is able to throw up visibly into the air great store of corpuscles, in the form of vapours or smoke. Thus, in a heated iron, the vehement agitation of the parts may be easily inferred, from the motion and hissing noise it imparts to drops of water, or spittle, that fall upon it. For it makes them hiss and boil, and quickly forces their particles to quit the form of a liquor, and fly into the air in the form of steams. And, lastly, fire, which is the hottest body we know, consists of parts so vehemently agitated, that they perpetually and swiftly fly abroad in swarms, and dissipate or shatter all the combustible bodies they meet with in their way; fire making so fierce a dissolution, and great a dispersion of its own fuel, that we may see whole piles of solid wood (weighing perhaps many hundred pounds) so dissipated, in very few hours, into flame and smoke, that, oftentimes, there will not be one pound of ashes remaining. And this is the first condition required to heat.

THE second is this, that the determinations be very various, some particles moving towards the right, some to the left hand, some directly upwards, some downwards, and some obliquely, &c. This variety of determinations appears to be in hot bodies, both by some of the instances newly mentioned, and especially that of flame, which is a body; and by the diffusion, that metals acquire, when they are melted, and by the operations of heat, that are exercised by hot bodies upon others, in what posture or situation soever the body to be heated be applied to them. As a thoroughly ignited coal will appear every way red, and will melt wax, and kindle brimstone, whether the body be applied to the upper or to the lower, or to any other part of the burning coal. And congruously to this notion, though air and water be moved never so vehemently, as in high winds and cataracts; yet we are not to expect, that they should be manifestly hot, because the vehemency belongs to the progressive motion of the whole body; notwithstanding which, the parts it consists of may not be near so much quickened in their motions, made according to other determinations, as to become sensibly hot. And this consideration may keep it from seeming strange, that, in some cases, where the whole body, though rapidly moved, tends but one way, it is not by that swift motion perceived to be made hot.

NAY, though the agitation be very various, as well as vehement, there is yet a third condition required to make it calorific; namely, that the agitated particles, or at least the greatest number of them, be so minute, as to be singly insensible. For though a heap of sand, or dust itself, were vehemently and confusedly agitated by a whirl-wind, the bulk of the grains or corpuscles, would keep their agitation from being properly heat, though, by their numerous strokes upon a man's face, and the brisk commotion of the spirits, and other small particles, that may thence ensue, they may perchance occasion the production of that quality.

IF some attention be employed, in considering the formerly proposed notion of the nature of heat, it may not be difficult to discern, that the mechanical production of it may be divers ways effected. For, excepting in some few anomalous cases, (wherein the regular course of things happens to be over-ruled,) by whatever ways the insensible parts of a body are put into a very confused and vehement agitation, by the same ways heat may be introduced into that body: agreeably to which doctrine, as there are several agents and operations, by which this calorific motion (if I may so call it) may be excited, so there may be several ways of mechanically producing heat, and many experiments may be reduced to almost each of them, chance itself having, in the laboratories of chemists, afforded divers phænomena, referable to any one or other of those heads. Many of the more familiar instances, applicable to our present purpose, have been long since collected by our justly-famous *Verulam*, in his short, but excellent paper *de forma calidi*, wherein (though I do not acquiesce in every thing I meet with there) he seems to have been, at least among the moderns, the person, that has first handled the doctrine of heat like an experimental philosopher. I shall therefore decline accumulating a multitude of instances of the production of heat, and I shall also forbear to insist on such known things, as the incalescence, observable upon the pouring either of oil of vitriol upon salt of tartar, (in the making of tartarum vitriolatum) or of aqua fortis upon silver or quicksilver, (in the dissolution of these metals,) but shall rather chuse to mention some few instances not so notorious as the former, but not so unfit, by their variety, to exemplify several of the differing ways of exciting heat.

AND yet I shall not decline the mention of the most obvious and familiar instance of all, namely, the heat observed in quick-lime, upon the affusion of cold water, because, among learned men, and especially Peripateticks, I find causes to be assigned, that are either justly questionable, or manifestly erroneous. For, as to what is inculcated by the schools, about the incalescence of a mixture of quick-lime and water, by virtue of a supposed Antiperistasis, or invigoration of the internal heat of the lime, by its being invironed by cold water, I have else-

elsewhere shewn, that this is but an imaginary cause, by delivering, upon experiment, (which any man may easily make,) that if, instead of cold water, the liquor be poured on very hot, the ebullition of the lime will not be the less, but rather the greater: and oil of turpentine, which is a lighter, and is looked upon as a subtiler liquor than water, though it be poured quite cold on quick-lime, will not, that I have observed, grow so much as sensibly hot with it.

AND now I have mentioned the incalescence of lime, which, though an obvious phænomenon, has exercised the wits of divers philosophers and chemists, I will add two or three observations, in order to an enquiry, that may be some other time made into the genuine causes of it; which are not so easy to be found, as many learned men may, at first sight, imagine. The acute *Helmont* indeed, and his followers, have ingeniously enough attempted to derive the heat under consideration from the conflict of some alcalizate and acid salts, that are to be found in quick-lime, and are dissolved, and so set at liberty, to fight with one another by the water that slakes the lime. But, though we have some manifest marks of an alcalizate salt in lime, yet, that it contains also an acid salt, has not, that I remember, been proved; and if the emerging of heat be a sufficient reason to prove a latent acid salt in lime, I know not, why I may not infer, that the like salt lies concealed in other bodies, which the chemists take to be of the purest or merest sort of alcalies.

EXPERIMENT I.

FOR I have purposely tried, that by putting a pretty quantity of dry salt of tartar in the palm of my hand, and wetting it well in cold water, there has been a very sensible heat produced in the mixture; and when I have made the trial with a more considerable quantity of salt and water in a vial, the heat proved troublesomely intense, and continued to be at least sensible a good while after.

THIS experiment seems to favour the opinion, that the heat produced in lime, whilst it is quenching, proceeds from the empyreuma, as the chemists call it, or impression left by the violent fire, that was employed to reduce the stone to lime. But if by empyreuma be meant a bare impression made by the fire, it will be more requisite than easy, to declare intelligibly, in what that impression consists, and how it operates to produce such considerable effects. And if the effect be ascribed to swarms of atoms of fire, that remain adherent to the substance of the lime, and are set at liberty to fly away by the liquor, which seems to be argued by the slaking of lime without water, if it be for some time left in the air, whereby the atoms of fire get opportunity to fly away by little and little: if this, I say, be alledged, I will not deny, but there may be a sense, which I cannot explicate in few words, wherein the cooperation of a substantial effluvium, (for so I call it,) of the fire, may be admitted in giving an account of our phænomenon. But the cause formerly assigned, as it is crudely proposed, leaves in my mind some scruples. For it is not so easy to apprehend, that such light and minute bodies, as those of fire, are supposed, should be so long detained, as by this hypothesis they must be allowed to be, in quick-lime, kept in well-stopped vessels, from getting out of so lax and porous a body as lime, especially since we see not a great incalescence or ebullition ensue upon the pouring of water upon minium, or *crocus Martis per se*, though they have been calcined by violent and lasting fires, whose effluviums or emanations appear to adhere to them by the increase of weight, that lead, if not also *Mars*, does manifestly receive from the operation of the fire. To which I shall add, that, whereas one would think, that the igneous atoms should either fly away, or be extinguished by the supervening of water, I know, and elsewhere give account, of an

EXPERIMENT II.

IN which two liquors, whereof one was furnished me by nature, did by being several times separated and reconjoined without additament, at each congress produce a sensible heat.

EXPERIMENT III.

AND an instance of this kind, though not so odd, I purposely sought and found in salt of tartar, from which, after it had been once heated by the affusion of water, we abstracted or evaporated the liquor, without violence of fire, till the salt was again dry; and then putting on water a second time, the same salt grew hot again in the vial, and, if I misremember not, it produced this incalescence the third time, if not the fourth; and might probably have done it oftener, if I had had occasion to prosecute the experiment. Which seems at least to argue, that the great violence of fire is not necessary to impress what passes for an empyreum upon all calcined bodies, that will heat with water.

AND on this occasion I shall venture to add, that I have sometimes doubted, whether the incalescence may not much depend upon the particular disposition of the calcined body, which being deprived of its former moisture, and made more porous by the fire, doth by the help of those igneous effluviums, for the most part of a saline nature, that are dispersed through it, and adhere to it, acquire such a texture, that the water impelled by its own weight, and the pressure of the atmosphere, is able to get into a multitude of its pores at once, and suddenly dissolve the igneous and alcalizate salt it every where meets with there, and briskly disjoin the earthy and solid particles, that were blended with them; which being exceeding numerous, though each of them perhaps be very minute, and moves but a very little way, yet their multitude makes the confused agitation of the whole aggregate of them, and of the particles of the water and salt vehement enough to produce a sensible heat; especially if we admit, that there is such a change made in in the pores, as occasions a great increase of this agitation, by the ingress and action of some subtile ethereal matter, from which alone Monsieur *Des Cartes* ingeniously attempts to derive the incalescence of lime and water, as well as that of metals dissolved in corrosive liquors; though as to the phænomena we have been considering, there seems at least to concur a peculiar disposition of body, wherein heat is to be produced to do one or both of these two things, namely, to retain good store of the igneous effluvia, and to be, by their adhesion or some other operation of the fire, reduced to such a texture of its component particles, as to be fit to have them easily penetrated, and briskly, as well as copiously, dissipated, by invading water. And this conjecture (for I propose it as no other) seems favoured by divers phænomena, some whereof I shall now annex. For here it may be observed, that both the dissolved salt of tartar lately mentioned, and the artificial liquor, that grows hot with the natural, re-acquires that disposition to incalescence upon abare constipation, or closer texture of the parts from the superfluous moisture they were drowned in before; the heat, that brought them to this texture, having been so gentle, that it is no way likely, that the igneous exhalations could themselves produce such a heat, or at least, that they should adhere in such numbers, as must be requisite to such an effect, unless the texture of the salt of tartar, or other body, did peculiarly dispose it to detain them; since

EXPERIMENT IV.

I HAVE found by trial, that sal armoniac dissolved in water, though boiled up with a brisker fire to a dry salt, would, upon its being again dissolved in water, not produce any heat, but a very considerable degree of cold. I shall add, that though one would expect a great cognation between the particles of fire adhering to quick-lime, and those of high rectified spirit of wine, which is of so igneous a nature, as to be totally inflammable; yet I have not found, that the affusion of alkaol of wine upon quick-lime would produce any sensible incalescence, or any visible dissolution or dissipation of the lime, as common water would have done, though it seemed to be greedily enough soaked in by the lumps of lime. And I further tried, that, if on this lime so drenched I poured cold water, there ensued no manifest heat, nor did I so much as find the lump swelled, and thereby broken, till some hours after; which seems to argue, that the texture of the lime was such, as to admit the particles of the spirit of wine into some of its pores, which were either larger or more congruous, without admitting it into the most numerous ones, whereinto the liquor must be received, to be able suddenly to dissipate the corpuscles of lime into their minuter particles, into which (corpuscles) it seems, that the change, that the aqueous particles received by associating with the spirituous ones, made them far less fit to penetrate and move briskly there, than if they had entered alone.

I made also an experiment, that seems to favour our conjecture, by shewing, how much the disposition of lime to incalescence may depend upon an idoneous texture, and the experiment, as I find it, registered in one of my memorials, is this.

EXPERIMENT V.

[UPON quick-lime we put in a retort as much moderately strong spirit of wine, as would drench it, and swim a pretty way above it; and then distilling with a gentle fire, we drew off some spirit of wine much stronger than that, which had been put on, and then the phlegm following it, the fire was encreased, which brought over a good deal of phlegmatick strengthless liquor; by which one would have thought, that the quick-lime had been slacked; but when the remaining matter had been taken out of the retort, and suffered to cool, it appeared to have a fiery disposition, that it had not before. For, if any lump of it, as big as a nutmeg, or an almond, was cast into the water, it would hiss as if a coal of fire had been plunged into the liquor, which was soon thereby sensibly heated. Nay, having kept divers lumps of this prepared calx well covered from the air for divers weeks, to try, whether it would retain this property, I found, as I expected, that the calx operated after the same manner, if not more powerfully. For sometimes, especially when it was reduced to small pieces, it would upon its coming into the water make such a brisk noise, as might almost pass for a kind of explosion.]

THESE phænomena seem to argue, that the disposition, that lime has to grow hot with water, depends much on some peculiar texture, since the aqueous parts, that one would think capable of quenching all, or most of the atoms of fire, that are supposed to adhere to quick-lime, did not near so much weaken the disposition of it to incalescence, as the accession of the spirituous corpuscles and their contexture, with those of the lime, encreased that igneous disposition. And that there might intervene such an association, seems to me the more probable, not only because much of the distilled liquor was as phlegmatick, as if it had been robbed of its more active parts, but because I have sometimes had spirit of wine come over with quick-lime not in unobserved steams, but white fumes. To which I shall add, that besides, that the taste, and perhaps odour of the spirit of wine, is often manifestly changed by a well-made distillation from quick-lime; I have sometimes found that liquor to give the lime a kind of alcalizate penetrancy, not to say fieriness of taste, that was very brisk and remarkable. But I will not undertake, that every experimenter, nor I myself, shall always make trials of this kind with the same success, that I had in those above recited, in regard, that I have found quick-limes to differ much, not only according to the degree of their calcination,

cination, and to their recentneſs, but alſo, and that eſpecially, according to the differing natures of the ſtones and other bodies calcined. Which obſervation engages me the more to propoſe what hath been hitherto delivered about quick-lime, as only narratives and a conjecture; which I now perceive has detained us ſo long, that I am obliged to haſten to the remaining experiments, and to be the more ſuccinct in delivering them.

EXPERIMENT VI.

AND it will be convenient to begin with an inſtance or two of the production of heat, wherein there appears not to intervene any thing in the part of the agent or patient, but local motion, and the natural effects of it. And as to this ſort of experiments, a little attention and reflection may make ſome familiar phænomenon appoſite to our preſent purpoſe. When, for example, a ſmith does haſtily hammer a nail, or ſuch like piece of iron, the hammered metal will grow exceeding hot, and yet there appears not any thing to make it ſo, ſave the forcible motion of the hammer, which impreſſes a vehement, and variouſly determined agitation of the ſmall parts of the iron; which being a cold body before, by that ſuperinduced commotion of its ſmall parts, becomes in divers ſenſes hot; firſt, in a more lax acceptation of the word in reference to ſome other bodies, in reſpect of whom it was cold before, and then ſenſibly hot; becauſe this newly gained agitation, ſurpaſſes that of the parts of our fingers. And in this inſtance, it is not to be overlooked, that oftentimes neither the hammer, by which, nor the anvil, on which a cold piece of iron is forged, (for all iron does not require precedent ignition to make it obey the hammer) continue cold, after the operation is ended; which ſhews, that the heat acquired by the forged piece of iron was not communicated by the hammer or anvil as heat, but produced in it by motion, which was great enough to put ſo ſmall a body, as the piece of iron, into a ſtrong and confuſed motion of its parts, without being able to have the like operation upon ſo much greater maſſes of metal, as the hammer and the anvil; though, if the percuſſions were often and nimbly renewed, and the hammer were but ſmall, this alſo might be heated, (though not ſo ſoon, nor ſo much, as the iron;) by which one may alſo take notice, that it is not neceſſary, a body ſhould be itſelf hot, to be calorifick. And now I ſpeak of ſtriking an iron with a hammer, I am put in mind of an obſervation, that ſeems to contradict, but does indeed confirm our theory: namely, that if a ſomewhat large nail be driven by a hammer into a plank, or piece of wood, it will receive divers ſtrokes on the head before it grow hot; but when it is driven to the head, ſo that it can go no further, a few ſtrokes will ſuffice to give it a conſiderable heat; for whilſt, at every blow of the hammer, the nail enters further and further into the wood, the motion, that is produced, is chiefly progreſſive, and is of the whole nail tending one way; whereas, when that motion is ſtopped, then the impulſe given by the ſtroke, being unable either to drive the nail further on, or deſtroy its intireneſs, muſt be ſpent in making a various vehement and inteſtine commotion of the parts among themſelves, and in ſuch an one we formerly obſerved the nature of heat to conſiſt.

EXPERIMENT VII.

IN the foregoing experiment, the briſk agitation of the parts of a heated iron was made ſenſible to the touch. I ſhall now add one of the attempts, that I remember I made, to render it diſcoverable to the eye itſelf. In order to this, and that I might alſo ſhew, that not only a ſenſible, but an intenſe degree of heat, may be produced in a piece of cold iron by local motion, I cauſed a bar of that metal to be nimbly hammered by two or three luſty men, accuſtomed to manage that inſtrument; and theſe ſtriking with as much force, and as little intermiſſion, as they could, upon the iron, ſoon brought it to that degree of heat, that not only it was a great deal too hot to be ſafely touched, but probably would, according to my deſign, have kindled gun-powder, if that, which I was fain to make uſe of, had been of the beſt ſort: for, to the wonder of the by-ſtanders, the iron kindled the ſulphur of many of the grains of the corns of powder, and made them turn blue, though I do not well remember, that it made any of them go off.

EXPERIMENT VIII.

BESIDES the effects of manifeſt and violent percuſſions, ſuch as thoſe we have been taking notice of to be made with a hammer, there are among phænomena obvious enough, ſome, that ſhew the producibleneſs of heat, even in cold iron, by cauſing an inteſtine commotion of its parts: for we find, that, if a piece of iron, of a convenient ſhape and bulk, be nimbly filed with a large rough file, a conſiderable degree of heat will be quickly excited in thoſe parts of the iron where the file paſſes to and fro, the many prominent parts of the inſtrument giving a multitude of ſtrokes or puſhes to the parts of the iron, that happen to ſtand in their way, and thereby making them put the neighbouring parts into a briſk and confuſed motion, and ſo into a ſtate of heat. Nor can it be well objected, that, upon this account, the file itſelf ought to grow as hot as the iron, which yet it will not do; ſince, to omit other anſwers, the whole body of the file being moved to and fro, the ſame parts, that touch the iron this moment, paſs off the next; and, beſides, have leiſure to cool themſelves, by communicating their newly received agitation to the air, before they are brought to grate again upon the iron, which, being ſuppoſed to be held immoveable, receives almoſt perpetual ſhakes in the ſame place.

WE find alſo, that attrition, if it be any thing vehement, is wont to produce heat in the ſolideſt bodies; as when the blade of a knife,

knife, being nimbly whetted, grows presently hot. And if, having taken a brass nail, and driven it as far as you can to the end of the stick, to keep it fast, and gain a handle, you then strongly rub the head to and fro against the floor, or a plank of wood, you may quickly find it to have acquired a heat intense enough to offend, if not burn one's fingers. And I remember, that going once, in exceeding hot weather, in a coach, which, for certain reasons, we caused to be driven very fast; the attrition of the nave of the wheel, against the axel-tree, was so vehement, as obliged us to light out of the coach, to seek for water to cool the over-chafed parts, and stop the growing mischief the excessive heat had begun to do.

THE vulgar experiment, of striking fire with a flint and steel, sufficiently declares, what a heat, in a trice, may be produced in cold bodies by percussion, or collision; the latter of which seems but mutual percussion.

BUT instances of the same sort, with the rest-mentioned in this VI. experiment, being obvious enough, I shall forbear to multiply and insist on them.

EXPERIMENT IX.

FOR the sake of those, that think the attrition of contiguous air is necessary to the production of manifest heat, I thought, among other things, of the following experiment, and made trial of it.

WE took some hard black pitch, and having, in a bason, porringer, or some such vessel, placed it a convenient distance under water, we cast on it, with a good burning-glass, the sun-beams, in such a manner, that, notwithstanding the refraction, that they suffered in the passage through the interposed water, the focus fell upon the pitch; wherein it would produce sometimes bubbles, sometimes smoke, and quickly communicated a degree of heat capable to make pitch melt, if not also to boil.

EXPERIMENT X.

THOUGH the first and second experiments of Section I. shew, that a considerable degree of cold is produced by the dissolution of sal armoniac in common water; yet, by an additament, though but single, the texture of it may be so altered, that, instead of cold, a notable degree of heat will be produced, if it be dissolved in that liquor. For the manifestation of which, we devised the following experiment.

WE took quick-lime, and slaked it in common cold water, that all the igneous, or other particles, to which its power of heating that liquor is ascribed, might be extracted and imbibed, and so the calx freed from them; then, on the remaining powder, fresh water was often poured, that all adhering reliques of salt might be washed off. After this, the thus dulcified calx, being again well dried, was mingled with an equal weight of powdered sal armoniac, and having, with a strong fire, melted the mass, the mixture was poured out; and, being afterwards beaten to powder, having given it a competent time to grow cold, we put two or three ounces of it into a wide-mouthed glass; and pouring water upon it, within about a minute of an hour, the mixture grew warm, and quickly attained so intense a heat, that I could not hold the glass in my hand. And though this heat did not long last at the same height, it continued to be very sensible for a considerable time after.

EXPERIMENT XI.

TO confirm this experiment, by a notable variation, we took finely powdered sal armoniac, and filings or scales of steel, and when they were very diligently mixed, (for that circumstance ought to be observed,) we caused them to be gradually sublimed in a glass vessel, giving a smart fire towards the latter end. By this operation, so little of the mixture ascended, that, as we desired, far the greatest part of the sal armoniac staid at the bottom with the metal; then, taking out the caput mortuum, I gave it time thoroughly to cool, but in a glass well stopped, that it might not imbibe the moisture of the air, (as it is very apt to do.) And lastly, though the filings of steel, as well as the sal armoniac, were bodies actually cold, and so might be thought likely to increase, not check the coldness wont to be produced in water by that salt; yet, putting the mixture into common water, there ensued, as we expected, an intense degree of heat. And I remember, that, having sublimed the forementioned salt in distinct vessels, with the filings of steel, and with filings of copper, and, for curiosity-sake, kept one of the caput mortuums (for I cannot certainly call to mind which of the two it was) divers months, (if I mistake not, eight or nine,) we at length took it out of the vessel, wherein it had been kept carefully stopped; and, upon trial, were not deceived in having expected, that all that while the disposition to give cold water a notable degree of heat, was preserved in it.

EXPERIMENT XII.

IF experiments were made after the above recited manner with sal armoniac and other mineral bodies than iron and copper, it is not improbable, that some of the emerging phænomena would be found to confirm what has been said of the interest of texture, (and some few other mechanical affections) in the production of heat and cold. Which conjecture is somewhat favoured by the following trial. Three ounces of antimony, and an equal weight of sal armoniac being diligently powdered and mixed, were, by degrees of fire, sublimed in a glass vessel, by which operation we obtained three differing substances, which we caused to be separately powdered, when they were taken out of the subliming glass, lest the air or time should make any change in them; and having before put the ball of a good sealed weather-glass for a while into water, that the spirit of wine might be brought to the temper of the external

external liquor, we put on a convenient quantity of the powdered Caput mortuum, which amounted to two ounces, and seemed to be little other than antimony, which accordingly did scarce sensibly raise the spirit of wine in the thermoscope, though that were a tender one. Then laying aside that water, and putting the instrument into fresh, of the same temper, we put to it a very yellow sublimate, that ascended higher than the other parts, and seemed to consist of the more sulphureous flowers of the antimony, with a mixture of the more volatile parts of the sal armoniac. And this substance made the tincted spirit in the thermoscope descend very slowly about a quarter of an inch; but when the instrument was put into fresh water of the same temper, and we had put in some of the powder of the lower sort of sublimate, which was dark coloured, though both the antimony and sal armoniac, it consisted of, had been long exposed to the action of a subliming heat; yet the water was thereby speedily and notably cooled, insomuch, that the spirit of wine in the weather-glass hastily descended, and continued to sink, till, by our guess, it had fallen not much short of three inches. Of these phænomena the ætiology, as some moderns call the theory, which proposes the causes of things, is more easy to be found by a little consideration, than to be made out in few words.

We made also an experiment like that above recited, by subliming three ounces a piece of minium and sal armoniac; in which trial we found, that though, in the Caput mortuum, the salt had notably wrought upon the calx of lead, and was in part associated with it, as appeared by the whiteness of the said Caput mortuum, by its sweetish taste, and by the weight (which exceeded, four drachms, that of all the minium;) yet a convenient quantity of this powdered mixture being put into water, wherein the former weather-glass had been kept a while, the tincted spirit of wine was not manifestly either raised or depressed. And when, in another glass, we prosecuted the trial with the sal armoniac, that had been sublimed from the minium, it did indeed make the spirit of wine descend, but scarce a quarter so much as it had been made to fall by the lately mentioned sublimate of sal armoniac and antimony.

EXPERIMENT XIII.

It is known, that many learned men, besides several chemical writers, ascribe the incalescences, that are met with in the dissolution of metals, to a conflict arising from a certain antipathy or hostility, which they suppose between the conflicting bodies, and particularly between the acid salt of the one, and the alcalizate salt, whether fixed or volatile, of the other. But since this doctrine supposes a hatred between inanimate bodies, in which it is hard to conceive, how there can be any true passions, and does not intelligibly declare, by what means their supposed hostility produces heat; it is not likely, that, for these and some other reasons, inquisitive naturalists will easily acquiesce in it. And on the other side it may be considered, whether it be not more probable, that heats, suddenly produced in mixtures, proceed either from a very quick and copious diffusion of the parts of one body through those of another, whereby both are confusedly tumbled and put into a calorific motion; or from this, that the parts of the dissolved body come to be every way, in great numbers, violently scattered; or from the fierce and confused shocks or justlings of the corpuscles of the conflicting bodies, or masses, which may be supposed to have the motions of their parts differingly modified according to their respective natures: or from this, that, by the plentiful ingress of the corpuscles of the one into the almost commensurate parts of the other, the motion of some etherial matter, that was wont before swiftly to permeate the distinct bodies, comes to be checked and disturbed, and forced to either brandish or whirl about the parts in a confused manner, till it have settled itself a free passage through the new mixture, almost as the light does through divers troubled liquors and vitrified bodies, which, at length, it makes transparent. But, without here engaging in a solemn examination of the hypothesis of alcali and acidum, and without determining whether any one, or more, of the newly mentioned mechanical causes, or whether some other, that I have not yet named, is to be entitled to the effect; it will not be impertinent to propose divers instances of the production of heat by the operation of one agent, oil of vitriol, that it may be considered whether it be likely, that this single agent should, upon the score of antipathy, or that of its being an acid menstruum, be able to produce an intense heat in many bodies of so differing natures as are some of those, that we shall have occasion to name. And now I proceed to the experiments themselves.

Take some ounces of strong oil of vitriol, and shaking it with three or four times its weight of common water, though both the liquors were cold, when they were put together, yet their mixture will, in a trice, grow intensely hot, and continue considerably so for a good while. In this case it cannot probably be pretended by the chemists, that the heat arises from the conflict of the acid and alcalizate salts abounding in the two liquors, since the common water is supposed an elementary body devoid of all salts; and at least, being an insipid liquor, it will scarce be thought to have alcali enough to produce, by its re-action so intense a heat. That the heat emergent upon such a mixture may be very great, when the quantities of the mingled liquors are considerably so, may be easily concluded from one of my memorials, wherein I find, that no more than two ounces of oil of vitriol being poured (but not all at once) into four ounces only of distilled rain-water, made and kept it manifestly warm for a pretty deal above an hour, and during no small part of that time, kept it so hot, that it was troublesome to be handled.

EXPERIMENT XIV.

THE former experiment brings into my mind one, that I mention, without teaching it in the history of cold, and it appeared very surprising to those, that knew not the ground of it. For having sometimes merrily proposed to heat cold liquors with ice, the undertaking seemed extravagant, if not impossible, but was easily performed by taking out of a bason of cold water, wherein divers fragments of ice were swimming, one or two pieces, that I perceived were well drenched with the liquor, and immersing them suddenly into a wide-mouthed glass, wherein strong oil of vitriol had been put; for this menstruum, presently mingling with the water, that adhered to the ice, produced in it a brisk heat, and that sometimes with a manifest smoke, which nimbly dissolved the contiguous parts of ice, and those the next, and so the whole ice being speedily reduced to water, and the corrosive menstruum being, by two or three shakes, well dispersed through it, and mingled with it, the whole mixture would grow, in a trice, so hot, that sometimes the vial, that contained it, was not to be endured in one's hand.

EXPERIMENT XV.

NOTWITHSTANDING the vast difference betwixt common water and high rectified spirit of wine, whereof men generally take the former for the most contrary body to fire, and whereof the chemists take the latter to be but a kind of liquid sulphur, since it may presently be all reduced into flame; yet, as I expected, I found, upon trial, that oil of vitriol, being mingled with pure spirit of wine, would as well grow hot, as with common water. Nor does this experiment always require great quantities of the liquors. For when I took but one ounce of strong oil of vitriol, though I put to it less than half an ounce of choice spirit of wine, yet those two being lightly shaken together, did, in a trice, conceive so brisk a heat, that they almost filled the vial with fumes, and made it so hot, that I had, unawares, like to have burned my hand with it before I could lay it aside.

I made the like trial with the same corrosive menstruum, and common aqua vitæ, bought at a strong-water-shop, by the mixture of which liquors heat was produced in the vial, that I could not well endure.

The like success I had in an experiment, wherein oil of vitriol was mixed with common brandy; save that in this the heat produced seemed not so intense as in the former trial, which itself afforded not so fierce a heat, as that, which was made with rectified spirit of wine.

EXPERIMENT XVI.

THOSE chemists, who conceive, that all the incalescences of bodies, upon their being mixed, proceed from their antiquity or hostility, will not perhaps expect, that the parts of the same body, (either numerically, or in specie, as the schools phrase it,) should, and that without manifest conflict, grow very hot together. And yet having for trial's sake put two ounces of colcothar so strongly calcined, that it was burnt almost to blackness, into a retort, we poured upon it two ounces of strong oil of English vitriol, and found, that after about a minute of an hour they began to grow so hot, that I could not endure to hold my hand to the bottom of the vessel, to which the mixture gave a heat, that continued sensible on the outside for between twenty and thirty minutes.

EXPERIMENT XVII.

THOUGH I have not observed any liquor to equal oil of vitriol in the number of liquors, with which it will grow hot; yet I have not met with any liquor, wherewith it came to a greater incalescence, than it frequently enough did with common oil of turpentine. For when we caused divers ounces of each to be well shaken together in a strong vessel, fastened, to prevent mischief, to the end of a pole or staff; the ebullition was great and fierce enough to be not undeservedly admired by the spectators. And this brings into my mind a pleasant adventure afforded by these liquors, of each of which, having for the production of heat and other purposes, caused a good bottle full to be put up with other things into a box, and sent down into the country, with a great change, that care should be had of the glasses; the waggon, in which the box was carried, happened, by a great jolt, that had almost overturned it, to be so rudely shaken, that these glasses were both broken, and the liquors, mingling in the box, made such a noise and stink, and sent forth such quantities of smoke by the vents, which the fumes had opened to themselves, that the passengers with great outcries and much haste threw themselves out of the waggon, for fear of being burnt in it.

THE trials we made with oil of turpentine, when strong spirit of nitre was substituted in the stead of oil of vitriol, belong not to this place.

EXPERIMENT XVIII.

BUT though petroleum, especially when rectified, be, as I have elsewhere noted, a most subtile liquor, and the lightest I have yet had occasion to try; yet to shew you, how much the incalescence of liquors may depend upon their texture, I shall add, that having mixed by degrees one ounce of rectified petroleum, with an equal weight of strong oil of vitriol, the former liquor seemed to work upon the surface of this last named, almost like a menstruum, upon a metal, in numerous and small bubbles continually ascending for a while into the oleum petræ, which had its colour manifestly altered and deepened by the operation of the spirituous parts. But by all the action and re-action of these liquors, there was produced no such smoking and boiling, or in-

tense

tense heat, as if oil of turpentine had been employed instead of oil of vitriol; the change, which was produced, as to qualities, being but a kind of tepidness discoverable by the touch.

ALMOST the like success we had in the conjunction of petroleum and spirit of nitre; a more full account whereof may be elsewhere met with.

IN this, and the late trials, I did not care to make use of spirit of salt, because, at least, if it be but ordinarily strong, I found its operation on the liquors above-mentioned inconsiderable, (and sometimes perhaps scarce sensible) in comparison of those of oil of vitriol, and in some cases of dephlegmed spirit of nitre.

EXPERIMENT XIX.

EXPERIENCED chemists will easily believe, that it were not difficult to multiply instances of heat producible by oil of vitriol upon solid bodies, especially mineral ones. For it is known, that, in the usual preparation of vitriolum martis, there is a great effervescence excited upon the affusion of the oil of vitriol upon filings of steel, especially, if they be well drenched in common water. And it will scarce be doubted, but that, as oil of vitriol will (at least partly) dissolve a great many, both calcined and testaceous bodies, as I have tried with lime, oyster-shells, &c. so it will, during the dissolution, grow sensibly, if not intensely hot with them, as I found it to do, both with those newly named, and others, as chalk, lapis calaminaris, &c. with the last of which, if the liquor be strong, it will heat exceedingly.

EXPERIMENT XX.

WHEREFORE I will rather take notice of its operation upon vegetable, as bodies, which corrosive menstuums have scarce been thought fit to dissolve and grow hot with. To omit then cherries, and divers fruits abounding in watery juices, with which, perhaps on that very account, oil of vitriol will grow hot; I shall here take notice, that, for trial sake, having mixed a convenient quantity of that liquor with raisins of the sun beaten in a mortar, the raisins grew so hot, that, if I mis-remember not, the glass, that contained it, had almost burnt my hand.

THESE kind of heats may be also produced by the mixture of oil of vitriol with divers other vegetable substances; but, as far as I have observed, scarce so eminently with any dry body, as with the crumbs of white bread, (or even of brown,) with a little of which we have sometimes produced a surprising degree of heat, with strong or well-dephlegmed oil of vitriol, which is to be supposed to have been employed in the foregoing experiments, and all others mentioned to be made by the help of that menstruum in our papers about qualities, unless it be in any particular case otherwise declared.

EXPERIMENT XXI.

IT is as little observed, that corrosive menstruums are able to work, as such, on the soft parts of dead animals, as on those of vegetables; and yet I have, more than once, produced a notable heat, by mixing oil of vitriol with minced flesh, whether roasted or raw.

EXPERIMENT XXII.

THOUGH common sea-salt does usually impart some degree, though not an intense one, of coldness unto common water, during the act of dissolution; yet some trials have informed me, that, if it were cast into a competent quantity of oil of vitriol, there would, for the most part, ensue an incalescence, which yet did not appear to succeed so regularly, as in most of the foregoing experiments. But, that heat should be produced usually, though not perhaps constantly, by the above-named menstruum and salt, seems therefore worthy of our notice, because it is known to chemists, that common salt is one main ingredient of the few, that make up common factitious sal armoniac, that is wont to be sold in the shops. And I have been informed, that the excellent academians of *Florence* have observed, that oil of vitriol would not grow hot, but cold, by being put upon sal armoniac: something like which, I took notice of in rectified spirit of sulphur, made *per campanam*, but found the effect much more considerable, when, according to the ingenious Florentine experiment, I made the trial with oil of vitriol; which liquor, having already furnished us with as many phænomena for our present purpose, as could be well expected from one agent, I shall scarce, in this paper about heat, make any farther use of it, but proceed to some other experiments, wherein it does not intervene.

EXPERIMENT XXIII.

WE took a good lump of common sulphur, of a convenient shape, and, having rubbed or chafed it well, we found, as we expected, that, by this attrition, it grew sensibly warm; and, that there was an intestine agitation, which you know is local motion, made by this attrition, did appear not only by the newly mentioned heat, whose nature consists in motion, and by the antecedent pressure, which was fit to put the parts into a disorderly vibration, but also by the sulphureous steams, which it was easy to smell, by holding the sulphur to one's nose as soon as it had been rubbed. Which experiment, though it may seem trivial in itself, may be worth the consideration of those chemists, who would derive all the fire and heat we meet with in sublunary bodies from sulphur. For, in our case, a mass of sulphur, before its parts were put into a new and brisk motion, was sensibly cold; and as soon

ſoon as its parts were put into a greater agitation than thoſe of a man's fingers, it grew ſenſibly hot; which argues, that it was not by its bare preſence, or any emanative action, (as the ſchools ſpeak,) that the ſulphur communicated any heat to my hand; and alſo, that, when it was briſkly moved, it did impreſs that quality, was no more than another ſolid body, though incombuſtible as common glaſs, would have done, if its parts had been likewiſe put into an agitation ſurpaſſing that of my organs of feeling; ſo that, in our experiment, ſulphur itſelf was beholden, for its actual heat, to local motion, produced by external agents in its parts.

EXPERIMENT XXIV.

WE thought it not amiſs to try, whether, when ſal armoniac, that much infrigidates water, and quick-lime, which is known to heat it, were by the fire exquiſitely mingled, the mixture would impart to the liquor a moderate or an intenſe degree of either of thoſe qualities. In proſecution of which enquiry, we took equal parts of ſal armoniac and quicklime, which we fluxed together, and putting an ounce, by gueſs, of the powdered mixture into a vial, with a convenient quantity of cold water, we found, that the colliquated maſs did, in about a minute, ſtrike ſo great a heat through the glaſs upon my hand, that I was glad to remove it haſtily, for fear of being ſcorched.

EXPERIMENT XXV.

WE have given ſeveral, and might have given many more, inſtances of the incaleſcence of mixtures, wherein both the ingredients were liquors, or, at leaſt, one of them was a fluid body. But ſometimes heat may alſo be produced by the mixture of two powders; ſince it has been obſerved, in the preparation of the butter or oil of antimony, that, if a ſufficient quantity of beaten ſublimate be very well mingled with powdered antimony, the mixture, after it has, for a competent time (which varies much according to circumſtances, as the weather, veſſel, place, &c. wherein the experiment is made,) ſtood in the air, would ſometimes grow manifeſtly hot, and now and then ſo intenſely ſo, as to ſend forth copious and fetid fumes, almoſt as if it would take fire. There is another experiment, made by the help of antimony, and a pulverized body, wherein the mixture, after it had been for divers hours expoſed to the air, viſibly afforded us mineral fumes. And to theſe I could add more conſiderable, and perhaps ſcarce credible inſtances of bodies growing hot without liquors, if philanthropy did not forbid me. But, to return to our butter of antimony, it ſeems not unfit to be enquired, whether there do not unobſervedly intervene an aqueous moiſture, which (capable of relaxing the ſalts, and ſetting them a-work,) I therefore ſuſpected might be attracted (as men commonly ſpeak) from the air, ſince the mixture of the antimony and the ſublimate is preſcribed to be placed in cellars; and in ſuch we find, that ſublimate, or at leaſt the ſaline part of it, is reſolved *per deliquium*, (as they call it,) which is nothing but a ſolution made by the watery ſteams wandering in the air.

EXPERIMENT XXVI.

I Have formerly delivered ſome inſtances of the incaleſcence produced by water in bodies, that are readily diſſolved in it, as ſalt of tartar and quick-lime. But one would not lightly expect, that mere water ſhould produce an incaleſcence in ſolid bodies, that are generally granted to be inſoluble in it; and are not wont to be, at leaſt without length of time, viſibly wrought on by it; and yet trial has aſſured me, that a notable incaleſcence may be produced by common water in flower or fine powder of ſulphur, and filings of ſteel or iron. For when, in ſummer-time, I cauſed to be mingled a good quantity, (as half a pound, or rather a pound, of each of the ingredients,) and cauſed them to be thoroughly drenched with common water, in a convenient quantity whereof they were very well ſtirred up and down, and carefully mingled, the mixture would, in a ſhort time, perhaps leſs than an hour, grow ſo hot, that the veſſel, that contained it, could not be ſuffered in one's hand; and the heat was manifeſted to other ſenſes than the touch, by the ſtrong ſulphureous ſtink, that invaded the noſe, and the thick ſmoke, that aſcended out of the mixture, eſpecially, when it was ſtirred with a ſtick or ſpattle. Whether the ſucceſs will be the ſame at all times of the year, I do not know, and ſomewhat doubt, ſince I remember not, that I had occaſion to try it in other ſeaſons, than in ſummer, or in autumn.

EXPERIMENT XXVII.

IN the inſtances, that chemiſtry is wont to afford us of the heat produced by the action of menſtruums upon other bodies, there intervenes ſome liquor, properly ſo called, that wets the hands of thoſe, that touch it; and there are divers of the more judicious chemiſts, that join with the generality of the naturaliſts in denying, that quickſilver, which is indeed a fluid body, but not a moiſt and wetting one, in reference to us, will produce heat by its immediate action on any other body, and particularly on Gold. But, though I was long inclinable to their opinion, yet I cannot now be of it, ſeveral trials having aſſured me, that a mercury, whether afforded by metals and minerals, or impregnated by them, may, by its preparation, be enabled to inſinuate itſelf nimbly into the body of gold, whether calcined or crude, and become manifeſtly incaleſcent with it in leſs than two or three minutes of an hour.

EXPERIMENT XXVIII.

SINCE we know, that ſome natural ſalts, and eſpecially ſalt-petre, can produce a coldneſs in the water they are diſſolved in, I thought

thought it might not be impertinent to our enquiry into heat and cold, and might perhaps also contribute somewhat to the discovery of the structure of metals, and the salts, that corrode them, if solutions were made of some saliformed bodies, as chemists call them, that are made up of metalline and saline parts, and do so abound with the latter, that the whole concretions are, on their account, dissoluble in common water.

OTHER experiments of this sort belonging less to this place than to another, I shall here only, for example-sake, take notice of one, that we made upon quicksilver, which is esteemed the coldest of metals. For having, by distilling from it four times its weight of oil of vitriol, reduced it to a powder, which, on the account of the adhering salts of the menstruum, that it detained, was white and glistering; we put this powder into a wide-mouthed glass of water, wherein a sealed weather-glass had been left, before it began manifestly to heat the water, as appeared by the quick and considerable ascent of the tincted spirit of wine, that continued to rise, upon putting in more of the magistery; which warm event is the more remarkable, because of the observation of *Helmont*, that the salt, adhering to the mercury, corroded in good quantity by oil of vitriol, if it be washed off, and coagulated, becomes a kind of alom.

THE event of the former trial deserves the more notice, because, having, after the same manner, and with the same weather-glass, made an experiment of common water, and the powder of vitriolum martis, made with oil of vitriol and the filings of steel, the tincted spirit of wine was not at all impelled up as before, but rather, after a while, began to subside, and fell though very slowly, about a quarter of an inch. The like experiment being tried with powdered sublimate in common water, the liquor in the thermoscope was scarce at all sensibly either raised or depressed, which argued the alteration, as to heat or cold, to have been either none, or very inconsiderable.

HAVING given warning at the beginning of this section, that in it I aimed rather at offering various, than numerous experiments about the production of heat, I think, what has been already delivered may allow me to take leave of this subject, without mentioning divers instances, that I could easily add, but think it fitter at present to omit. For those afforded me by trials about antiperistasis belong to a paper on that subject. Those, that might be offered about potential heat in human bodies, would, perchance, be thought but unnecessary, after what has been said of potential coldness; from which an attentive considerer may easily gather what, according to our doctrine, is to be said of the contrary quality. And divers phænomena, which would have been of the most considerable, I could have mentioned of the production of heat, since in them that quality is the most exalted, I reserve for the title of combustibleness and incombustibility, having already suffered this collection (or rather chaos) of particulars about the production of heat, to swell to too great a bulk.

EXPERIMENTS AND OBSERVATIONS, ABOUT THE Mechanical PRODUCTION of TASTES.

TO make out the mechanical origin, or production of ſapors, as far as is neceſſary for my preſent purpoſe, it will be expedient to premiſe in general, that, according to our notion of taſtes, they may depend upon the bigneſs, figure, and motion of the ſaporifick corpuſcles, conſidered ſeparately, and as the affections of ſingle, and very minute particles of matter; or elſe in a ſtate of conjunction, as two or more of theſe affections, and the particles they belong to, may be combined or aſſociated, either among themſelves, or with other particles, that were not ſaporous before. And as theſe coalitions, and other aſſociations come to be diverſified; ſo the taſtes, reſulting from them, will be altered or deſtroyed.

BUT, to handle theſe diſtinctly, and fully, were a taſk not only too difficult and long, but improper in this place, where I pretend to deliver not ſpeculations, but matters of fact: in ſetting down whereof, nevertheleſs, to avoid too much confuſion, I am content, where I can do it readily and conveniently, in ſome of my trials, to couch ſuch references, as may beſt point at thoſe heads, whence the mechanical explications may be derived, and conſequently our doctrine confirmed.

BY taſte conſidered, as belonging to the object, (under which notion I here treat of it,) I mean, that quality, or whatever elſe it be, which enables a body, by its operation, to produce in us that ſenſation, which we feel, or perceive, when we ſay we taſte.

THAT this ſomething, whether you will call it a quality, or whatever elſe it be, that makes, or denominates an object ſaporous, or rather (if I may be allowed a barbarous term) ſaporifick, may ſo depend upon the ſhape, ſize, motion, and other mechanical affections of the ſmall parts of the taſted body, and reſult from the aſſociation of two or more of them, not excluding their congruity, or incongruity to the organs of taſting, may be made probable by the following inſtances.

EXPERIMENT I.

To divide a body, almoſt inſipid, into two bodies of very ſtrong, and very differing taſtes.

IT is obſerved, that ſalt-petre refined, and by that purification freed from the ſea-ſalt, that is wont to be mingled with it, does rather cool the tongue, than make any great ſaporifick impreſſions on it. And though I will not ſay, that it is, as ſome have thought, an inſipid body; yet the bitteriſhneſs, which ſeems to be its proper taſte, is but very faint and languid. And yet this almoſt inſipid body, being diſtilled by the way of inflammation, (which I elſewhere teach,) or even by the help of an additament of ſuch clay, as is itſelf a taſteleſs body, will afford a nitrous ſpirit, that is extremely ſharp or corroſive upon the tongue, and will diſſolve ſeveral metals themſelves, and a fixed ſalt, that is likewiſe very ſtrongly taſted, but of a taſte altogether different from that of the ſpirit, that is extremely ſharp or corroſive upon the tongue; and accordingly, this ſalt will diſſolve divers compact bodies, that the other will not work on, and will precipitate divers metals, and other concretes, out of thoſe ſolutions, that have been made of them by the ſpirit.

EXPERIMENT II.

Of two bodies, the one highly acid and corroſive, and the other alkalizate and fiery, to produce a body almoſt inſipid.

THIS may be performed by the way I have elſewhere mentioned of compoſing ſalt-petre. For, if upon a liquor of fixed nitre, made *per deliquium*, you warily drop good ſpirit of nitre, till it be juſt enough to ſatiate the alkaly, (for if there be too much, or too little, the experiment may miſcarry,) we may, by a gentle evaporation, and ſometimes without it, and that in a few minutes, obtain cryſtals, which, being dried after they have been, if it be needful, freed from any adhering particles, (not of their own nature,) will have upon the tongue, neither a ſharp nor an alkalizate taſte, but that faint, and ſcarce ſenſible bitterneſs, that belongs to ſalt-petre, if it be pure ſalt-petre; for the impure may, perhaps, ſtrongly reliſh of the common ſalt, that is uſually contained in it.

THE like production of ſalt-petre we have ſometimes made in far leſs time, and ſometimes indeed in a trice, by ſubſtituting, inſtead of the fixed ſalt of nitre, the ſaline parts of good pot-aſhes, carefully freed by ſolution and filtration from the earthy and feculent ones.

I have ſometimes conſidered, whether the phænomena of theſe two experiments may not be explicated, by ſuppoſing them to ariſe from the new magnitudes and figures of the particles, which

which the fire, by breaking them, or forcibly rubbing them one against the other, or also against the corpuscles of the additament, may be presumed to give them; as if, for example, since we find the larger and best formed crystals of nitre to be of a prismatical shape with six sides, we should suppose the corpuscles of nitre to be little prisms, whose angles and ends are too obtuse or blunt to make vigorous and deep impressions on the tongue; and yet, if these little prisms be by a violent heat split, or otherwise broken, or forcibly made, as it were, to grind one another, they may come to have parts so much smaller than before, and endowed with such sharp sides, and angles, that, being dissolved and agitated by the spittle, that usually moistens the tongue, their smallness may give them great access to the pores of that organ, and the sharpness of their sides and points may fit them to stab and cut, and perhaps, sear the nervous and membranous parts of the organ of taste, and that variously, according to the grand diversities, as to shape and bulk of the saporifick particles themselves. And this being granted, it seemed further conceivable, that when the alkalizate and acid particles come to be put together in the fluid mixture, wherein they swam, many of them might, after a multitude of various justlings and occursions, meet with one another so luckily and opportunely, as to re-compose little prisms, or convene into other bodies, almost like those, that made up the crystals of nitre, before it was exposed to the fire. To illustrate which, we may conceive, that, though a prism of iron may be so shaped, that it will be wholly unfit to pierce the skin; yet it may be so cut by transverse planes reaching to the opposite bases or ends, as to afford wedges, which, by the sharpness of their edges, may be fit both to cleave wood, and cut the skin; and these wedges, being again put together, after a requisite manner, may re-compose a prism, whose extremes shall be too blunt to be fit for the former use. This may be also illustrated by the breaking of a dry stick, circularly cut off at the ends, which, though it is unapt, whilst entire, and of that bulk, to prick the hand; yet if it be violently broken, the ragged ends of it, and the splinters, may prove stiff, slender, and sharp enough to pierce and run into the hand: to which divers other such mechanical illustrations might be added. But, since I fear you think, as well as I, the main conjecture may not be worthy any farther prosecution, I shall not insist any longer on it. And because the historical part of these experiments was for the main delivered by me already in the essay about the analysis and redintegration of nitre, I shall now proceed to other trials.

EXPERIMENT III.

Of two bodies, the one extremely bitter, and the other exceeding salt, to make an insipid mixture.

TO make this experiment, we must very warily pour upon crystals made of silver, dissolved in good aqua fortis or spirit of nitre, strong brine made of common salt and water. For the mixture of these two being dried, and afterwards brought to fusion in a crucible, and kept a competent while in that state, will afford a tough mass, the chemists call luna cornea, which you may lick divers times, and scarce judge it other than insipid; nor will it easily be brought to dissolve in much more piercing menstruums than our spittle, as I have elsewhere shewn.

EXPERIMENT IV.

Of two bodies, the one extremely sweet, and the other salter than the strongest brine, to make an insipid mixture.

THE doing of this requires some skill and much wariness in the experimenter, who, to perform it well, must take a strong solution of minium, made with an appropriated menstruum, as good spirit of vinegar, or else saccharum saturni itself, dissolved in a convenient vehicle; and then must have great care and caution to put to it, by degrees, a just proportion of strong spirit of sal armoniac, or the like urinous spirit, till the whole be precipitated; and if the two former tastes are not sufficiently destroyed in the mixture, it may be dried and fluxed, as was above directed about luna cornea.

EXPERIMENT V.

Of an insipid body and a sour one, to make a substance more bitter than gall or aloes.

THIS is easily performed by dissolving in strong spirit of nitre or good aqua-fortis as much pure silver as the menstruum will take up; for this solution, being filtrated, has been often esteemed more bitter than so much gall or wormwood, or any other of those simples, that have been famous for that quality: and if the superfluous moisture be abstracted, you may by coagulation obtain crystals of luna, that have been judged more strongly bitter than the solution itself. And that the corpuscles of these crystals should leave a far more lasting taste of themselves, than the above-mentioned bitter bodies are wont to do, will not seem so marvellous, as I remember some, that tried, have complained; if we take notice, how deep the particles of these crystals may pierce into the spungy organs of taste, since, if one does but touch the pulp or nail of one's finger, (first a little wetted with spittle or otherwise,) with the powder of these crystals, they will so penetrate the skin or nail, and stick so fast there, that you cannot in a reasonable time wash the stain off of the skin, and much less off of the nail, but it will continue to appear many hours on the former, and many days on the other.

EXPERIMENT VI.

Of an insipid body and a highly corrosive one, to make a substance as sweet as sugar.

THIS is easily done, by putting upon good minium purified aqua-fortis or spirit of nitre, and letting them work upon one another

another in a gentle heat, till the liquor have dissolved its full proportion of the metal. For then, if the ingredients were good, and the operation rightly performed, the menstruum would have a sweetness like that of ordinary saccharum saturni. But it was not for nothing, that I intimated, the ingredients should be also pure and good in their kind; for, if the minium be adulterated, as often it is, or the spirit of nitre or aqua-fortis be mingled, as it is usual before it be purged with spirit of common salt, or other unfit ingredients, the operation may be successless, as I have more than once observed.

EXPERIMENT VII.

Of obtaining, without addition from the sweetest bodies, liquors corrosive enough to dissolve metals.

IF sugar be put into a sufficiently capacious retort, and warily distilled, (for otherwise it will be apt to break the vessel) it will afford, among other things, a copious red spirit, which, being slowly rectified, will lose its colour, and come over clear. The *caput mortuum* of the sugar, which I have more than once had of an odd contexture, may be found either almost or altogether insipid. And though the spirit will be of a very penetrant taste, yet it will be very far from any kind of sweetness; and though that liquor be thought to be homogeneous, and to be one of the principles of the analized sugar, yet (as I have elsewhere shewn) I found it to be a mixture of two spirits; with the one of which, besides bodies of a less close texture, I dissolved (even in the cold) crude copper, as was easy to be seen by the deep and lovely colour of the solution. And to these four spirits, afforded by sugar itself, we have restored a kind of saccharine sweetness, by compounding them with the particles of so insipid a body as minium; part of which they will in digestion dissolve. A like spirit to that distilled from sugar may be obtained from honey; but in regard of its aptness to swell exceedingly, chemists are not wont to distil it without sand, brick, or some other additament.

EXPERIMENT VIII.

To divide a body, bitter in the highest degree, into two substances, the one extremely sour, and the other perfectly insipid.

THIS is easily done by putting some fine crystals of *Luna* into a good retort, and then distilling them in a sand-furnace, capable of giving them so strong a fire, as to drive away all the spirits from the silver. For, this remaining behind, according to its metalline nature, will be insipid, and the spirits, that are driven away from it, will unite in the receiver into an acid and corrosive menstruum.

EXPERIMENT IX.

To produce variety of tastes in one insipid body, by associating it with divers menstruums.

AS this operation may, upon the account I elsewhere mention, be serviceable to investigate the figures of the particles of dissolved metals and other bodies; so it is very fit to manifest, what we would here have it shew, how much taste may be diversified by, and consequently depend upon texture; since a body, that has no taste, may, in conjunction with sapid bodies, give them strong tastes, all differing from one another, and each of them from that, which the saporous bodies had before. I could propose divers ways of bringing this to trial, there being several insipid bodies, which I have found this way diversifiable. But because I remember not, that I have met with any mineral, that is dissoluble by near so many saline menstruums, as zink, I look on that, as the most fertile subject to afford instances to our present purpose. For I have found, that it will be dissolved, not only by aqua-fortis, aqua regis, oil of vitriol, spirit of nitre, spirit of salt, and other mineral menstruums, but also by vegetable spirits, as distilled vinegar, and by animal ones too, as spirit of sal armoniac; though the one be acid, and the other urinous. And if the several solutions, which may be made of this mineral, by so many differing liquors, be compared, the number of their differing tastes will suffice to make good the title of the experiment.

EXPERIMENT X.

To produce variety of tastes with one menstruum, by associating it with insipid bodies.

THIS proposition a mathematician would go near to call the converse of the foregoing; and as it may serve, as well as that, to discover the structure of the minute parts of divers metalline and mineral bodies; so it may not only as well, but better than that, serve us to illustrate the corpuscularian doctrine of tastes, by shewing us, that a single, and, as far as chemistry teaches us, a simple body, endowed with a peculiar taste, may, by being compounded with others, each of them insipid of itself, produce a considerable number of differing tastes. There may be more instruments than one made use of in this trial; but of those, that are known, and we may easily obtain, the most proper are spirit of nitre, and good aqua-fortis: for that, with refined silver, will make a solution bitter as gall; with lead, it will be of a saccharine sweetness; with that part of tin, which it will keep dissolved, (for the greatest it is wont but to corrode and precipitate) it produces a taste very distant from both the former, but not odious; with copper, it affords an abominable taste; with mercury and iron, it affords other kinds of bad tastes. Nor are metals the only mineral bodies it will work upon; for, it will dissolve tin-glass, antimony, brass; to which I could add emery, zink, and other bodies, whereon I have tried it. All which together will make up no despicable number of differing tastes.

EXPERIMENT XI.

Of two liquors, the one highly corrosive, and the other very pungent and not pleasant, to compose a body of a pleasant and aromatick taste.

THIS experiment, which I elsewhere mention to other purposes, does in some regards

gards better suit our present design, than most of the foregoing; since here the corrosive menstruum is neither mortified by fixed nor urinous salts, supposed to be of a contrary nature to it; nor yet, as it were, tired out nor disarmed by corroding of metals or other solid bodies. The experiment being somewhat dangerous to make at first in great, it may suffice for our present turn, to make it in the less quantity, as follows.

TAKE one ounce of strong spirit of nitre, or of very good aqua-fortis itself, and put to it by little and little, (which caution, if you neglect, you may soon repent it,) another ounce of such rectified spirit of wine, as, being kindled in a spoon, will flame all away: when these two liquors are well mixed, and grown cold again, you may, after some digestion, or, if haste require, without it, distil them totally over together, to unite them exquisitely into one liquor, in which, if the operation have been well performed, the corrosive particles of the salts will not only lose all their cutting acidity, wherewith they wounded the palate; but by their new composition with the vinous spirits, the liquor acquires a vinous taste, that is not only not acid or offensive, but very pleasing, as if it belonged to some new or unknown spice.

EXPERIMENT XII.

To imitate by art, and sometimes even in minerals, the peculiar tastes of natural bodies, and even vegetables.

THIS is not a fit place to declare, in what sense I do or do not admit of souls in vegetables, nor what I allow or deny to the seminal or plastick principle ascribed to plants: but perhaps it will not be erroneous to conceive, that, whatever be the agent in reference to those tastes, that are said to be specifick to this or that plant, that, on whose immediate account it is, or becomes of this or that nature, is a complication of mechanical affections, as shape, size, &c. in the particles of that matter, which is said to be endowed with such a specifick taste.

To illustrate this, I thought it expedient, to endeavour to imitate the taste of some natural bodies by artificial compositions or preparations, but found it not easy, beforehand, to be assured of the success of such trials: and therefore I shall content my self here to mention three or four instances, that, except the first, are rather observations than such experiments as we are speaking of.

I remember then, that, making some trials to alter the sensible qualities of smell, taste, &c. of oil of vitriol, and spirit of wine, I obtained from them, among other things, that suited with my design, a certain liquor, which, though at first pleasant, would, at a certain nick of time, make one, that had it in his mouth, think it had been imbued with garlick.

AND this brings into my mind, that a skilful person, famous for making good cyder, coming one day to advise with me, what he should do to heighten the taste of it, and make it keep the longer, complained to me, that having, among other trials, put into a good vessel full of juice of apples a certain proportion of mustard seed, with hopes it would make the cyder more spirituous and piquant, he found, to his wonder and loss, that, when he came to draw it, it stunk of garlick so rank, that every body rejected it.

I remember also, that, by fermenting a certain proportion (for that we found requisite) of *semen dauci* with beer or ale, the liquor had a very pleasant relish of lemon-pills.

BUT that seems much more considerable, which I shall now add; that, with an insipid metal and a very corrosive menstruum, one may compound a taste, that I have several times observed to be so like a vegetable, that I presume it may deceive many. This may be done by dissolving gold, without any gross salt, in the mixture of aqua-fortis and the spirit of salt, or even in common aqua-regis, made by dissolving sal-armoniac in aqua-fortis. For if the experiment be happily made, one may obtain either a solution or a salt, whose austere taste will very much resemble that of sloes, or of unripe bullace. And this taste, with some little variety, I found in gold dissolved without any distilled liquor at all; and also, if I much forget not, in gold, that by a peculiar menstruum I had volatilized.

THE last instance I shall give of the imitation of tastes, I found to have been, for the main, known to some ingenious ladies. But to make the experiment succeed very well, a due proportion is the principal circumstance, which is wont to be neglected. I cannot readily call to mind that, which I found to succeed best; but the trial may be indifferently well made after such a manner as this:

TAKE a pint or a pound of *Malaga* or *Canary* sack, (for though French, and the like wines, may serve the turn, yet they are not so proper;) and put into it a drachm or two of good odoriferous orrice roots, cut into thin slices, and let them infuse in the liquor a convenient time, till you perceive, that they have given it a desired taste and smell; then keep the thus perfumed wine exactly stopped in a cool place: according to which way, I remember, that (when I hit on the right proportion of ingredients, and kept them a due time in infusion) I had many years ago a wine, which, being coloured with cochineal, or some such tinging ingredient, was taken for good rasberry-wine, not only by ordinary persons, but, among others, by a couple of eminent physicians, one of whom pretended to an extraordinary criticalness of palate on such occasions; both of them wondering, how at such an unlikely time of the year, as I chose to present them that liquor among others, I could have such excellent rasberry-wine: some of which (to add that by the by) I found to preserve the specifick taste two or three years after it was made.

A Short EXCURSION about some CHANGES made of TASTES by MATURATION.

IT will not perhaps be thought impertinent but rather necessary, to add a word or two on this occasion for their sakes, that think the maturation of fruits, and the changes of tastes, by which it is usually known, must needs be the effect of the vegetable soul of the plant. For, after the fruit is gathered, and so, by being no longer a part of the tree, does, according to the most common opinion, cease to be a part of the living plant, as a hand or a foot cut off is no more reckoned among the limbs of the man it belonged to; yet it is very possible, that some fruits may receive maturation, after they have been severed from the plants, that bore them. For, not to mention, that apples, gathered somewhat before the time, by lying in heaps, do usually obtain a mellowness, which seems to be a kind or degree of maturation; or that medlars, gathered whilst they are hard and harsh, do become afterwards in process of time soft and better tasted; in which state, though some say they are rotten, yet others think that supposed rottenness is the proper maturity of that kind of fruit: not to mention these, I say, or the like instances, it is a famous assertion of several writers of the Indian affairs, that the fruit they call bananas, is usually gathered green, and hung up in bunches or clusters in the house, whereby they ripen by degrees, and have an advantageous change made both of their colour and of their taste. And this an ancient acquaintance of mine, a literate and observing person, of whom I enquired about it, assured me, he had himself lately tried and found to be true in *America*. And indeed I see not, why a convenient degree of warmth, whether external from the sun and fire, or internal from some degree of fermentation or analogous intestine commotion, may not (whether the fruit be united to the plant or no) put the saporifick corpuscles into motion, and make them, by various and insensible transcursions, rub against each other, and thereby make the little bodies more slender or thin, and less rigid, or cutting and harsh, than they were before, and by various motions bring the fruit they compose to a state, wherein it is more soft in point of consistence, and abound in corpuscles less harsh and more pliable, than they were before, and more congruous to the pores of the organ of taste; and, in a word, make such a change in the constitution of the fruit, as men are wont to express by the name of Maturity. And that such mechanical changes of texture may much alter the qualities, and among them the taste of a fruit, is obvious in bruised cherries and apples, which in the bruised parts soon come to look and taste otherwise than they did before. The possibility of this is also obvious by wardens, when slowly roasted in embers, with so gentle a fire, as not to burn off the paper they are wont to be wrapped in, to be kept clean from the ashes. And I have seen in the bordering country between *France* and *Savoy*, a sort of pears, (whose name I now remember not,) which being kept for some hours in a moderate heat, in a vessel exactly closed, with embers and ashes above and beneath them, will be reduced to a juicy substance of a lovely red colour, and very sweet and luscious to the taste. Many other sorts of fruit in other countries, if they were handled after the same way, or otherwise skilfully wrought on by a moderate heat, would admit as great alterations in point of taste. Neither is that sort of pear to be here omitted, which by mere compression, duly ordered, without external heat, will in a few minutes be brought to exchange it's former hardness and harshness for so yielding a contexture and pleasant a taste, as I could not but think very remarkable. And that even more solid and stubborn salts, than those of vegetables, may have the sharpness and piercingness of their tastes very much taken off by the bare internal action of one part upon the other, without the addition of any sweetening body, I have been induced to think by having found, upon trial, that, by the help of insipid water, we may, without any violence of fire, reduce sea-salt into a brine of so mild and peculiar (I had almost said) pleasant a taste, that one would scarce suspect what it had been, or believe, that so great a change of a mineral body could be effected by so slight an intestine commotion, as indeed produced it; especially, since the alteration of tastes was not the most considerable, that was produced by this operation.

As to liquors, that come from vegetables, the emerging of new sapors upon the intestine commotion of the saporifick parts, as consequences of such commotions, is more obvious than is commonly considered in the juice of grapes, which, from a sweet and spiritless liquor, do by that internal motion, we call fermentation, acquire that pleasing pungency and briskness of taste, that belongs to wine, and afterwards degenerates into that acid and cutting taste, that is proper to vinegar; and all this, by a change of constitution made by the action of the parts themselves on one another, without the help of any external additament.

EXPERIMENTS AND OBSERVATIONS, ABOUT THE Mechanical Production of ODOURS.

SINCE tastes and odours (perhaps by reason of the nearness of the organs they affect) are wont, by physical writers, to be treated of next to one another, I also shall imitate them in handling those two qualities, not only for the intimated reason, but because, what I have premised in general, and some other things, that I have said already under the title of tastes, being applicable to odours also, it will not be necessary, and therefore it would be tedious, to repeat them here.

EXPERIMENT I.

With two bodies, neither of them odorous, to produce immediately a strong urinous smell.

TAKE good quick lime and sal armoniac, and rub or grind them well together, and holding your nose to the mixture, you will be saluted with an urinous smell produced by the particles of the volatile salt, untied by this operation, which will also invade your eyes, and make them to water.

EXPERIMENT II.

By the bare addition of common water, to produce immediately a very strong smell in a body, that had no such smell before.

THIS is one of the phænomena of an experiment made with camphire and oil of vitriol, which I have elsewhere mentioned to another purpose. For, if in that corrosive menstruum you dissolve a good proportion, but not too much, of the strongly scented gum, the odour of the camphire will be quite concealed in the mixture; but if you pour this mixture into a good quantity of fair water, the dissolved gum will immediately recover out of the menstruum, and smell as strong as before, if not (by reason of the warmth produced in the operation) more strongly.

EXPERIMENT III.

Of producing some odours, each of them quite differing from that of any of the ingredients.

HAVING taken two ounces, or parts of clear oil of turpentine, and mixed it with one ounce, or part, of oil of vitriol, (which must be done by degrees, for otherwise the vessel will be endangered,) the clear liquor, that came over, upon the distillation of the mixture in a sand-furnace, instead of the odour of turpentine, (for the oil of vitriol alone is wont to be inodorous,) smelt very strong of sulphur; insomuch, that once, when I shewed this experiment, approaching my nose very boldly and hastily to the receiver newly severed from the retort, the sulphureous stink proved so strong, that it had almost (to speak with the vulgar) taken away my breath. And to illustrate yet farther the possible emergency of such odours upon the mixture of ingredients, as neither of them was apart endowed with, we caused the substance, that remained behind in the retort (in the form of a thin extract) after one of the newly mentioned distillations to be farther pressed by a stronger fire, which forced most of it over, partly in the form of a thick oil, and partly in that of butter; both which we keep together in the same vial, because their odour is neither that of oil of turpentine, nor that of brimstone, but they smell exceedingly like the distilled oil of bees-wax.

EXPERIMENT IV.

About the production of some odours by local motion.

I Shall not now examine, whether the local motion of an external agent may not, without materially concurring to the operation, produce, by agitating and shuffling the parts, odorous corpuscles: but that the celerity and other modifications of the local motion of the effluvia of bodies may not only serve to diversify their odours, but so far produce them, as to make them perceptible by the sense, which otherwise would not be so, may be gathered from some observations, which, being obvious, are not so proper for this place. Wherefore I shall rather take notice, that I know several bodies, that are not only inodorous when cold, but when considerably hot, and are fixed in the fire, and yet, by having their parts put into a peculiar kind of agitation, will presently grow plainly odorous. On this occasion I shall add, that, as there are some very hard woods, that acquire a strong smell by the motion they may be exposed to in a turner's lath, (as I have observed by trials particularly made with the hard and ponderous lignum vitæ,) so some afford, whilst the operation lasts, an unexpected odour. And having enquired about this matter of two eminent artists, (whom I often employ) concerning the odour of beech-wood, whilst it is turning, they both agreed, that it would emit well-scented effluviums. And one of them affirmed to me farther, that, having bought a great block of that wood, to make divers pieces of workmanship with it, when he came to turn it, there would issue out not only a copious odour, but of such a peculiar fragrancy, that one, that knew not whence it proceeded, would have concluded he was smelling roses.

EXPERIMENT V.

By mixing a good proportion of a very strongly scented body with an almost inodorous one, to deprive it speedily of all its smell.

TAKE salt of tartar, and drop upon it either spirit of nitre, or aqua fortis, not too much dephlegmed, till all the effervescence cease, and the liquor will no longer work upon the alcali. These, by a slow evaporation of the superfluous moisture, may be made to shoot into crystals, like those of nitre, which, after you have (if need be) by rubbing them with a dried cloth, freed them from loose adhering corpuscles, will emulate salt-petre, as in other qualities, so in its not being odorous; though, if you distil them, or burn them on kindled coals, their fumes will quickly make you sensible, that they abounded with the stinking spirits, that make aqua fortis so offensive to the smell.

EXPERIMENT VI.

By putting a very strongly stinking body to another of a not sweet smell, to produce a mixture of a pleasant and strongly aromatic odour.

WHAT is here proposed is performed at the same time, that the eleventh of the foregoing experiments of tastes is made. For the liquor thereby produced, if it be well prepared, has not only a spicy taste, but also a kind of aromatic and pleasant smell; and I have some now by me, that, though kept not over-carefully, does, after some years, retain much of its former odour, though not so much as of its taste.

EXPERIMENT VII.

By digesting two bodies, neither of them well scented, to produce bodies of a very subtile and strongly fragrant odour.

WE took a pound (for instance) of Spanish wine, and put to it some ounces of oil of vitriol; then, keeping them for a reasonable time in digestion, we obtained, as we expected, a mixture odoriferous enough. But this trial you will find improved by that, which ensues.

EXPERIMENT VIII.

By the bare addition of a body almost inodorous, and not well scented, to give a pleasant and aromatic smell to spirit of wine.

THIS we have several times done, by the ways elsewhere related for another scope, the sum of which, as far as it needs be mentioned in this place, is this:

WE took good oil of blue vitriol (that was brought from *Dantzic*) though the very common will serve well, and having put to it, by degrees, an equal weight of spirit of wine totally inflammable, we digested them together, for two, three, or four weeks, (sometimes much longer, and then with better success;) from which, when we came to distil the mixture, we had a very fragrant spirit, which was sometimes so subtile, that, though distilled in a tall glass with a gentle heat, it would (in spite of our care to secure the closeness of the vessels at the junctures) pierce through, and fill the laboratory with a perfume, which, though men could not guess what body afforded it, yet they could not but wonder at it. Whence we may learn, both how much those spirituous and inflammable particles, the chemists call the vegetable sulphur of wine, may work on and enoble a mineral sulphur; (for, that such an one there is in oil of vitriol, I have elsewhere proved by experience;) and how much the new commissions and contextures, made by digestion, may alter the odours of bodies, whether vegetable or mineral. That also another constitution of the same matter, without any manifest addition or recess of particles, may proceed to exhibit a very differing smell, will appear by the following trial.

EXPERIMENT IX.

To make the formentioned fragrant body, without addition or fire, degenerate into the rank smell of garlick.

TO make out this, I need only relate, that I have more than once put the above mentioned fragrant liquor in stopped glasses, whereof the one, and not the other, stood in a warm place, till, in process of time, I found that odoriferous liquor so to degenerate in point of scent, that one would have thought it to have been strongly infected with garlick. And the like unpleasant smell I observed in a certain oil made of vegetable and mineral substances distilled together.

AND on this occasion I will add, (though not as an argument,) this observation, which though I shall not undertake it will always succeed, I think may not impertinently be set down in this place, partly because of the likeness of the odour produced, to that, which was the effect of the last named trial; and partly (or rather cheifly) because it may shew us, that a body, which itself is not only inodorous, but very fixed, may yet, in some cases, have a great stroke in the phænomena of odours; whether by being wrought on by, and sometimes mingled

mingled with, the parts of the odorous body, and thereby giving it a new modificatian, I ſhall not now ſtay to enquire.

WE took then good ſalt of tartar, and put to it, ſeveral times, its weight of the expreſſed juice of onions; we kept them in a light digeſtion for a day or two, and then unſtopping the vial, we found the former ſmell of the onions quite degenerated into a rank ſmell of garlick, as was judged, even when freſh juice of garlick was procured to compare them. To vary this experiment, we made, with fixed ſalts, and ſome other ſtrongly ſcented juices, trials, whoſe events it would perhaps be tedious here to relate.

EXPERIMENT X.

With an inodorous body, and another not well ſcented, to produce a muſky ſmell.

THIS we have ſometimes done by caſting into ſpirit (not oil) of vitriol a large proportion of ſmall pearls unbroken. For the action of the acid menſtruum upon theſe being moderated, partly by the weakneſs of the menſtruum, and partly by the entireneſs of the pearls, the diſſolution would ſometimes laſt many hours. Holding, from time to time, my noſe to the open orifice of the glaſs, it was eaſy to perceive a pleaſant muſky ſmell, which alſo others, to whom I mentioned it, took notice of, as well as I. And, if I miſremember not, I took notice of the like ſmell, upon pearls not only diſſolved in ſpirit of vinegar, but in another liquor, that had but a bad ſcent of its own. The foregoing experiment calls to my mind that, which follows.

EXPERIMENT XI.

With fixed metals, and bodies either inodorous or ſtinking, to produce ſtrong and pleaſant ſmells, like thoſe of ſome vegetables and minerals.

THAT gold is too fixed a body to emit any odour, and that aqua regis has an odour, that is very ſtrong and offenſive, I think will be eaſily granted. But yet aurum fulminans being made (as it is known) by precipitating, with the inodorous oil of tartar, the ſolution made of the former in the latter, and this precipitate being to be farther proceeded with in order to another experiment; we fulminated it *per ſe* in a ſilver veſſel like that, but better contrived, that is (if I miſremember not) ſomewhere deſcribed by *Glauberus*. And among other phænomena of this operation, that belong not to this place, we obſerved with pleaſure, that, when the fulmination was recently made, the ſteams, which were afforded by the metal, that had been fired, were endowed with a delightful ſmell, not unlike that of muſk. From which experiment, and the foregoing, we may learn, that art, by lucky contextures, may imitate the odours that are preſumed to be natural and ſpecifick; and that mineral and vegetable ſubſtances may compound a ſmell, that is thought to be peculiar to animals.

AND as art ſometimes imitates nature in the production of odours, as may be confirmed by what is above related concerning counterfeit raſberry wine, wherein thoſe, that drank it, believed they did not only taſte, but ſmell the raſberry; ſo ſometimes nature ſeems to imitate herſelf, in giving like odours to bodies extremely differing. For, not yet to diſmiſs the ſmell of muſk, there is a certain ſeed, which, for the affinity of its odour to that perfume, they call the muſk-ſeed; and indeed, having ſome of it preſented me by a gentleman, that had newly brought it from the *Weſt-Indies*, I found it, whilſt it was freſh, to have a fragancy ſuitable to the name, that was given it. There is alſo a ſort of rats in *Muſcovy*, whoſe ſkins, whereof I have ſeen ſeveral, have a ſmell, that has procured them the name of muſk-rats. To which I know not whether we may not add the mention of a certain ſort of ducks, which ſome call muſk-ducks, becauſe at a certain ſeaſon of the year, if they be chafed by violent motion, they will, under the wing, emit a muſky inſtead of a ſweaty ſcent; which, upon trial, I perceived to be true. On the other ſide, I have known a certain wood, growing in the *Indies*, which, eſpecially when the ſcent is excited by rubbing, ſtinks ſo rankly, and ſo like *Paracelſus*'s zibetum occidentale, (ſtercus humanum) that one would ſwear it were held under his noſe. And ſince I have been ſpeaking of good ſcents, produced by unlikely means, I ſhall not pretermit this obſervation, that, though generally the fire impreſſes a ſtrong offenſive ſmell, which chemiſts therefore call empyreumatical, upon the odorous bodies, that it works ſtrongly on; yet the conſtitution of a body may be ſuch, that the new contexture, that is made of its parts, even by the violence of the fire, ſhall be fit to afford effluviums, rather agreeable to the organs of ſmelling, than any way offenſive. For I remember, that, having, for a certain purpoſe, diſtilled ſaccharum Saturni in a retort with a ſtrong fire, I then obtained, (for I dare not undertake for the like ſucceſs to every experiment,) beſides a piercing and empyreumatical liquor, that was driven over into the receiver, a good lump of a caput mortuum, of a grayiſh colour, which, notwithſtanding the ſtrong impreſſion it had received from the fire, was ſo far from having any empyreumatical ſcent, that it had a pleaſing one; and when it was broken, ſmelled almoſt like a fine cake new baked, and broken, whilſt yet warm. And as the fire, notwithſtanding the empyreuma it is wont to give to almoſt all the bodies it burns, may be reduced to confer a good ſmell on ſome of them, if they be fitted upon ſuch a contexture of their parts, to emit ſteams of ſuch a nature, (whatever were the efficient cauſe of ſuch a contexture;) ſo we obſerve in the muſk animal, that nature in that cat, or rather deer, (though it properly belong to neither kind,) produces muſk by ſuch a change, as is wont in other animals to produce a putrefactive ſtink. So that, provided a due conſtitution of parts be introduced into a portion of matter, it may, on that account, be endowed with noble and deſirable ſcents, or other qualities, though that conſtitution were introduced

See in the paper of taſtes, exper. xii.

duced by such unlikely means, as combustion and putrefaction themselves. In confirmation of which, I shall subjoin, in the ensuing account, a notable, though casual phænomenon, that occurred to a couple of virtuosi of my acquaintance.

AN eminent professor of mathematicks affirmed to me, that, chancing one day in the heat of summer, with another mathematician, (who, I remember, was present, when this was told,) to pass by a large dunghil, that was then in *Lincoln's-inn-fields*, when they came to a certain distance from it, they were both of them surprised to meet with a very strong smell of musk, (occasioned, probably, by a certain degree or a peculiar kind of putrefaction,) which each was for a while shy of taking notice of, for fear his companion should have laughed at him for it; but, when they came much nearer the dunghil, that pleasing smell was succeeded by a stink proper to such a heap of excrements. This puts me in mind of adding, that, though the excrements of animals, and particularly their sweat, are usually fœtid; yet, that it is not the nature of an excrement, but the constitutions, that usually belong to them, make them so, hath seemed probable to me, upon some observations. For, not to mention what is related of *Alexander* the Great, I knew a gentleman of a very happy temperature of body, whose sweat, upon a critical examination, wherein I made use also of a surprize, I found to be fragrant; which was confirmed also by some learned men of my acquaintance, and particularly a physician, that lay with him.

THOUGH civet usually passes for a perfume, and as such is wont to be bought at a great rate; yet it seems to be but a clammy excrement of the animal, that affords it, which is secreted into bags provided by nature to receive it. And I the rather mention civet, because it usually affords a phænomenon, that agrees very well with the mechanical doctrine concerning odours, though it do not demonstrate it. For, when I have had the curiosity to visit divers of those civet-cats, (as they call them) though they have heads liker foxes than cats; I observed, that a certain degree of laxity (if I may so stile it) of the odorous atmosphere was requisite to make the smell fragrant. For, when I was near the cages, where many of them were kept together, or any great vessel full of civet, the smell (probably by the plenty, and perhaps the over-brisk motion of the effluvia,) was rather rank and offensive, than agreeable; whereas, when I removed into the next room, or to some other convenient distance, the steams (being less crowded, and farther from their fountain,) presented themselves to my nostrils, under the notion of a perfume.

AND, not to dismiss this our eleventh experiment, without touching once more upon musk, I shall add, that an ingenious lady, to whom I am nearly related, shewed me an odd monkey, that had been presented her as a rarity by the then admiral of *England*, and told me, among other things, that she had observed in it, that, being sick, he would seek for spiders as his proper remedies, for some of which he then seemed to be looking, and thereby gave her occasion to tell me this; which, when he had eaten, the alteration it made in him, would sometimes fill the room with a musky scent: but he had not the good luck to light on any, whilst my visit lasted.

EXPERIMENT XII.

To heighten good smells by composition.

IT is well known to perfumers, and is easy to be observed, that amber-grease alone, though esteemed the best and richest perfume, that is yet known in the world, has but a very faint, and scarce a pleasant scent. And I remember, that I have seen some hundreds of ounces together, newly brought from the *East-Indies*; but if I had not been before acquainted with the smell of ambergrease alone, and had had only the vulgar conceit of it, that it is the best and strongest of perfumes, my nostrils would scarce have made me suspect those lumps to have been any thing a-kin to ambergrease. But if a due proportion of musk, or even civet, be dexterously mixed with amber, the latent fragrancy, though it be thereby somewhat compounded, will quickly be called forth, and exceedingly heightened. And indeed it is not, as it is commonly presumed, the plenty of the richest ingredients, as amber-grease and musk, but the just proportion and skilful mixture of them, that makes the noblest and most lasting perfume, of which I have had sufficient experience; so that with a far less quantity of musk and amber, than not only ordinary persons, but perfumers themselves are wont to employ, we have had several perfumes, that, for fragrancy, were much preferred to those, where musk and amber-grease are so plentifully employed. The proportions and ways of mixture we best approved of, would be too long, and are not necessary to be here set down; but you will not much err in making use of such a proportion as this, viz. eight parts of ambergrease, two of musk, and one of civet: which quantities of ingredients, if they be skilfully and exactly mingled, you will not miss of a good composition, with which you may ennoble other materials, as benzoin, storax, sweet flowers, &c. fit to make pastils, ointments for leather, pomander, &c. And we may here add, that, upon the score of the new texture acquired by composition, some things, that are not fragrant themselves, may yet much heighten the fragrancy of odoriferous bodies. And of liquid perfumes, I remember, it was the secret of some court ladies, noted for curiosity about perfumes, to mingle always a due proportion of wine-vinegar with the odoriferous ingredients. And, on this occasion, to shew the power of mixtures in improving odours, I shall add something about a liquor of mine, that has had the good fortune to be very favourably spoken of by persons of quality accustomed to choice perfumes. This liquor, though thought an elaborate preparation, as well for another reason, as to recommend it to some, whose critical

critical palates can taste the very titles of things, I called it essence of musk, is indeed a very plain simple preparation, which I thus make.

I take an arbitrary quantity of choice musk, without finely powdering it, and pour upon it about a finger's breadth of pure spirit of wine; these, in a glass closely stopped, I set in a quiet place to digest, without the help of any furnace; and after some days, or a few weeks, (according as circumstances determined,) the spirit, which is somewhat odd, will, in the cold, have made a solution of the finest parts of the musk, and will be thereby much tinged, but not of a red colour. This liquor, being decanted, I keep by itself as the richest of all; and pour a like quantity of spirit on the remaining musk, which usually will, in the cold, though more slowly, draw a tincture, but fainter than the former; which being poured off, the remaining musk may be employed for inferior uses. Now that, which made me mention this preparation as pertinent to our present subject, is this phænomenon of it; that the first essence, or rather tincture, being smelled to by itself, has but a faint, and not very pleasing odour of musk, so that every body would not discover, that there was musk in it; but if a single drop, or two drops at most, were mixed with a pint, or perhaps a quart of good sack, the whole body of the wine would presently acquire a considerably musky scent, and be so richly perfumed, as to taste and smell, as seemed strange enough to those, that knew the vast disproportion of the ingredients.

OF THE

IMPERFECTION

OF THE

CHEMIST'S DOCTRINE

OF

QUALITIES.

CHAP. I.

SINCE a great part of those learned men, especially physicians, who have discerned the defects of the vulgar philosophy, but are not yet come to understand and relish the corpuscularian, have slid into the doctrine of the chemists; and since the spagyrists are wont to pretend to make out all the qualities of bodies, from the predominancy of some one of their three hypostatical principles, I suppose it may both keep my opinion from appearing too presumptuous, and (which is far more considerable) may make way for the fairer reception of the mechanical hypothesis about qualities, if I here intimate (though but briefly and in general) some of those defects, that I have observed in chemist's explications of qualities.

AND I might begin with taking notice of the obscurity of those principles, which is no small defect in notions, whose proper office it should be to conduce to the illustration of others. For, how can that facilitate the understanding of an obscure quality, or phænomenon, which is itself scarcely intelligible, or, at least, needs almost as much explanation, as the thing it is designed and pretended to explicate? Now a man need not be very conversant in the writings of chemists to observe, in how lax, indefinite, and almost arbitrary senses they employ the terms of salt, sulphur, and mercury; of which I could never find, that they were agreed upon any certain definitions, or settled notions; not only differing authors, but not unfrequently one and the same, and perhaps in the same brook, employing them in very differing senses. But I will not give the chemists any rise to pretend, that the chief fault, that I find with their hypothesis, is but verbal; though that itself may not a little blemish any hypothesis, one of the first of whose requisites ought to be clearness; and therefore I shall now advance, and take notice

tice of defects, that are manifeſtly of another kind.

AND, firſt, the doctrine, that all their theory is grounded on, ſeems to me inevident, and undemonſtrated, not to ſay precarious. It is ſomewhat ſtrange to me, that neither the ſpagyriſts themſelves, nor yet their adverſaries, ſhould have taken notice, that chemiſts have rather ſuppoſed than evinced, that the analyſis of bodies by fire, or even, that at leaſt ſome analyſis, is the only inſtrument of inveſtigating what ingredients mixed bodies are made up of, ſince, in divers caſes, that may be diſcovered by compoſition, as well as by reſolution; as it may appear, that vitriol conſiſts of metalline parts, (whether martial or venereal, or both,) aſſociated by coagulation with acid ones, one may, I ſay, diſcover this, as well by making true vitriol with ſpirit (improperly called oil) of ſulphur, or that of ſalt, as by diſtilling or reſolving vitriol by the fire.

BUT I will not here enlarge on this ſubject, nor yet will I trouble you with what I have largely diſcourſed in the Sceptical Chymiſt, to call in queſtion the grounds on which chemiſts aſſert, that all mixed bodies are compounded of ſalt, ſulphur, and mercury. For it may ſuffice me now to tell you, that, whatſoever they may be able to obtain from other bodies, it does not appear by experience, which is the grand, if not the only argument they rely on, that all mixed bodies, that have qualities, conſiſt of their *tria prima*, ſince they have not been able, that we know, truly, and without new compoſitions, to reſolve into thoſe three, either gold, or ſilver, or cryſtal, or Venetian talck, or ſome other bodies, that I elſewhere name; and yet theſe bodies are endowed with divers qualities, as the two former with fuſibleneſs and malleability, and all of them with weight and fixity; ſo that in theſe and the like bodies, whence chemiſts have not made it yet appear, that their ſalt, ſulphur, and mercury, can be truly and adequately ſeparated, it will ſcarce be other than precarious, to derive the malleableneſs, colour, and other qualities of ſuch bodies from thoſe principles.

UNDER this head I conſider alſo, that a great part of the chemical doctrine of qualities is bottomed on, or ſuppoſes, beſides their newly queſtioned analyſis by fire, ſome other things, which as far as I know, have not yet been well proved, and I queſtion whether they ever will be.

ONE of their main ſuppoſitions is, that this or that quality muſt have its πρῶτον δεκτικὸν, as *Sennertus*, the learnedeſt champion of this opinion, calls it, or ſome particular material principle, to the participation of which, as of the primary native and genuine ſubject, all other bodies muſt owe it: but upon this point having purpoſely diſcourſed elſewhere, I ſhall now only obſerve, that, not to mention local motion and figure, I think it will be hard to ſhew, what is the πρῶτον δεκτικὸν of gravity, volatility, heat, ſonorouſneſs, tranſparency, and opacity, which are qualities to be indifferently met with in bodies, whether ſimple or mixed.

AND, whereas the ſpagyriſts are wont to argue, that becauſe this or that quality is not to be derived truly from this or that particular principle, as ſalt, for inſtance, and mercury; therefore it muſt needs be derivable from the third, as ſulphur. This way of arguing involves a further ſuppoſition than that newly examined. For it implies, that every quality in a compounded body muſt ariſe from ſome one of the *tria prima*, whereas experience aſſures us, that bodies may, by compoſition, obtain qualities, that were not to be found in any of the ſeparate ingredients. As we ſee in painting, that though blue and yellow be neither of them green, yet their mixture will be ſo. And though no ſingle ſound will make an octave or diapaſon; yet two ſounds, whoſe proportion is double, will have an eighth. And tin and copper melted and mingled together in a due proportion, will make a bell-metal far more ſonorous than either of them was before. It is obvious enough for chemiſts themſelves to obſerve, that, though lead be an inſipid body, and ſpirit of vinegar a very ſharp one, yet ſaccharum ſaturni, that is compounded out of theſe two, has a ſweetneſs, that makes it not ill deſerve its name.

BUT this ill-grounded ſuppoſition of the chemiſts, is extended farther in an uſual topic of theirs, according to which they conclude, that I know not how many qualities, as well manifeſt as occult, muſt be explicated by their *tria prima*, becauſe they are not explicable by the four elements of the Peripateticks. To make which argumentation valid, it muſt be proved, (which I fear it will never be) that there are no other ways, by which thoſe qualities may be explicated, but by a determinate number of material principles, whether four or three: beſides that, till they have ſhewn that ſuch qualities may be intelligibly explicated by their principles, the objection will lie as ſtrong for the Ariſtotelians againſt them, as for them againſt the Ariſtotelians.

CHAP. II.

NEXT I conſider, that there are divers qualities even in mixed bodies, wherein it does not appear, that the uſe of the chemical doctrine is neceſſary. As for inſtance, when pure gold is by heat only brought to fuſion, and conſequently to the ſtate of fluidity, and upon the remiſſion of that heat, grows a ſolid and conſiſtent body again, what addition or expulſion, or change of any of the *tria prima*, does appear to be the cauſe of this change of of conſiſtence? which is eaſy to be accounted for according to the mechanical way, by the vehement agitation, that the fire makes of the minute parts of the gold to bring it to fuſion; and the coheſion of thoſe parts, by virtue of their gravity and fitneſs to adhere to one another, when that agitation ceaſes. When Venice glaſs is merely, by being beaten to powder, deprived of its tranſparency and turned into a body opacous and white, what need or uſe of the *tria prima* have we in the explication of this phænomenon? Or of that other, which

which occurs, when by barely melting down this white and opacous body it is deprived of its opacity and colour, and becomes diaphanous; and of this sort of instances you will meet with divers in the following notes about particular qualities; for which reason I shall forbear the mention of them here.

CHAP. III.

I OBSERVE too, that the spagyrical doctrine of qualities is insufficient, and too narrow to reach to all the phænomena, or even to all the notable ones, that ought to be explicable by them. And this insufficiency I find to be twofold; for, first, there are divers qualities, of which chemists will not so much as attempt to give us explications, and of other particular qualities, the explications, such as they are, that they give us, are often very deficient and unsatisfactory; and do not sometimes so much as take notice of divers considerable phænomena, that belong to the qualities, whereof they pretend to give an account; of which you will meet with divers instances in the ensuing notes. And therefore I shall only (to declare my meaning the better) invite you to observe with me, that though gold be the body they affect to be most conversant with; yet it will be very hard to shew, how the specific weight of gold can be deduced from any, or all, of the three principles, since mercury itself, that is, of bodies known to us, the heaviest next to gold, is so much lighter than gold, that, whereas I have usually found mercury to be to an equal weight of water, somewhat, though little, less than fourteen to one, I find pure gold to be about nineteen times as heavy as so much water. Which will make it very difficult, not to say impossible for them to explain, how gold should barely, by participating of mercury, which is a body much lighter than itself, obtain that great specific gravity we find it to have; for the two other hypostatical principles, we know, are far lighter than mercury. And I think it would much puzzle the chemists, to give us any examples of a compounded body, that is specifically heavier than the heaviest of the ingredients, that it is made up of. And this is the first kind of insufficiency I was taking notice of in the chemical doctrine of qualities.

The other is, that there are several bodies, which the most learned among themselves confess not to consist of their *tria prima*, and yet are endowed with qualities, which consequently are not in those subjects to be explicated by the *tria prima*, which are granted not to be found in them. Thus elementary water, though never so pure, (as distilled rain water,) has fluidity and coldness and humidity and transparency and volatility, without having any of the *tria prima*. And the purest earth, as ashes, carefully freed from the fixed salt, has gravity and consistence and dryness and colour and fixity, without owing them either to salt, sulphur, or mercury; not to mention, that there are celestial bodies, which do not appear, nor are wont to be pretended to consist of the *tria prima*, that yet are endowed with qualities. As the sun has light and, as many philosophers think, heat and colour; and the moon has a determinate consistence and figuration, (as appears by her mountains) and astronomers observe, that the higher planets, and even the fixed stars, appear to be differingly coloured. But I shall not multiply instances of this kind, because what I have said may not only serve for my present purpose, but bring a great confirmation to what I lately said, when I noted, that the chemical principles were in many cases not necessary to explicate qualities: for since in earth, water, &c. such diffused qualities, as gravity, fixedness, colour, transparency and fluidity, must be acknowledged not to be derived from the *tria prima*; it is plain, that portions of matter may be endowed with such qualities by other causes and agents than salt, sulphur and mercury. And then why should we deny, that also in compounded bodies those qualities may be (sometimes at least) produced by the same or the like causes? as we see, that the reduction of a diaphanous solid to powder produces whiteness, whether the comminution happens to rock-crystal or to Venice glass, or to ice: the first of which is acknowledged to be a natural and perfectly mixed body; the second a factitious, and not only mixed, but decompounded body; and the last, for aught appears, an elementary body, or at most very slightly and imperfectly mixed. And so by mingling air in small portions with a diaphanous liquor, as we do, when we beat such a liquor into foam, a whiteness is produced, as well in pure water, which is acknowledged to be a simple body, as in white wine, which is reckoned among perfectly mixed bodies.

CHAP. IV.

I FURTHER observe, that the chemist's explications do not reach deep and far enough. For first, most of them are not sufficiently distinct and full, so as to come home to the particular phænomena, not oftentimes so much as to all the grand ones, that belong to the history of the qualities they pretend to explicate. You will readily believe, that a chemist will not easily make out by his salt, sulphur and mercury, why a loadstone, capped with steel, may be made to take up a great deal more iron, sometimes more than eight or ten times as much, than if it be immediately applied to the iron: or why, if one end of the magnetic needle is disposed to be attracted by the north-pole, for instance, of the load-stone, the other pole of the load-stone will not attract it, but drive it away: or why a bar or rod of iron, being heated red-hot, and cooled perpendicularly, will, with its lower end, drive away the flower de luce, or the north end of a mariner's needle, which the upper end of the same bar or rod will not repel, but draw to it. In short, of above threescore properties, or notable phænomena of magnetic bodies, that some writers have reckoned up, I do not remember, that any three have been by chemists so much as attempted to be solved by their three principles. And even in those qualities, in whose explications these principles may more

more probably, than elsewhere, pretend to have a place, the Spagyrists accounts are wont to fall so short of being distinct and particular enough, that they use to leave divers considerable phænomena untouched, and do but very lamely or slightly explicate the more obvious or familiar. And I have so good an opinion of divers of the embracers of the Spagyrical theory of qualities (among whom I have met with very learned and worthy men) that I think, that if a quality being proposed to them, they were at the same time presented with a good catalogue of the phænomena, that they may take, in the history of it, as it were with one view, they would plainly perceive, that there are more particulars to be accounted for, than at first they were aware of; and divers of them such, as may quite discourage considering men from taking upon them to explain them all by the *tria prima*, and oblige them to have recourse to more catholic and comprehensive principles. I know not, whether I may not add on this occasion, that, methinks, a chemist, who by the help of his *tria prima*, takes upon him to interpret that book of nature, of which the qualities of bodies make a great part, acts at but a little better rate than he, that seeing a great book written in a cypher, whereof he were acquainted but with three letters, should undertake to decypher the whole piece. For though it is like he would, in many words, find one of the letters of his short key, and in divers words two of them, and perhaps in some all three; yet, besides that in most of the words, wherein the known letter or letters may be met with, they may be so blended with other unknown letters, as to keep him from decyphering a good part of those very words, it is more than probable, that a great part of the book would consist of words wherein none of his three letters were to be found.

CHAP. V.

AND this is the first account, on which I observe, that the chemical theory of qualities does not reach far enough: but there is another branch of its deficiency. For even, when the explications seem to come home to the phænomena, they are not primary, and, if I may so speak, fontal enough. To make this appear, I shall at present employ but these two considerations. The first is, that those substances themselves, that chemists call their principles, are each of them endowed with several qualities. Thus salt is a consistent, not a fluid body; it has its weight, it is dissoluble in water, is either diaphanous or opacous, fixed or volatile, sapid or insipid; (I speak thus disjunctively, because chemists are not all agreed about these things; and it concerns not my argument, which of the disputable qualities be resolved upon.) And sulphur, according to them, is a body fusible, inflammable, &c. and, according to experience, is consistent, heavy, &c. So that it is by the help of more primary and general principles, that we must explicate some of those qualities, which being found in bodies, supposed to be perfectly similar or homogeneous, cannot be pretended to be derived in one of them from the other. And to say, that it is the nature of a principle to have this or that quality, as for instance, of sulphur to be fusible, and therefore we are not to exact a reason why it is so; though I could say much by way of answer, I shall now only observe, that this argument is grounded but upon a supposition, and will be of no force, if from the primary affections of bodies one may deduce any good mechanical explication of fusibility in the general, without necessarily supposing such a primogeneal sulphur, as the chemists fancy, or deriving it from thence in other bodies. And indeed, since not only salt petre, sea salt, vitriol and allom, but salt of tartar, and the volatile salt of urine, are all of them fusible; I do not well see, how chemists can derive the fusibleness even of salts obtained by their own analysis (such as salt of tartar and of urine) from the participation of the sulphureous ingredient; especially since, if such an attempt should be made, it would overthrow the hypothesis of three simple bodies, whereof they will have all mixed ones to be compounded; and still it would remain to be explicated, upon what account the principle, that is said to endow the other with such a quality, comes to be endowed therewith itself. For it is plain, that a mass of sulphur is not an atomical or adamantine body, but consists of a multitude of corpuscles of determinate figures, and connected after a determinate manner; so that it may be reasonably demanded, why such a convention of particles, rather than many another, that does not, constitutes a fusible body.

CHAP. VI.

AND this leads me to a further consideration, which makes me look upon the chemist's explications, as not deep and radical enough; and it is this, that, when they tell us, for instance, that the fusibleness of bodies proceeds from sulphur, in case they say true, they do but tell us what material ingredient it is, that being mingled with, and dispersed through the other parts of a body, makes it apt to melt: but this does not intelligibly declare, what it is, that makes a proportion of matter fusible, and how the sulphureous ingredient introduces that disposition into the rest of the mass, wherewith it is commixed or united. And yet it is such explications as these, that an inquisitive naturalist chiefly looks after, and which I therefore call philosophical. And to shew, that there may be more fontal explications, I shall only observe, that, not to wander from our present instance, sulphur itself is fusible. And therefore, as I lately intimated, fusibility, which is not the quality of one atome, or particle, but of an aggregate of particles, ought itself to be accounted for in that principle, before the fusibleness of all other bodies be derived from it. And it will in the following notes appear, that in sulphur itself, that quality may be probably deduced from the convention of corpuscles of determinate shapes, and

and sizes; contexed, or connected, after a convenient manner. And if either nature, or art, or chance, should bring together particles endowed with the like mechanical affections, and associate them after the like manner, the resulting body would be fusible, though the component particles had never been parts of the chemist's primordial sulphur: and such particles so convening, might, perhaps, have made sulphur itself, though before there had been no such body in the world. And what I say to those chemists, that make the sulphureous ingredient the cause of fusibility, may easily, *mutatis mutandis*, be applied to their hypothesis, that rather ascribe that quality to the mercurial, or the saline principle, and consequently cannot give a rational account of the fusibility of sulphur. And therefore, though I readily allow, (as I shall have afterwards occasion to declare,) that sulphur, or an other of the *tria prima*, may be met with, and even abound in several bodies endowed with the quality, that is attributed to their participation of that principle; yet, that this may be no certain sign, that the proposed quality must flow from that ingredient, you may perhaps be assisted to discern by this illustration, that if tin be duly mixed with copper or gold, or, as I have tried, with silver or iron, it will make them very brittle; and it is also an ingredient of divers other bodies, that are likewise brittle, as blue, green, white, and otherwise coloured amels, which are usually made of calcined tin, (which the tradesmen call putty) colliquated with the ingredients of crystal glass, and some small portion of mineral pigment. But though, in all the above-named brittle bodies, tin be a considerable ingredient; yet it were very unadvised to affirm, that brittleness, in general, proceeds from tin. For, provided the solid parts of consistent bodies touch one another but according to small portions of their surfaces, and be not implicated by their contexture, the metalline, or other composition, may be brittle, though there be no tin at all in it. And, in effect, the materials of glass, being brought to fusion, will compose a brittle body, as well when there is no putty colliquated with them, as when there is. Calcined lead, by the action of the fire, may be melted into a brittle mass, and even into transparent glass, without the help of tin, or any other additament. And I need not add, that there are a multitude of other bodies, that cannot be pretended to owe their brittleness to any participation of tin, of which they have no need, if the matter they consist of, wants not the requisite mechanical dispositions.

And here I shall venture to add, that the way employed by the chemists, as well as the Peripateticks, of accounting for things by the ingredients, whether elements, principles, or other bodies, that they suppose them to consist of, will often frustrate the naturalists expectation of events, which may frequently prove differing from what he promised himself, upon the consideration of the qualities of each ingredient. For the ensuing notes contain divers instances, wherein there emerges a new quality differing from, or even contrary to any, that is conspicuous in the ingredients; as two transparent bodies may make an opacous mixture, a yellow body and a blue, one, that is green; two malleable bodies, a brittle one; two actually cold bodies, a hot one; two fluid bodies, a consistent one, &c. And as this way of judging, by material principles, hinders the foreknowledge of events from being certain; so it much more hinders the assignation of causes from being satisfactory; so that, perhaps, some would not think it very rash to say, that those, who judge of all mixed bodies, as apothecaries do of medicines, barely by the qualities and proportions of the ingredients (such as, among the Aristotelians, are the four elements, and among the chemists the *tria prima*,) do, as if one should pretend to give an account of the phænomena and operations of clocks and watches, and their diversities by this, that some are made of brass wheels, some of iron, some have plain ungilt wheels, others of wheels overlaid with gold, some furnished with gut-strings, others with little chains, &c. and that therefore the qualities and predominancies of these metals, that make parts of the watch, ought to have ascribed to them, what indeed flows from their co-ordination and contrivance.

CHAP. VII.

THE last defect I observe in the chemical doctrine of qualities, is, that in many cases it agrees not well with the phænomena of nature, and that by one or both of these ways. First, there are divers changes of qualities, wherein one may well expect, that a chemical principle should have a great stroke, and yet it does not at all appear to have so. He, that considers, what great operations divers of the Hermeticks ascribe to this or that hypostatical principle, and how many qualities, according to them, must from it be derived, can scarce do other than expect, that a great change, as to those qualities, happening in a mixed body, should, at least, be accompanied with some notable action of, or alteration in the principle. And yet I have met with many instances, wherein qualities are produced, or abolished, or very much altered, without any manifest introduction, expulsion, or considerable change of the principle, whereon that quality is said to depend, or perhaps of either of the two others: as when a piece of fine silver, that having been nealed in the fire, and suffered to cool leisurely, is very flexible, is made stiff, and hard to bend, barely by a few strokes of a hammer. And a string of a lute acquires or loses a sympathy, as they call it, with another string of the same or another instrument, barely by being either stretched, so as to make an unison with it, or screwed up, or let down, beyond or beneath that degree of tension.

To multiply instances of this kind, would be to anticipate those, you will hereafter meet with in their due places. And therefore I shall pass on, from the first sort of phænomena, that favour not the chemical hypothesis about quali-

ties,

ties, to the other, which consists of those, wherein either, that does not happen, which, according to their hypothesis, ought to happen, or the contrary happens to what, according to their hypothesis, may justly be expected. Of this you will meet with instances hereafter; I shall now trouble you but with one, the better to declare my meaning. It is not unknown to those chemists, that work much in silver and in copper, that the former will endure ignition, and become red-hot in the fire, before it will be brought to fusion; and the latter is yet far more difficult to be melted down than the other: yet if you separately dissolve those two metals in aqua fortis, and by evaporation reduce them to crystals, these will be brought to fusion in a very little time, and with a very moderate heat, without breaking the glasses that contain them. If you ask a vulgar chemist the cause of this facility of fusion, he will, probably, tell you, without scruple, that it is from the saline parts of the aqua fortis, which, being imbodied in the metals, and of a very fusible nature, impart that easiness of fusion to the metals they are mixed with. According to which plausible explication one might well expect, that, if the saline corpuscles were exquisitely mingled with tin, they would make it far more fusible than of itself it is. And yet, as I have elsewhere noted, when I put tin into a convenient quantity of aqua fortis, the metal being corroded, subsided, as is usual, in the form of whites of eggs, which being well dried, the tin was so far from being grown more fusible by the addition of the saline particles of the menstruum, that, whereas it is known, that simple tin will melt long before it come to be red-hot, this prepared tin would endure, for a good while, not only a thorough ignition, but the blast of a double pair of bellows, (which we usually employed to melt silver and copper itself,) without being at all brought to fusion. And as for those Spagyrists, that admit, as most of them are granted to do, that all kinds of metals may be turned into gold, by a very small proportion of what they call the philosophers elixir, one may, I think, shew them, from their own concessions, that divers qualities may be changed, even in such constant bodies as metals, without the addition of any considerable proportion of the simple ingredients, to which they are wont to ascribe those qualities; provided the agent, (as an efficient, rather than as a material cause,) be able to make a great change in the mechanical affections of the parts whereof the metal it acts on is made up. Thus if we suppose a pound of silver, a pound of lead, and a pound of iron to be transmuted into gold, each by a grain of the powder of projection, this tinging powder, as a material cause, is inconsiderable, by reason of the smallness of its bulk, and as an efficient cause, it works differing, and even contrary effects, according to the disposition, wherein it finds the metal to be transmuted, and the changes it produces in the constituent texture of it. Thus it brings quicksilver to be fixed, which it was not before, and deprives it of the fluidity, which it had before; it brings silver to be indissolvable in aqua fortis, which readily dissolved it before, and dissoluble in aqua regis, which before would not touch it; and which is very considerable to our present purpose, whereas it makes iron much more fusible than *Mars*, it makes lead much less fusible than whilst it retained its pristine form, since *Saturn* melts ere it come to ignition, which gold requires to bring it to fusion. But this is proposed only as an argument *ad hominem*, till the truth of the transmutation of metals into gold, by way of projection, be sufficiently proved, and the circumstances, and phænomena of it, particularly declared.

I must not forget to take notice, that some learned modern chemists would be thought to explicate divers of the changes, that happen to bodies in point of odours, colours, &c. by saying, that, in such alterations, the sulphur, or other hypostatical principle, is intraverted or extraverted, or, as others speak, inverted. But I confess, to me these seem to be rather new terms, than real explications. For, to omit divers of the arguments mentioned in this present treatise, that may be applied to this way of solving the phænomena of qualities, one may justly object, that the supposed extraversion or intraversion of sulphur, can by no means reach to give an account of so great a variety of odours, colours, and other qualities, as may be found in the changed portions of matter we are speaking of. And, which is more, what they call by these and the like names, cannot be done without local motion transposing the particles of the matter, and consequently producing in it a change of texture, which is the very thing we would infer, and which being supposed, we may grant sulphur to be oftentimes actually present in the altered bodies, without allowing it to be always necessary to produce the alterations in them, since corpuscles, so conditioned and contexed, would perform such effects, whether sulphur, as such, did, or did not make up the subject matter of the change.

AND now I shall conclude, and partly recapitulate what has been delivered in this and the two foregoing chapters, with this summary consideration; that the chemist's salt, sulphur, and mercury themselves are not the first and most simple principles of bodies, but rather primary concretions of corpuscles, or particles more simple than they, as being endowed only with the first, or most radical, (if I may so speak) and most catholick affections of simple bodies, namely, bulk, shape, and motion, or rest; by the different conventions or coalitions of which, minutest portions of matter are made those differing concretions, that chemists name salt, sulphur, and mercury. And to this doctrine it will be consonant, that several effects of this or that spagyrical principle need not be derived from salt, for instance, or sulphur as such, but may be explained by the help of some of those corpuscles, that I have lately called more simple and radical; and such explications being more simple and mechanical, may be thought, upon that score, more fundamental and satisfactory.

CHAP.

CHAP. VIII.

I KNOW it may be objected, in favour of the chemiſts, that as their hypoſtatical principles, ſalt, ſulphur, and mercury, are but three, ſo the corpuſcularian principles are but very few; and the chief of them bulk, ſize, and motion, are but three neither; ſo that it appears not, why the chemical principles ſhould be more barren than the mechanical. To which allegation I anſwer, that, beſides that theſe laſt named principles are more numerous, as taking in the poſture, order, and ſituation, the reſt, and, above all, the almoſt infinitely diverſifiable contextures of the ſmall parts, and the thence reſulting ſtructures of particular bodies, and fabrick of the world: beſides this, I ſay, each of the three mechanical principles, ſpecified in the objection, though but one in name, is equivalent to many in effect; as figure, for inſtance, comprehends not only triangles, ſquares, rhomboides, trapeziums, and a multitude of polygons, whether ordinate or irregular; but, beſides cubes, priſms, cones, ſpheres, cylinders, pyramids, and other ſolids of known denominations, a ſcarce numerable multitude of hooked, branched, eel-like, ſcrew-like, and other irregular bodies; whereof, though theſe, and ſome others, have diſtinct appellations, yet the greateſt part are nameleſs; ſo that it need be no wonder, that I ſhould make the mechanical principles ſo much more fertile, that is, applicable to the production and explication of a far greater number of phænomena, than the chemical; which, whilſt they are conſidered but as ſimilar bodies, that are ingredients of mixed and compounded ones, are chiefly variable but by the greater or leſſer quantity, that is employed by nature or art to make up the mixed body. And painters obſerve, that black and white, though mixed in differing proportions, will ſtill make but lighter and darker greys. And if it be ſaid, that theſe ingredients, by the texture reſulting from their mixtures, may acquire qualities, that neither of them had before; I ſhall anſwer, that to alledge this, is, in effect, to confeſs, that they muſt take in the mechanical principles (for to them belongs the texture or ſtructure of bodies) to aſſiſt the chemical ones. And, on this occaſion, to borrow an illuſtration from our unpubliſhed dialogue of the requiſites of a good hypotheſis, I ſhall add, that a chemiſt, that ſhould pretend, that becauſe his three principles are as many as thoſe of the Corpuſcularians, they are as ſufficient, as theſe, to give an account of the book of nature, methinks, I ſay, he would do like a man, that ſhould pretend, that, with four and twenty words, he would make up a language, as well as others can with the four and twenty letters of the alphabet, becauſe he had as many words already formed, as they had of bare letters; not conſidering, that, inſtead of the ſmall number of variations, that can be made of his words by prepoſitions and terminations, the letters of the alphabet being variouſly combined, placed, and reiterated, can be eaſily made to compoſe not only his four and twenty words, with their variations, but as many others, as a whole language contains.

CHAP. IX.

NOTWITHSTANDING all, that I have been obliged to ſay to the diſadvantage of the chemical principles, in reference to the explication of qualities, I would not be thought to grant, that the Peripateticks have reaſon to triumph, as if their four elements afforded a better theory of qualities. For, if I had, together with leiſure enough to perform ſuch a taſk, any obligation to undertake it, I preſume, it would not be difficult to ſhew, that the Ariſtotelian doctrine, about particular qualities, is liable to ſome of the ſame objections with the chemical, and to ſome others no leſs conſiderable; and that, to derive all the phænomena their doctrine ought to ſolve, from ſubſtantial forms and real qualities elementary, is to impoſe on us a theory more barren and precarious, than that of the Spagyriſts.

THAT, to derive the particular qualities of bodies from thoſe ſubſtantial forms, whence the ſchools would have them to flow, is but an inſufficient and unfit way of accounting for them, may appear by this, that ſubſtantial forms themſelves are things, whoſe exiſtence many learned philoſophers deny, whoſe theory many of them think incomprehenſible, and the moſt candid and judicious of the Peripateticks themſelves confeſs it to be very abſtruſe; ſo that, from ſuch doubtful and obſcure principles, we can hardly expect clear explications of the nature and phænomena of qualities; not to urge, that the Ariſtotelian definitions, both of qualities in general, and of divers of the more familiar qualities in particular, as heat, cold, moiſture, diaphaneity, &c. are far enough from being clear and well framed, as we elſewhere have occaſion to ſhew.

ANOTHER thing, which makes the ſcholaſtic doctrine of qualities unſatisfactory, is, that it ſeldom ſo much as attempts to teach the manner, how the qualities themſelves, and their effects or operations, are produced. Of this you may elſewhere find an inſtance given in the quality, that is wont to be firſt in the liſt, viz. that of heat; which, though it may intelligibly and probably be explicated by the corpuſcular hypotheſis, yet, in the Peripatetic account, that is given of it, is both too queſtionable and too ſuperficial to give much content to a rational inquirer. And indeed to ſay, that a ſubſtantial form (as that of *the fire*) acts by a quality, (called heat) whoſe nature it is to produce ſuch an effect, (as to ſoften wax, or harden clay,) ſeems to be no other in ſubſtance, than to ſay, that it produces ſuch an effect by ſome power it has to produce it. But what that power is, and how it operates, is that, which, though we moſt deſire to know, we are left to ſeek. But to proſecute the imperfections of the Peripatetic hypotheſis, were to intrench upon another diſcourſe, where they are more fully laid open. And therefore I ſhall now but lightly glance upon a couple of imper-

imperfections, that more particularly relate to the doctrine of qualities.

AND first, I do not think it a convincing argument, that is wont to be employed by the Aristotelians for their elements, as well as by the chemists for their principles, that, because this or that quality, which they ascribe to an element or a principle, is found in this or that body, which they call mixed, therefore it must owe that quality to the participation of that principle or element. For, the same texture of parts, or other modification of matter, may produce the like quality in the more simple and the more compounded body, and they may both separately derive it from the same cause, and not one from the participation of the other. So water, and earth, and metals, and stones, &c. are heavy, upon the account of the common cause of gravity, and not because the rest partake of the earth; as may appear in elementary water, which is as simple a body as it, and yet is heavy: so water and oil, and exactly dephlegmed spirit of wine, and mercury, and also metals and glass of antimony, and minium or calcined lead, whilst these three are in fusion, are fluid, being made so by the variously determined motions of their minute parts, and other causes of fluidity, and not by the participation of water, since the arid calces of lead and antimony are not like to have retained, in the fire, so volatile a liquor as water, and since fluidity is a quality, that mercury enjoys in a more durable manner than water itself: for that metalline liquor, as also spirit of wine well rectified, will not be brought to freeze with the highest degree of cold of our sharpest winters, though a far less degree of cold would make water cease to be fluid, and turn it into ice.

To this I shall only add, in the second place, that it is not unpleasant to see, how arbitrarily the Peripateticks derive the qualities of bodies from their four elements, as if, to give an instance in the lately named quality, liquidity, you shew them exactly dephlegmed spirit of wine, and ask them, whence it has its great fluidness, they will tell you, from water, which yet is far less fluid than it; and this spirit of wine itself is much less so than the flame, into which the spirit of wine is easily resoluble. But if you ask, whence it becomes totally inflammable, they must tell you, from the fire; and yet the whole body, at least, as far as sense can discover, is fluid, and the whole body becomes flame, (and then is most fluid of all;) so that fire and water, as contrary as they make them, must both be, by vast odds, predominant in the same body. This spirit of wine also, being a liquor, whose least parts, that are sensible, are actually heavy, and compose a liquor, which is seven or eight hundred times as heavy as air of the same bulk, which yet experience shews not to be devoid of weight, must be supposed to abound with earthy particles; and yet this spirituous liquor may, in a trice, become flame, which they would have to be the lightest body in the world.

BUT, to enlarge on this subject, would be to forget, that the design of this tract engages me to deal not with the Peripatetic school, but the Spagyrical. To which I shall therefore return, and give you this advertisement about it, that what I have hitherto objected, is meant against the more common and received doctrine about the material principles of bodies reputed mixed, as it is wont, by vulgar chemists, to be applied to the rendering an account of the qualities of substances corporeal; and therefore I pretend not, that the past objections should conclude against other chemical theories, than that, which I was concerned to question. And if adept philosophers, (supposing there be such) or any other more than ordinarily intelligent Spagyrists, shall propose any particular hypothesis, differing from those, that I have questioned, as their doctrine and reasons are not yet known to me; so I pretend not, that the past arguments should conclude against them, and am willing to think, that persons advantaged with such peculiar opportunities, to dive into the mysteries of nature, will be able to give us, if they shall please, a far better account of the qualities of bodies, than what is wont to be proposed by the generality of chemists.

THUS, dear *Pyrophilus*, I have laid before you some of the chief imperfections I have observed, in the vulgar chemists doctrine of qualities; and consequently I have given you some of the chief reasons, that hinder me from acquiescing in it. And as my objections are not taken from the scholastical subtleties, nor the doubtful speculations of the Peripateticks, or other adversaries of the Hermetick philosophy, but from the nature of things, and from chemical experiments themselves; so, I hope, if any of your spagyrical friends have a mind to convince me, he will attempt to do it by the most proper way, which is, by actually giving us clear and particular explications, at least, of the grand phænomena of qualities; which, if he shall do, he will find me very ready to acquiesce in a truth, that comes ushered in, and endeared by so acceptable and useful a thing, as a philosophical theory of qualities.

REFLECTIONS UPON THE HYPOTHESIS OF ALCALI and ACIDUM.

CHAP. I.

I PRESUME, it will not be difficult to discern, that much of what has been said about the imperfection of the vulgar chemical doctrine concerning qualities, may, with easy variations, be applied to some other hypotheses, that are of kin to that doctrine, and particularly to their theory, that would derive both the qualities of bodies, and the rest of the phænomena of nature from what they call acidum and alcali. For though these two differences may be met with in a great number and variety of bodies, and consequently the consideration of them may frequently enough be of good use, (especially to Spagyrists, and physicians, when they are conversant about the secondary and, if I may so call them, chemical causes and operations of divers mixed bodies;) yet I confess I cannot acquiesce in this hypothesis of alcali and acidum, in the latitude, wherein I find it urged and applied by the admirers of it, as if it could be usefully substituted in the place of matter and motion.

THE hypothesis being in a sort subordinate to that of the *tria prima*, in ascribing to two contrary saline principles, what vulgar chemists do to their salt, sulphur, and mercury; most of the objections we have made against the vulgar chemical doctrine, may, as I lately intimated, be applied, by a little variation, to this, and therefore I shall need but to touch upon the main things, that keep me from acquiescing in this hypothesis.

CHAP. II.

AND first, it seems precarious to affirm, that in all bodies, or even in all the sensible parts of mixeds, acid and alcalizate parts are found; there not having been, that I know, any experimental induction made of particulars any thing near numerous enough to make out so great an assertion, and in divers bodies, wherein experience is vouched for the inexistence of these principles, that inexistence is indeed proved, not by direct and clear experience, but upon a supposition, that such and such effects flow from the operations of the assumed principles.

SOME Spagyrists, when they see aqua-fortis dissolve filings of copper, conclude from thence, that the acid spirits of the menstruum meet in the metal with an alcali upon which they work; which is but an unsafe way of arguing, since good spirit of urine, which they take to be a volatile alcali, and which will make a great conflict with aqua-fortis, will, as I have elsewhere noted, dissolve filings of copper both readily enough and more genuinely than the acid liquor is wont to do. So, when they see the magistery of pearl or coral made by dropping oil of tartar into the solutions of those bodies made with spirit of vinegar, they ascribe the precipitation to the fixed alcali of the tartar, that mortifies the acidity of the spirit of vinegar; whereas the precipitation would no less ensue, if, instead of alcalizate oil of tartar, we employ that highly acid liquor which they call *oleum sulphuris per campanam*.

I think also it may be doubted, whether those, I reason with, are so certain as they suppose, that at least, when they can manifestly discover an acid, for instance, in a body, the operation of that body upon another, which they judge to abound with an alcali, must be the effect of a conflict between those two jarring principles, or, if I may so call them, duellists. For an acid body may do many things, not simply as an acid, but on the score of a texture or modification, which endows it with other qualities as well as acidity, whose being associated with those other qualities in some cases, may be but accidental to the effect to be produced; since by one or more of these other qualities the body may act in cases, where prejudice may make a chemist consider nothing but acidity. Thus when some chemists see an acid menstruum, as aqua-fortis, spirit of salt, oil of vitriol, &c. dissolve iron, they presently ascribe the effect to an acidity of the liquors, whereas well dephlegmed urinous spirits, which they hold to have a great antipathy to acids, will

will, as I have tried in ſome of them, readily enough diſſolve crude iron even in the cold. And on the other ſide, mercury will not work on the filings of iron, though this be ſo open a metal, that even weak liquors will do it; and yet if one ſhould urge, that quick-ſilver readily diſſolves gold in amalgamation, he may expect to be told, according to their doctrine, that mercury has in it an occult acid, by which it performs the ſolution; whereas it ſeems much more probable, that mercury has corpuſcles of ſuch a ſhape and ſize, as fit them to inſinuate themſelves into the commenſurate pores they meet with in gold, but make them unfit to enter readily the pores of iron, to which nature has not made them congruous; as on the other ſide the ſaline corpuſcles of aqua-fortis will eaſily find admiſſion into the pores of iron, but not into thoſe of gold, to which they do not correſpond as they do to the others. And when a knife, whoſe blade is touched with a load-ſtone, cuts bread and takes up filings of iron, it does neither of them upon the ſcore of alcali and acidum, but the one upon the viſible ſhape, and the ſtiffneſs of the blade, and the other upon the latent contrivance or change of texture produced by the operation of the load-ſtone in the particles, that compoſe the ſteel.

THIS may perhaps be farther illuſtrated by adding, that when blue vitriol, being beaten and finely ſearced, makes a white powder, that whiteneſs is a quality, which the powder has not, as being of a vitriolate nature. For rock-cryſtal or Venice-glaſs, being finely beaten, will have the ſame operation on the eye, but it proceeds from the tranſparency of the body and the minuteneſs, multitude, and confuſed ſituation of the corpuſcles, that make up the powder. And therefore, if other bodies be brought by comminution into parts endowed with ſuch mechanical affections, as we have named; theſe aggregates will act upon the organs of ſight, as white bodies.

CHAP. III.

AND this leads me to another exception againſt the hypotheſis of the duelliſts, which is, that the framers of it ſeem arbitrarily to have aſſigned provinces or offices to each of their two principles, as the chemiſts do to each of their *tria prima*, and the Peripateticks to each of their four elements. For it is not enough to ſay, that an acid, for inſtance, as ſuch, performs theſe things, and an alcali ſo many others, that they divide the operations and phænomena of nature, or at leaſt (as ſome, more cautious, are content to ſay) of mixed bodies between them; ſince aſſertions of ſuch great moment ought not to be advanced or received without ſufficient proof. And perhaps the very diſtribution of ſalts into acids and alcalies hath ſomewhat of arbitrary in it, ſince others may, without aſſuming much more, take the freedom to diſtribute them otherwiſe, there being not only ſeveral things, wherein acids and alcalies agree, but alſo ſeveral things, wherein ſalts of the ſame denomination widely differ. As for inſtance, ſome alcalies, according to thoſe I reaſon with, are, like ſalt of tartar, fixed, and will endure the violence of the fire; others, like ſalt of urine or harts-horn, are exceedingly fugitive, and will be driven up with a ſcarce ſenſible degree of heat; ſome, as ſalt of tartar, will precipitate the ſolution of ſublimate into an orange-tawny; others, as ſpirit of blood and harts-horn, precipitate ſuch a ſolution into a milky ſubſtance. Oil of tartar will very ſlowly operate upon filings of copper, which ſpirit of urine and harts-horn will readily diſſolve in the fire.

AND among acids themſelves the difference is no leſs, if not much greater. Some of them will diſſolve bodies, that others will not, as aqua-fortis will diſſolve ſilver and mercury, but leave gold untouched; or as aqua-regis, though made without ſal-armoniac, that diſſolves gold readily, will diſſolve mercury but ſcurvily, and ſilver not at all. And this may happen, when the menſtruum, that will not diſſolve the body, is reputed much ſtronger than that, which does; as dephlegmed ſpirit of vinegar will diſſolve lead, reduced to minute parts in the cold; which is an effect, that chemiſts are not wont to expect from ſpirit of ſalt. Nay, which is more, one acid will precipitate what another has diſſolved, and contrarily; as ſpirit of ſalt will precipitate ſilver out of ſpirit of nitre. And I found oil of vitriol to precipitate bodies of divers kinds, minerals and others, out of ſome acid menſtruums, particularly ſpirit of vinegar.

To this might be added the properties, peculiar to ſome particular acids, as that ſpirit of nitre or aqua-fortis will diſſolve camphire into an oil, and coagulate common oil into a conſiſtent and brittle ſubſtance like tallow; and, though it will both corrode ſilver, copper, lead, and mercury, and keep them diſſolved, it will quickly let fall almoſt the whole body of tin, very ſoon after it has corroded as much as it can of it. By all which, and ſome other like inſtances, I am induced to queſtion whether the acidum and alcali, we are ſpeaking of, have the ſimplicity, that philoſophy requires in principles; and ſhall be kept from wondering, if others ſhall think it as free for them to conſtitute other principles, as it is for the learned men I reaſon with, to pitch upon acidum and alkali.

AND ſome perhaps will be bold to ſay, that, ſince the former of thoſe principles comprehend ſuch a number of bodies, that are, many of them, very differing, and ſome of them directly contrary in their operations, it ſeems a ſlight and not philoſophical account of their nature, to define an acid by its hoſtility to an alcali, which, they will ſay, is almoſt as if one ſhould define a man, by ſaying, that he is an animal, that is at enmity with the ſerpent; or a lion, that he is a fourfooted beaſt, that flies from a crowing cock.

CHAP. IV.

BUT although one of the chiefeſt conditions, that philoſophers may juſtly require in principles, is, that, being to explain other things, they should be very clear themselves; yet I do not much wonder, that the definitions given us of acidum and alcali should be but unaccurate and superficial, since I find not, that they have themselves any clear and determinate notion or sure marks, whereby to know them distinctly, without which chemists will scarce be able to form clear and settled notions of them. For to infer, as is usual, that, because a body dissolves another, which is dissoluble by this or that known acid, the solvent must also be acid; or to conclude, that, if a body precipitates a dissolved metal out of a confessedly acid menstruum, the precipitant must be an alcali; to argue thus, I say, it is unsecure; since, not to repeat what I said lately of copper, I found, that filings of spelter will be dissolved as well by some alcalies, (as spirit of sal armoniac) as by acids. And bodies may be precipitated out of acid menstruums, both by other acids, and by liquors, where there appears not the least alcali: as I have found, that a solution of tin-glass, made in aqua-fortis, would be precipitated both by spirit of salt, and by common, or rain water. And as for the other grand way, that chemists employ, to distinguish acids and alcalies, namely by the heat, commotion, and bubbles, that are excited upon their being put together, that may be no such certain sign, as they presume, they having indeed a dependence upon particular contextures, and other mechanical affections, that chemists are not wont to take any notice of. For almost any thing, that is fitted variously and vehemently to agitate the minute parts of a body, will produce heat in it; and so, though water be neither an acid, nor an alcalizate liquor, yet it would quickly grow very hot, not only with the highly acid oil of vitriol, but (as I have more than once purposely tried and found) with the fiery alcalizate salt of tartar. And it is to be noted, that neither in the one, nor the other of these incalescent mixtures, there is produced any such visible or audible conflict, as, according to the doctrine of the chemists I reason with, one would expect. And as for the production of bubbles, especially if accompanied with a hissing noise, neither is that such a certain sign as chemists imagine: for the production of bubbles is not a necessary effect, or concomitant of heat excited by conflicts, but depends very much upon the peculiar disposition of bodies put together to extricate, produce, or intercept particles of air, (or steams, for the time equivalent to them;) and therefore as oil of vitriol, mixed in a due proportion with fair water, may be brought to make the water too hot to be held in one's hand, without exciting bubbles; so I have found, by trials purposely made, that alcalizate spirit of urine drawn from some kinds of quick lime, being mixed with oil of vitriol moderately strong, would produce an intense heat, whilst it produced either no manifest bubbles at all, or scarce any, though the urinous spirit was strong, and in other trials operated like an alcali; and although also with spirit of urine, made *per se* the common way, the oil of vitriol will produce a great hissing, and a multitude of conspicuous bubbles. On the other side, I have sometimes, though not so constantly, found, that some acid spirits, especially that of verdigrease made *per se*, would, when poured upon salt of tartar, make a conflict with it, and produce a copious froth, though we observed it not to be accompanied with any manifest heat. And I elsewhere mention two bodies, upon whose putting together, numerous bubbles would, for a long time, and not without noise, be generated, and succeed one another, though I could perceive no heat all to accompany this tumult.

As for the taste, which by many is made a great touchstone, whereby to know acids and alcalies, I consider, that there is a multitude of mixed bodies, wherein we can so little discern by the taste, which of the principles is predominant, that this sense would not oblige one to suspect, much less to conclude, there were one grain of either of them to be found there; such bodies are diamonds and rubies, and most gems, besides many ignobler stones, and gold, and silver, and mercury, and I know not how many other bodies. On the other side, there are bodies, that abound with acid or alcalizate salts, which either have no taste, or a quite differing one from that of the chemical principle. As though Venice-glass be in great part composed of a fixed alcali; yet to the tongue it is insipid, and crystals of lune, and of lead, made with aqua fortis, and containing great store of the acid particles of the menstruum, have nothing of acidity in the mouth, the latter having a saccharine sweetness, and the former an extreme bitterness. And even in vegetable substances, that have a manifest taste, it is not so easy to know by that, whether it be the acid, or the alcalizate principle, that is predominant in them; as in the essential oils of spices, and other vegetables. And in the gross empyreumatical oils of woods, and even in high rectified spirit of wine, which therefore some will have to be an alcalizate liquor, and others list it among acids, though I did not find it neither to be destroyed, or much altered, by being put upon coral, or salt of tartar, as would happen to an acid menstruum, nor yet by being digested with, and distilled from sea salt, as might be probably expected from an alcalizate one: and among those very bodies, which their tastes persuade chemists to reckon among acids, one may (according to what I formerly noted) observe so great a difference and variety of relishes, that, perhaps, without being too severe, I may say, that if I were to allow acids to be one principle, it should be only in some such metaphysical sense, as that wherein air is said to be one body, though it consist of the associated effluviums of a multitude of corpuscles of very differing natures, that agree in very little, save in their being minute enough to concur to the composition of a fluid aggregate, consisting of flying parts. But having dwelt longer than I intended on one objection, it is time, that I proceed to those that remain.

CHAP. V.

ANOTHER particular, I am unſatisfied with in the hypotheſis of alcali and acidum, is, that it is in divers caſes either needleſs or uſeleſs to explain the phænomena of qualities, there being ſeveral of theſe produced, deſtroyed, or altered, where there does not appear any acceſſion, receſs, or change of either of thoſe two principles; as when fluid water by hard beating is turned into conſiſtent froth, and when tranſparent red coral is, barely by being beaten and ſifted finely, changed into a white and opacous powder; and as when a very flexible piece of fine ſilver being hammered is brought to have a briſk ſpring, and after a while will, inſtead of continuing malleable, crack or cleave under the hammer; and as when (to diſpatch and omit other inſtances) a ſufficiently thin leaf of gold, held between the light and the eye, appears green.

ANOTHER thing (of kin to the former,) that I like not in the doctrine of acidum and alcali, is, that though the patrons of it, whilſt they would ſeem to conſtitute but two principles, are fain, as I lately intimated, to make I know not how many differing ſorts of acids, beſides ſome variety of alcalies; yet their principles are too few and narrow, to afford any ſatisfactory explication of the phænomena. For I fear, it will be very difficult for them to give a rational account of gravity, ſpringineſs, light, and emphatical colours, ſounds, and ſome other qualities, that are wont to be called manifeſt; and much more of ſeveral, that are confeſſed to be occult, as electricity, and magnetiſm; in which laſt I ſee not, how the affirming, that there is in the magnet an acid and an alcali, and that theſe two are of contrary natures, will help to explain, how a load-ſtone does, as they ſpeak, attract the ſame end of a poiſed needle, with one of its poles, which it will drive away with the other, and determine that needle, when freely placed, to point north and ſouth, and enable it to communicate by its bare touch the ſame properties, and abundance of other ſtrange ones, to another piece of ſteel. But I forbear to alledge particular examples referable to the ſeveral qualities above-mentioned, whether manifeſt or hidden, becauſe that in great part is already done in our notes about particular qualities, in which it will appear, how little able the employing of alcali and acidum will be to afford us an account of many things. And though I enlarge not here on this objection, yet I take it to be of that importance, that, though there were no other, this were enough to ſhew, that the hypotheſis, that is liable to it, is inſufficient for the explication of qualities; and therefore it will not, I preſume, be thought ſtrange, that I add, that as for thoſe, that would extend this narrow chemical doctrine to the whole object of natural philoſophy, they muſt do more, than I expect they will be able before they can make me their proſelyte, there being a multitude of phænomena in nature (divers whereof I elſewhere take notice of in reference to the chemiſt's philopſophy) in which what acidum and alkali have to do, I confeſs, I do not underſtand.

CHAP. VI.

THE laſt thing (which comprizes ſeveral others) that ſeems to me a defect in the doctrine of alcali and acidum, is, that divers, if not moſt of thoſe very things, that are pretended to be explicated by them, are not ſatisfactorily explicated, ſome things being taken into the explications, that are either not fundamental enough, or not clearly intelligible, or are chargeable with both thoſe imperfections.

AND firſt I am diſſatisfied with the very fundamental notion of this doctrine, namely a ſuppoſed hoſtility between the tribe of acids and that of alkalies, accompanied, if you will have it ſo, with a friendſhip or ſympathy with bodies belonging to the ſame tribe or family. For I look upon amity and enmity as affections of intelligent beings; and I have not yet found it explained by any, how thoſe appetites can be placed in bodies inanimate and devoid of knowledge, or of ſo much as ſenſe. And I elſewhere endeavour to ſhew, that what is called ſympathy and antipathy between ſuch bodies does, in great part, depend upon the actings of our own intellect, which ſuppoſing in every body an innate appetite to preſerve itſelf both in a defenſive and an offenſive way, inclines us to conclude, that that body, which, though deſignleſly, deſtroys or impairs the ſtate or texture of another body, has an enmity to it, though perhaps a ſlight mechanical change may make bodies, that ſeem extremely hoſtile, ſeem to agree very well and co-operate to the production of the ſame effects. As if the acid ſpirit of ſalt and the volatile alkali (as they will have it) that is commonly called ſpirit of urine be put together, they will, after a ſhort, though fierce conflict, upon a new contexture unite together into a ſalt, little, if at all, differing from ſal armoniac, in which the two reconciled principles will amicably join in cooling of water, diſſolving ſome metalline bodies, and producing divers other effects. And ſo, if upon a ſtrong ſolution of ſalt of pot-aſhes, or of ſalt of tartar, good ſpirit of nitre be dropped in a due proportion, after the heat and tumult and ebullition are over, the acid and the alkalizate ſalts will convene into ſuch a concretion as ſalt-petre, which is taken to be a natural body, either homogeneous, or at leaſt conſiſting of parts, that agree very friendly together, and conſpired to conſtitute the particular kind of ſalt, that chemiſts call nitre.

BUT the ſympathy and antipathy, that is ſaid to be betwixt inanimate bodies, I elſewhere more particularly conſider; and therefore I ſhall now add in the ſecond place, that the explications made of phænomena, according to the doctrine of alkali and acidum, do not, in my apprehenſion, perform what may be juſtly expected from philoſophical explications. It is ſaid indeed, that the acidum working on the alkali, or this upon that, produces

duces the effect propofed; but that is only to tell us, what is the agent, that operates, and not the manner of the operation, or the means and procefs, whereby it produces the effect propofed, and it is this modus, that inquifitive naturalifts chiefly defire to learn. And if it be faid, that it is by the mutual hoftility of the principles, that the effect is produced, it may be anfwered, that befides that that hoftility itfelf is not, as we have juft now obferved, a thing clear, if fo much as intelligible; this is fo general and indeterminate a way of explicating things, as can afford little or no fatisfaction to a fearching and cautious naturalift, that confiders how very numerous and very various the phænomena of qualities are.

CHAP. VII.

TO clear up and to countenance what I have been now faying, I fhall only take notice of fome few obvious phænomena of one of the moft familiar operations wherein acidum and alkali are fuppofed to be the grand agents. It is known to the very boys of chemifts, that aqua regis will diffolve gold, copper and mercury, and that with thefe metals, efpecially with the fecond, it will produce an intenfe degree of heat. If now the caufe of this heat be demanded, it may be expected, that the patrons of the Duellifts will anfwer, that it is from the action of the acid falts of the menftruum upon the alkali they meet with in the metals. But not to mention how many things are here prefumed, not proved; nor that I know fome acid menftruums, and fome much more evidently alkalizate bodies than thefe metals are, which yet do not upon their mixtures produce any fenfible heat; not, I fay, to mention thefe, it is eafy to difcern, that this anfwer names indeed two fuppofed efficients of heat, but does not explicate or declare how thefe agents produce that quality, which depends upon a certain vehement and various agitation of the fingly infenfible parts of bodies, whether the Duellifts, or any other, though very differing caufes, put them into a motion fo modified. And therefore gold and copper, by bare concuffion, may be brought to an intenfe degree of heat, without the acceffion of any acid parts to work upon them. But then further, when we are told, that aqua regis by its acidity working on the metalline alkali makes a diffolution of the metal; I am told indeed, what they think to be the agent in this change, but not at all fatisfied how this agent effects it; for, copper being a very hard metal, and gold generally efteemed by chemifts the clofeft and compacteft body in nature, I would gladly know, by what power and way fuch weak, and probably either brittle or flexible bodies, as acid falts, are enabled with that force to disjoin fuch folid and clofely coherent copufcles, as make up the vifible maffes of copper and gold, nay, and fcatter them with that violence, as, perhaps to tofs up multitudes of them into the air. And fince in the diffolution of thefe metals there is another phænomenon to be accounted for, as well as the forcing of the parts afunder, namely the fuftentation of the metal in the menftruum, the chemifts would have much informed me, if they had well explained, how their acidum and alkali is able to fuftain and give fluidity to the corpufcles of the diffolved metal, which though it be but copper, is nine times as heavy as a bulk of water equal to it, and if it be gold, is nineteen times heavier than the liquor, that muft keep it from finking; and at leaft divers times heavier in fpecie than the falts, that are mingled with the aqueous parts, can make the menftruum compofed of them both. Whereas trial has affured me, that, if a piece of wax, or any other fuch matter, be made by lefs than the hundredth part heavier than an equal bulk of water, it will, when thoroughly immerfed, fall to the bottom, and reft there. I might alfo afk a further queftion about thefe diffolutions, as why, whereas aqua regis diffolves mercury, without being much changed in colour by it, gold retains its own citrinity or yellownefs in the folvent, and the folution of copper is of a colour, which being greenifh-blew is quite differing from that of the metal, that affords it, as well as from that of the folvent? And I might recruit thefe with other queries not impertinent, but that thefe may fuffice (for a fample) on this occafion, and allow me to conclude this chapter, by reprefenting one thing, which I would gladly recommend and inculcate to you, namely, that "Thofe hypothefes do not a little hinder the progrefs of humane knowledge, that introduce morals and politicks into the explications of corporeal nature, where all things are indeed tranfacted according to laws mechanical."

CHAP. VIII.

I MIGHT eafily have been more copious in the inftances annexed to the foregoing animadverfions, but that, being defirous to be fhort as well as clear, I purpofely declined to make ufe of divers others, that feemed proper to be employed, and indeed might fafely enough have been fo, becaufe thofe I have mentioned and efpecially thofe (which make a great part of them) that are mechanical, are not liable to the fame exceptions, that I forefaw might be made to elude the force of the examples I paffed by. And though I think I could very well make thofe forefeen objections appear groundlefs or unfatisfactory; yet that could fcarce be done without engaging in controverfies, that would prove more tedious than I judged them neceffary.

AND yet, although what I have faid in this excurfion be but a part of what I could fay, I would not be thought to have forgot what I intimated at the beginning of it. For though the reafons I alledged keep me from acquiefcing in the doctrine of alkali and acidum, as it is propofed ander the notion of a philofophical hypothefis, fuch as the Cartefian or Epicurean, which are each of them alledged by their embracers to be mechanical, and of a very Catholick extent; yet I deny not, that the confideration of the Duellifts (or the two jarring principles of alkali and acidum) may be of good ufe to Spagyrifts and phyficians, as I elfe-elſewhere further declare. Nor do I pretend by the paſt diſcourſe, that queſtions one doctrine of the chemiſts, to beget a general contempt of their notions, and much leſs of their experiments. For the operations of chemiſtry may be miſapplied by the erroneous reaſonings of the artiſts, without ceaſing to be themſelves things of great uſe, as being applicable, as well to the diſcovery or confirmation of ſolid theories, as the production of new phænomena, and beneficial effects. And though I think, that many notions of *Paracelſus* and *Helmont*, and ſome other eminent Spagyriſts, are unſolid, and not worthy the veneration, that their admirers cheriſh for them; yet divers of the experiments, which either are alledged to favour theſe notions, or on other accounts are to be met with among the followers of theſe men, deſerve the curioſity, if not the eſteem, of the induſtrious inquirers into nature's myſteries. And looking upon chemiſtry in groſs as a diſcipline ſubordinate to phyſicks, even mechanical philoſophers may juſtly, in my opinion, think favourably of it, ſince, whatever imperfections, or, if they pleaſe, extravagancies there may be in the principles and explications of *Paracelſus* or other leading artiſts, theſe faults of the theorical part may be ſufficiently compenſated by the utilities, that may be derived from the practical part. And this I am the rather induced to ſay, becauſe the experiments, that chemiſtry furniſhes, may much aſſiſt a naturaliſt to rectify the erroneous theories, that oftentimes accompany them, and even thoſe (miſtakes) that are endeavoured to be evinced by them.

AND (to conclude) chemiſtry ſeems to deal with men, in reference to notions, as it does in reference to metals, aſſiſting wary men to detect the errors, unto which it may have miſled the unwary: for the ſame art, that has taught ſome to impoſe on others, (and perhaps themſelves firſt) by blanching copper, imitating gold, &c. does alſo ſupply ſaymaſters and refiners, with the means, by the cupel, cements, aqua fortis, &c. to examine, whether coins be true or falſe, and diſcover adulterate gold and ſilver to be counterfeit.

EXPE-

EXPERIMENTS AND NOTES, ABOUT THE MECHANICAL ORIGIN and PRODUCTION OF VOLATILITY.

ADVERTISEMENTS about the EXPERIMENTS and NOTES relating to CHEMICAL QUALITIES.

WHEN, after I had gone through the common operations of chemiſtry, I began to make ſome ſerious reflections on them, I thought it was pity, that inſtruments, that might prove ſo ſerviceable to the advancement of natural philoſophy, ſhould not be more ſtudiouſly and ſkilfully made uſe of to ſo good a purpoſe. I ſaw indeed, that divers of the chemiſts had, by a diligent and laudable employment of their pains and induſtry, obtained divers productions, and lighted on ſeveral phænomena, conſiderable in their kind, and indeed more numerous, than, the narrowneſs and ſterility of their principles conſidered, could well be expected. But I obſerved too, that the generality of thoſe, that buſy themſelves about chemical operations; ſome, becauſe they practiſe phyſick, and others, becauſe they either much wanted, or greedily coveted money, aimed, in their trials, but at the preparation of good medicines for the human body, or to diſcover the ways of curing the diſeaſes or imperfections of metals, without referring their trials to the advancement of natural philoſophy in general; of which moſt of the alchymiſts ſeem to have been ſo incurious, that not only they did not inſtitute experiments for that purpoſe, but overlooked and deſpiſed thoſe undeſigned ones, that occurred to them, whilſt they were proſecuting a preparation of a medicine, or a tranſmutation of metals. The ſenſe I had of this too general omiſſion of the chemiſts, tempted me ſometimes to try, whether I could do any thing towards the repairing of it by handling chemiſtry, not as a phyſician, or an alchymiſt, but as a meer naturaliſt, and ſo by applying chemical operations to philoſophical purpoſes. And, in purſuance of theſe thoughts, I remember I drew up a ſcheme of what I ventured to call a *chemia philoſophica*, not out of any affectation of a ſplendid title, but to intimate, that the chemical operations, there treated of, were not directed to the uſual ſcopes of phyſicians, or tranſmuters of metals, but partly to illuſtrate, or confirm ſome philoſophical theories by ſuch operations; and partly to explicate thoſe operations, by the help of ſuch theories.

BUT before I had made any great progreſs in the purſuit of this deſign, the fatal peſtilence, that raged in *London*, and in many other parts of *England*, in the years 1664 and 65, obliging me, among the reſt, to make ſeveral removes, which put me upon taking new meaſures, and engaging me in other employments of my time, made me ſo long neglect the papers I had drawn up, that, at laſt, I knew not where to find them, (though, I hope, they are not yet miſlaid beyond recovery,) which I was the leſs troubled at, becauſe the great difficulties, to be met with in ſuch an undertaking, did not a little diſcourage me, ſuch a taſk requiring, as well as deſerving, a perſon better furniſhed, than I had reaſon to think myſelf, with abilities, leiſure, chemical experiments, and conveniences, to try as many more, as should appear needful. But yet, to break the ice for any, that may hereafter think fit to set upon such a work, or, to shorten my own labour, if I should see cause to resume it myself, I was content to throw in, among my notes about other particular qualities, some experiments and observations about some of those, that I have elsewhere called chemical qualities, because it is chiefly by the operations of chemists, that men have been induced to take special notice of them. Of these notes I have assigned to some qualities more, and to some fewer, as either the nature or importance of the subject seemed to require, or my leisure and other circumstances would permit. And though I have not here handled the subjects they belonged to, as if I intended such a *chemia philosophica*, as I lately mentioned, because my design did not make it necessary, but did, perhaps, make it impertinent for me to do so; yet, in some of the larger notes, about volatility and fixedness, and especially about precipitation, I have given some little specimens of the theorical part of a philosophical account of those qualities, or operations, that, I hope, will not be wholly useless. I know, it may be objected, that I should have employed, for instances, some more considerable experiments, if not arcana; but, though possibly I am not altogether unfurnished with such, yet, aiming rather to promote philosophy, than appear a possessor of elaborate processes, I declined several experiments, that required either more skill, or more time, or more expence, than could be well expected from most readers, and chose rather to employ such experiments, as may be more easily or cheaply tried; and, which is mainly to be considered, being more simple, are more clearly intelligible, and more fit to have notions and theories built upon them; especially considering, that the doctrine of qualities being itself conversant about some of the rudimental parts, if I may so call them, of natural philosophy, it seemed unfit to employ intricate experiments, and whose causes were liable to many disputes, to settle a theory of them. In short, my design being to hold a taper, not so much to chemists, as to the naturalists, it was fit I should be less sollicitous to gratify the former, than to inform the latter.

EXPERIMENTS and NOTES, about the MECHANICAL ORIGIN and PRODUCTION of VOLATILITY.

CHAP. I.

AS far as I have yet observed, the qualifications or attributes, on whose account a portion of matter is found to be volatile, are chiefly four; whereof the three former most regard the single corpuscles, as such; and the last, the manner of their union in the aggregate or body they make up.

BUT before I enter upon particulars, give me leave to advertise you here, once for all, that, in the following notes about volatility and fixedness, when I speak of the corpuscles, or minute parts of a body, I do not mean strictly either the elementary parts, such as earth and water, or the hypostatical principles, such as salt, sulphur, or mercury; for these things come not here into consideration: but only such corpuscles, whether of a simple, compounded, or decompounded nature, as have the particles they consist of so firmly united, that they will not be totally disjoined, or dissipated, by that degree of fire or heat, wherein the matter is said to be volatile, or to be fixed. But these combined particles will, in their aggregate, either ascend, or continue unraised *per modum unius*, (as they speak) or as one entire corpuscle. As in a corpuscle of sal armoniac, whether it be a natural or factitious thing, or whether it be perfectly similar, or compounded of differing parts, I look upon the entire corpuscle, as a volatile portion of matter; and so I do on a corpuscle of sulphur, though experience shews, when it is kindled, that it has great store of acid salt in it, but which is not extricated by bare sublimation: and so colcothar of vitriol falls under our consideration, as a fixed body, without enquiring what cupreous or other mineral, and not totally fixed parts, may be united with the earthly ones; since the fires, we expose it to, do not separate them.

AND this being premised in the general, I now proceed to some particulars. And first, to make a volatile body, the parts should be very small. For, *cæteris paribus*, those, that are so, are more easily put into motion by the action of the fire, and other agents, and consequently more apt to be elevated, when, by the determination of the movent, the situation of the neighbouring bodies, or other mechanical circumstances, the agitated corpuscles can continue their motion with less resistance upwards, than any other way, (as either downwards, or horizontally.) And if, as it is highly probable, that, which in light bodies, or, at least, in most of them, is wont to pass for positive levity, be but a less degree of gravity, than that of those contiguous bodies, that raise them; it will happen, that, in very many cases, (for, I say, not in all,) the great proportion of the surface of a corpuscle to its bulk, (which

(which is usually greater in the lesser particles,) by making it more apt to be wrought on, either by the air agitated by the fire, or by the effluvia of kindled fuel, or by the impulse of the shaken corpuscles of the body itself, will much facilitate the elevation of such a minute particle, by exposing a greater portion of it to the action of the agent, as it will oftentimes also facilitate the renewed sustentation of such a small body in the air, which resists more the descent of particles, whose surfaces are large, than of others of the same gravity and bulk: as a leaf of paper displayed will much longer hover in the air, than if it were reduced into a ball or pellet. That this minuteness of particles may dispose them to be carried upwards, by the impulse of other bodies, and that of the agitated air, is very obvious to be observed: as we see, that horses in a highway, though they be not able, with the strokes of their feet, to make stones, or gravel, or clods of earth fly up, yet they will easily raise clouds of dust, oftentimes mingled with the smaller grains of sand. And, where timber is sawing, the same wind, that will not, in the least, move the beams, and scarce at all move the chips, will easily carry up the saw-dust into the air. And we see in our chimneys, that the smoke readily ascends, whilst even small clods of soot, which is but an aggregate of the particles of smoke, fall headlong down.

CHAP. II.

THE next qualification requisite in the corpuscles of volatile bodies is, that they be not too solid or heavy. For if they be so, though their bulk be very small, yet, unless other circumstances do much compensate their weight, it will be very difficult to elevate them, because of the great disproportion of their specific gravity to that of the air, (which contributes to sustain and even raise many sorts of volatile parts) and to the strength of the igneous effluvia or other agents, that would carry them up. Thus we see, that filings of lead or iron, and even minium (which is the calx of lead) though the grains they consist of be very small, will not easily be blown up like common dust, or meal, or other powders made of less ponderous materials.

A third qualification to be desired in the corpuscles, that should make up a volatile body, is, that they be conveniently shaped for motion. For if they be of branched, hooked, or other very irregular or inconvenient figures, they will be apt to be stopped and detained by other bodies, or intangled among themselves, and consequently very difficult to be carried upwards, in regard that, whilst they are thus fastened, either to one another, or to any stable body, each single corpuscle is not only to be considered, as having its own peculiar bulk, since its cohesion with the other corpuscle or body, that detains it, makes them fit to be looked upon *per modum unius*; that degree of heat, they are exposed to, being presumed uncapable of disjoining them. And this may be one reason, why water, though it be specifically heavier than oil, yet is much more easily brought to exhale in the form of vapours than is oil, whose corpuscles by the lasting stains they leave on cloth, wood, wool, &c. (which water will but transiently moisten, not stain) seem to be of very intangling figures.

THE fourth and last qualification requisite in a volatile body is, that the parts do loosely adhere, or at least be united in such a manner, as does not much indispose them to be separated by the fire in the form of fumes or vapours.

FOR he, that considers the matter, will easily grant, that, if the contexture of the corpuscles, whereof a body consists, be intricate, or their cohesion strong, their mutual implication, or their adherence to each other, will make one part hinder another from flying separately away, and their conjunction will make them too heavy or unweildy to be elevated together, as intire, though compounded parts. Thus we see, that in spring, or the beginning of summer, a wind, though not faint, is unable to carry off the lightest leaves of trees, because they stick fast to the bows and twigs on which they grow, but in Autumn, when that adhesion ceases, and the leaves sit but loosely on, a wind no stronger than that they resisted before, will with ease blow them off, and perhaps carry them up a good way into the air. But here note, that it was not without some cause, that I added above, that in a fluid body, the parts should at least be united in such a manner, as does not much indispose them to be separated. For it is not impossible, that the parts of a body may, by the figures and smoothness of the surfaces, be sufficiently apt to be put into motion, and yet be indisposed to admit such a motion as would totally separate them and make them fly up into the air. As, if you take two pieces of very flat and well-polished marble or glass, and lay them one upon the other, you easily make them slide along each others surfaces, but not easily pull up one of them, whilst the other continues its station. And when glass is in the state of fusion, the parts of it will easily slide along each other, as is usual in those of other fluids, and consequently change places, and yet the continuity of the whole is not intirely broken, but every corpuscle does somewhere touch some other corpuscle, and thereby maintain the cohesion, that indisposes it for that intire separation accompanied with a motion upwards, that we call avolation. And so, when saltpetre alone is in a crucible exposed to the fire, though a very moderate degree of it will suffice to bring the salt to a state of fusion, and consequently to put the corpuscles, that compose it, into a restless motion; yet a greater degree of heat, than is necessary to melt it, will not extricate so much as the spirits, and make them fly away.

CHAP. III.

THE foregoing doctrine of the volatility of bodies may be as well illustrated as applied, if we proceed to deduce from it the general ways of volatilization of bodies, or of intro-

introducing volatility into an assigned portion of matter. For these ways seem not inconveniently reducible to five, which I shall severally mention, though nature and art do usually employ two or more of them in conjunction. For which reason I would not, when I speak of one of these ways, be understood, as if, excluding the rest, I meant, that no other concurred with it.

THE first of the five ways or means of volatilizing a body is, to reduce it into minute parts, and, *cæteris paribus*, the more minute they are the better.

THAT the bringing a body into very minute parts may much conduce to the volatilizing of it, may be gathered from the vulgar practice of the chemists, who, when they would sublime or distil antimony, sal armoniac, sea-salt, nitre, &c. are wont to beat them to powders to facilitate their receiving a further comminution by the action of the fire. And here I observe, that in some bodies this comminution ought not to be made only at first, but to be continued afterwards. For chemists find by experience, though perhaps, without considering the reason of it, that sea-salt and nitre will very hardly afford their spirits in distillations, without they be mingled with powdered clay or bole, or some such other additament, which usually twice or thrice exceeds the weight of the salt itself: although these additaments, being themselves fixed, seem unlikely to promote the volatilization of the bodies mixed with them, yet by hindering the small grains of salt to melt together into one lump or mass, and consequently by keeping them in the state of comminution, they much conduce to the driving up of the spirits, or the finer parts of the salts by the operation of the fire.

BUT to prosecute a little what I was saying of the conduciveness of bringing a body into small parts to the volatilization of it, I shall add, that in some cases the comminution may be much promoted by employing physical, after mechanical, ways; and that, when the parts are brought to such a pitch of exiguity, they may be elevated much better than before. Thus, if you take filings of Mars, and mix them with sal armoniac, some few parts may be sublimed; but if, as I have done, you dissolve those filings in good spirit of salt, instead of oil of vitriol, and having coagulated the solution, you calcine the greenish crystals or *vitriolum martis*, that will be afforded, you may with ease, and in no long time, obtain a *crocus martis* of very fine parts; so that I remember, when we exquisitely mingled this very fixed powder with a convenient proportion of sal armoniac, and gradually pressed it with a competent fire, we were able to elevate at the first sublimation a considerable part of it; and adding a like, or som what inferior, proportion of fresh sal armoniac to the *caput mortuum*, we could raise so considerable a part of that also, and in it of the crocus, that we thought, if we had had conveniency to pursue the operation, we should, by not many repeated sublimations, have elevated the whole crocus, which (to hint that upon the by,) afforded a sublimate of so very astringent a taste, as may make the trial of it in stanching of blood, stopping of fluxes, and other cases, where potent astriction is desired, worthy of a physician's curiosity.

CHAP. IV.

THE second means to volatilize bodies is, to rub, grind, or otherwise reduce their corpuscles to be either smooth, or otherwise fitly shaped to clear themselves, or be disintangled from each other.

BY reason of the minuteness of the corpuscles, which keeps them from being separately discernible by the eye, it is not to be expected, that immediate and ocular instances should be given on this occasion; but that such a change is to be admitted in the small parts of many bodies, brought to be volatile, seems highly probable from the account formerly given of the requisites or conditions of volatility, whose introduction into a portion of matter, will scarce be explicated without the intervention of such a change. To this second instrument of volatilization, in concurrence with the first, may probably be referred the following phænomena: in the two first of which there is employed no additional volatile ingredient; and in the fourth, a fixed body is disposed to volatility by the operation of a liquor, though this be carefully abstracted from it.

1. IF urine freshly made be put to distil, the phlegm will first ascend, and the volatile salt will not rise till that be almost totally driven away, and then requires a not inconsiderable degree of fire to elevate it. But, if you putrify or digest urine, though in a well-closed glass-vessel, for seven or eight weeks, that gentle warmth will make the small parts so rub against, or otherwise act upon, one another, that the finer ones of the salt, will perhaps, be made more slender and light, and however will be made to extricate themselves so far, as to become volatile, and, ascending in a very gentle heat, leave the greatest part of the phlegm behind them.

2. So, if must, or the sweet juice of grapes, be distilled, before it have been fermented, it is observed by chemists, and we have tried the like in artificial wine made of raisins, that the phlegm, but no ardent spirit, will ascend. But when this liquor is reduced to wine by fermentation, which is accompanied with a great and intestine commotion of the justling parts, hitting and rubbing against one another, whereby some probably come to be broken, others to be variously ground and subtilized, the more subtile parts of the liquor being extricated, or some of the parts being, by these operations, brought to be subtile, they are qualified to be raised by a very gentle heat before the phlegm, and convene into that fugitive liquor, that chemists, for its activity, call spirit of wine. Nor is it only in the slighter instances afforded by animals and vegetables, that volatility may be effected by the means lately mentioned; for experience hath assured me, that it is possible, by an artificial and long digestion, wherein the parts have leisure for

frequent justlings and attritions, so to subtilize and dispose the corpuscles, even of common salt, for volatility, that we could make them ascend in a moderate fire of sand, without the help of bole, oil of vitriol, or any volatilizing additament; and, which is more considerable, the spirit would, in rising, precede the phlegm, and leave the greatest part thereof behind it.

THIS intestine commotion of parts, capable of producing volatility in the more disposed portions of a body, though it be much more easy to be found in liquors, or in moist and soft bodies, yet I have sometimes, though rarely, met with it in dry ones. And particularly I remember, that, some years ago, having, for trial-sake, taken mustard-seed, which is a body pregnant with subtile parts, and caused it to be distilled *per se* in a retort, I had, as I hoped, (without any more ado,) a great many grains of a clear and figured volatile salt at the very first distillation: which experiment having, for the greater security, made a second time with the like success, I mentioned it to some lovers of chemistry, as what, I justly supposed, they had not heard of. I leave it to farther enquiry, whether, in a body so full of spirits, as mustard-seed, the action or re-action of the parts among themselves, perhaps promoted by just degrees of fire, might not suffice to make in them a change equivalent in order to volatilization, and the yielding a volatile salt, to that, which we have observed fermentation and putrefaction to have made in the juice of grapes, urine, and some other bodies. How far the like success may be expected in other trials, I cannot tell; especially, not having by me any notes of the events of some attempts, which that enquiry put me upon: only, I remember in general, that, as some trials, I made with other seeds, and even with aromatic ones, did not afford me any volatile salt; so the success of other trials made me now and then think, that some subjects of the vegetable kingdom, whence we are wont to drive over acid spirits, but no dry salt, may be distilled with so luckily regulated a heat, as to afford something, though but little, of volatile salt; and, that perhaps more bodies would be found to do so, were they not too hastily or violently pressed by the fire, whereby such saline schematisms of the desired parts of the matter are (by being dissipated or confounded) destroyed or vitiated, as in a slow, dextrous, or fortunate way of management would come forth, not in a liquid, but a saline form. Of which observation, we may elsewhere mention some instances, and shall, before the close of this paper, name one, afforded us by crude tartar.

3. THOUGH silver be one of the fixedest bodies, that we know of, yet, that it is not impossible, but that, chiefly by a change of texture, it may strangely be disposed to volatility, I was induced to think, by what I remember once happened to me. A gentleman of my acquaintance, studious of chemical arcana, having lighted on a strange menstruum, which he affirmed, and I had some cause to believe, not to be corrosive, he abstracted it from several metals, (for the same liquor would serve again and again,) and brought me the remainders, with a desire, that I would endeavour to reduce those of lead and silver into the pristine metals again, which he had, in vain, attempted to do: whereupon, though I found the white calx of lead reducible, yet, when I came to the calx of silver, I was not able to bring it into a body; and having, at length, melted some lead in a gentle fire, to try whether I could make it swallow up the calx, in order to a farther operation, I was not a little surprized to find, that this mild heat made the calx of silver presently fly away, and sublime in the form of a *farina volatilis*, which whitened the neighbouring part of the chimney, as well as the upper part of the crucible.

4. From that, which chemists themselves tell us, I think we may draw a good argument *ad hominem*, to prove, that volatility depends much upon the texture, and other mechanical affections of a body. For divers of those Hermetick philosophers (as they are called) that write of the elixir, tell us, that when their philosophick mercury or grand solvent, being sealed up together with a third or fourth part of gold in a glass egg, is kept in convenient degrees of fire, the whole matter, and consequently the gold, will, by the mutual operation of the included substances, be so changed, that not only it will circulate up and down in the glass, but, in case the digestion or decoction should be broken off at a certain inconvenient time, the gold would be quite spoiled, being, by the past and untimely ended operation, made too volatile to be reducible again into gold: whereas, if the decoction be duly continued unto the end, not only the gold, but all the philosophical mercury or menstruum will be turned into a sulphur, or powder of a wonderfully fixed nature. I know, there are several Chrysopæans, that speak much otherwise of this operation, and tell us, that the gold employed about it must be philosophic gold: but I know too, that there are divers others, (and those too none of the least candid or rational,) that speak of it, as I have done; and that is sufficient to ground an argument on towards all those, that embrace their doctrine. And, in this case, it is considerable, that it is not by any superadded additament, that the most fixed body of gold is made volatile, but the same massy matter, consisting of gold and philosophic mercury, is, by the change of texture produced, or occasioned by the various degrees and operations of fire upon it, brought to be first volatile, and then extreamly fixed. And having said this, in reference to one tribe of the modern Spagyrists, to another of them, the *Helmontians*, I think, I can offer a good argument *ad hominem* from the testimony and experiments of the founder of their sect.

5. THE acute *Helmont*, among other prodigious powers, that he ascribes to the alkahest, affirms, that, by abstracting it frequently enough, it would so change all tangible bodies, and consequently stones and metals, that they might be distilled over into liquors equiponderant to the respective bodies, that afforded them,

them, and having all the qualities of rain water; which if they have, I need not tell you, that they muſt be very volatile. And I ſee not how thoſe, that admit the truth of this ſtrange alkaheſtical operation, can well deny, that volatility depends upon the mechanical affections of matter, ſince it appears not, that the alkaheſt does, at leaſt in our caſe, work upon bodies otherwiſe than mechanically. And it muſt be confeſſed, that the ſame material parts of a portion of a corporeal ſubſtance, which, when they were aſſociated and contexed (whether by an archeus, ſeed, form, or what elſe you pleaſe,) after ſuch a determinate manner, conſtituted a ſolid and fixed body, as a flint or a lump of gold; by having their texture diſſolved, and (perhaps after being ſubtilized) by being freed from their former implications, or firm coheſions, may become the parts of a fluid body totally volatile.

CHAP. V.

THE fourth means of making a body volatile is, by aſſociating the particles to be raiſed with ſuch as are more volatile than themſelves, and of a figure fit to be faſtened to them, or are at leaſt apt, by being added to them, to make up, with them, corpuſcles more diſpoſed than they to volatility. This being the grand inſtrument of volatilization, I ſhall ſpend ſomewhat the more time about it. But I ſhall firſt here a little explain the laſt clauſe, (that I may not be obliged to reſume it elſewhere) by intimating, that it is not impoſſible, that the particles of an additament, though not more volatile than thoſe of the body it is mixed with, and perhaps, though not volatile at all, will yet conduce to volatilize the body wherewith it is mingled. For the particles of the additament may be of ſuch figures, and ſo aſſociated with thoſe of the body to be elevated, as in this to enlarge the former pores, or produce new ones, by intercepting little cavities (for they muſt not be great ones) between the particles of a body to be raiſed, and thoſe of the additament. For, by theſe and other ſuch ways of aſſociation, the corpuſcles, reſulting from the combination or coalition of two or more of theſe differing particles, may, without becoming too big and unweildy, become more conveniently ſhaped, or more light in proportion to their bulk, and ſo more eaſily buoyed up and ſuſtained in the air, (as when the lid of a copper box being put on, makes the whole box emerge and ſwim in water, becauſe of the intercepted cavity, though neither of the parts of the box would do ſo,) or otherwiſe more fitted for avolation than the particles themſelves were, before their being joined to thoſe of the additament.

BY two things, chiefly, the corpuſcles of the additament may contribute to the elevation of a body. For, firſt, the parts of the former may be much more diſpoſed for avolation than is neceſſary to their own volatility. As when in the making of ſal armoniac, the ſaline particles of urine and of ſoot, are more fugitive than they need be, to be themſelves ſublimed, and thereby are advantaged to carry up with them the more ſluggiſh corpuſcles, whereof ſea ſalt conſiſts. And next, they may be of figures ſo proper to faſten them well to the body to be elevated, that the more fugitive will not be driven away, or disjoined from the more fixed by ſuch a degree of heat as is ſufficient to raiſe them both together: to which effect the congruity, or figuration, is as well required, as the lightneſs or volatility of the particles of the additament. And therefore ſome of the fugitiveſt bodies, that we know, as ſpirit of wine, camphire, &c. will not volatilize many bodies, which will be elevated by far leſs fugitive additaments; becauſe the corpuſcles of ſpirit of wine ſtick not to thoſe of the body they are mingled with, but, eaſily flying up themſelves, leave thoſe behind them, which they did rather barely touch, than firmly adhere to: whereas far leſs fugacious liquors, if they be endowed with figures, that fit them for a competently firm coheſion with the body they are mingled with, will be able to volatilize it. Of which I ſhall now give you ſome inſtances in bodies, that are very ponderous, or very fixed, or both.

AND I ſhall begin with colcothar, though it being a vitriolate calx, made by a laſting and vehement fire, it is (conſequently) capable of reſiſting ſuch a one. This being exquiſitely ground with an equal weight of ſal armoniac, which is itſelf a ſalt, but moderately volatile, will be in good part ſublimed into thoſe yellow flowers, which we have elſewhere more particularly taught to prepare, under the name of *ens primum veneris*; in which, that many vitriolate corpuſcles of the colcothar are really elevated, you may eaſily find, by putting a grain or two of that reddiſh ſubſtance into a ſtrong infuſion of galls, which will thereby immediately acquire an inky colour.

STEEL alſo, which, to deſerve that name, muſt have endured extraordinary violences of the fire, and greater than is needful to obtain other metals from their mother earth; ſteel itſelf, I ſay, being reduced to filings, and diligently ground with about an equal weight of ſal armoniac, will, if degrees of fire be ſkilfully adminiſtred, (for it is eaſy to err in that point,) without any previous calcination or reduction to a crocus, ſuffer ſo much of the metal to be carried up, as will give the ſal armoniac a notable colour, and an ironiſh taſte.

AND here it will be proper to obſerve, for the ſake of practical chemiſts, that the quantity or proportion of the volatile additament is to be regarded; though not ſo much as its nature, yet more than it is wont to be: and divers bodies, that are thought either altogether unfit for ſublimation, or, at leaſt, uncapable to have any conſiderable portion of them elevated, may be copiouſly enough ſublimed, if a greater proportion of the additament, than we uſually content our ſelves with, be ſkilfully employed. And in the newly-mentioned inſtance of filings of ſteel, if, inſtead of an equal weight of ſal armoniac, the treble weight be taken, and the operation be duly managed, a far greater quantity of the metal may be raiſed, eſpecially if freſh ſal armoniac be carefully ground with the caput mortuum. And ſal armoniac may, perhaps,

haps, be compounded with such other bodies, heavier than itself, as may qualify it, when it is thus clogged, to elevate some congruous bodies better than it would of itself alone. And I shall venture to add this farther advertisement, that if, besides the plenty of the additament, there be a sufficient fitness of its particles to lay hold on those of the body to be wrought on, mineral bodies, and those ponderous enough, may be employed to volatilize other heavy bodies. And I am apt to think, that almost, if not more than almost, all metals themselves may by copious additaments and frequent cohobations be brought to pass through the neck of the retort in distillation; and perhaps, if you melt them not with equal parts, but with many parts of regulus of antimony, and then proceed as the hints now given will direct you, you will not find cause to despise what I have been saying.

YOU know what endeavours have been, and are still fruitlessly employed by chemists to elevate so fixed a body, as salt of tartar, by additaments. I shall not now speak much of the enterprize in general, designing chiefly to tell you on this occasion, that, whereas frequent experience shews, that sal armoniac being abstracted from salt of tartar, not only the salt of tartar is left at the bottom, but a good part of the sal armoniac is left behind with it: I suspected the cause might be, that sal armoniac, by the operation of the alkali of tartar, is reduced into sea-salt, and urinous or fuliginous salt, as it was at first composed of those differing ingredients; and that by this means the volatile salt being loosened or disintangled from the rest, and being of a very fugacious nature, flies easily away itself, without staying long enough to take up any other salt with it. And therefore, if this analysis of the sal armoniac could be prevented, it seemed not impossible to me, that some part of the salt of tartar, as well as of colcothar and steel, might be carried up by it: and accordingly having caused the ingredients to be exceedingly well dried, and both nimbly and carefully mixed, and speedily exposed to the fire, I have sometimes had a portion of salt of tartar carried up with the sal armoniac: but this happened so very rarely, that I suspected some peculiar fitness for this work in some parcels of sal armoniac, that are scarce but by the effect to be discerned from others. But however, what has happened to us may argue the possibility of the thing, and may serve to shew the volatilizing efficacy of sal armoniac; which is a compound, that I elsewhere recommend, and do it now again, as one of the usefullest productions of vulgar chemistry.

AND since I have mentioned the volatilization of salt of tartar, presuming your curiosity will make you desire my opinion about the possibility of it, I shall propose to you a distinction, that perhaps you do not expect, by saying, that I think there is a great deal of difference between the making a volatile salt of tartar, and the making salt of tartar volatile. For though this seem to be but a nicety, yet really it is none; and it is very possible, that a man may from tartar obtain a volatile salt, and yet be no wise able to volatilize that tartareous salt, that has been once by the incineration of the tartar brought to fixed alkali. I have in the Sceptical Chemist summarily delivered a way, by which both I, and some Spagyrists, that learned it of me, obtained from a mixture of antimony, nitre and crude tartar, a volatile salt, which in probability comes from the last named of those three bodies; but experience carefully made has assured me, that without any additament, by a distillation warily and very slowly made, (insomuch that I have spent near a week in distilling one pound of matter) very clean tartar, or at least the crystals of tartar, may, in conveniently shaped vessels, be brought to afford a substance, that in rectification will ascend to the upper part of the vessel, in the form of a volatile salt, as if it were of urine or of hart's-horn; of which tartareous salt, I keep some by me: but this operation requires not only a dexterous, but a patient distiller.

BUT now as to the making a fixed alkali of tartar become volatile, I take it to be another, and have found it to be a far more difficult, work; the common processes of performing it being wont to promise much more than they can make good; which I may justly say of some other, that private men have vaunted for great arcana, but upon trial have satisfied me so little, that I have divers times offered pretenders to make salt of tartar volatile, that without at all inquiring into their processes, I would lay good wagers, that they could do what they pretended; not only as divers philosophical Spagyrists require, without any visible additament, but by any additament whatever; provided I were allowed to bring the salt of tartar myself, and to examine the success, not by what may appear in the alembic and receiver, but by the weight of what would remain in the bottom. For I have convinced some of the more ingenuous artists, that the salt, that sublimed, was not indeed the alkali of tartar, but somewhat, that was by the operation produced, or rather extricated out of the additaments. But yet I would not be thought to affirm, that it is not possible to elevate the fixed salt of tartar. For sometimes I have been able to do it, even at the first distillation by an artificial additament perhaps more fixed than itself; but, though the operation was very grateful to me, as it shewed the possibility of the thing, yet the paucity of the salt sublimed and other circumstances, kept me from much valuing it upon any other account. And there are other ways, whereby experience has assured me, that salt of tartar may be raised. And if one of them were not so uncertain, that I can never promise before hand, that it will at all succeed, and the other so laborious, difficult and costly, that few would attempt or be able to practice it, I should think them very valuable things; since by the former way most part of the salt of tartar was quickly brought over in the form of a liquor, whose

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piercing ſmell was ſcarce tolerable; and by the latter way ſome ſalt of tartar of my own, being put into a retort, and urged but with ſuch a fire as could be given in a portable ſand-furnace, there remained not at the bottom near one half of the firſt weight, the additament having carried up the reſt, partly in the form of a liquor, but chiefly in that of a white ſublimate, which was neither ill-ſcented, nor in taſte corroſive, or alkalizate, but very mild, and ſomewhat ſweetiſh. And I do not much doubt, but that by other ways the fixed alkali of tartar may be elevated, eſpecially if, before it be expoſed to the laſt operation of the fire, it be dexterouſly freed from the moſt of thoſe earthy and viſcuous parts, that I think may be juſtly ſuſpected to clog and bind the truly ſaline ones.

BUT I have too long digreſſed, and therefore ſhall intimate only upon the by, that even the ſpurious ſal tartari volatilized, that is made with ſpirit of vinegar, may, if it be well prepared, make amends for its empyreumatical ſmell and taſte, and may, notwithſtanding them, in divers caſes be of no deſpicable uſe, both as a medicine, and a menſtruum.

CHAP. VI.

BEFORE I draw towards a concluſion of theſe notes about volatility, perhaps it will not be amiſs, to take notice of a phænomenon, which may much ſurprize, and ſometimes diſappoint thoſe, that deal in ſublimations, unleſs they be forewarned of it. For though it be taken for granted, and for the moſt part may juſtly be ſo, that by carefully mingling what is ſublimed with what remains, and reſubliming the mixture, a greater quantity of the body to be ſublimed may be elevated the ſecond time than was the firſt, and the third time than the ſecond, and ſo onwards; yet I have not found this rule always to hold, but in ſome bodies, as particularly in ſome kinds of dulcified colcothar, the ſal armoniac, would at the firſt ſublimation carry up more of the fixed powder, than at the ſecond or third. So that I was by ſeveral trials perſuaded, when I found a very well and highly coloured powder elevated, to lay it by for uſe, and thereby ſave my ſelf the labour of a proſecution, that would not only have proved uſeleſs, but prejudicial. And if I miſremember not, by often repeated cohobations, if I may ſo call them, of ſal armoniac upon crude or mineral antimony, though the ſublimate, that was obtained by the firſt operation, was much of it variouſly, and in ſome places richly coloured; yet afterwards, the ſalt aſcended from time to time paler and paler, leaving the antimony behind it. Which way of making ſome minerals more fixed and fuſible I conceive may be of great uſe in ſome medicinal preparations, though I think it not fit to particularize them in this place: where my chief intent was, to mention the phænomenon itſelf, and invite you to conſider, whether it may be aſcribed to this, that by the reiterated action of the fire, and grinding together of the body to be raiſed, either the corpuſcles of the ſal armoniac, or thoſe of the other body, may have thoſe little hooked or equivalent particles, whereby they take hold of one another, broken or worn off; and whether the indiſpoſedneſs of the colcotharine or antimonial parts to aſcend, may not in ſome caſes be promoted by their having, by frequent attritions, ſo ſmoothed their ſurfaces, that divers of them may cloſely adhere, like pieces of poliſhed glaſs, and ſo make up cluſters too unweildy to be ſo raiſed, as the ſingle corpuſcles they conſiſt of, were. Which change may diſpoſe them to be at once leſs volatile and more fuſible. Which conjectures I mention to excite you to frame better, or at leaſt to make amends for my omiſſion of examining theſe, by trying whether the ſal armoniac, grown white again, will be as fit as it was at firſt to carry up freſh bodies; and alſo by obſerving the weight of the unelevated part, and employing thoſe other ways of examen, which I ſhould have done, if I had not then made ſublimations for another end, than to clear up the doctrine of volatility.

AND here it may be profitable to ſome chemiſts, though not neceſſary to my ſubject, to intimate, that ſublimations may be uſeful to make very fine comminutions of divers bodies. That thoſe, that are elevated are reduced to a great fineneſs of parts, is obvious to be obſerved in many examples, whence it has been anciently, not abſurdly, ſaid, that ſublimations are the chemiſts peſtles, ſince (as in flowers of ſulphur and antimony) they do really reſolve the elevated bodies into exceeding fine flower, and much finer than peſtles and mortars are wont to bring them to. But that, which I intend in this paragraph, is not a thing ſo obvious, ſince it is to obſerve, that ſometimes even bodies ſo fixed, as not at all to aſcend in ſublimation, may yet be reduced by that operation into powders extremely fine. For exemplifying of which, I ſhall put you in mind, that though Spagyriſts complain much of the difficulty of making a good calx of gold, and of the imperfection of the few ordinary proceſſes preſcribed to make it, (which would be more complained of, but that chemical phyſicians ſeldom attempt to prepare it,) yet we are informed by trial, that by exactly grinding a thick amalgam of gold and mercury with a competent weight, (at leaſt equal to its own) of finely powdered ſulphur, we may, by putting the mixture to ſublime in a conveniently ſhaped glaſs, by degrees of fire obtain a cinaber, that will leave behind it a finer calx of gold, than will be had by ſome far more difficult proceſſes.

BUT it is now time to draw towards a concluſion of our notes about volatility; which quality depends ſo much upon the contexture of the corpuſcles, that are to be raiſed together, that even very ponderous bodies may ſerve for volatilizing additaments, if they be diſpoſed to faſten themſelves ſufficiently to the bodies they are to carry up along with them. For, though lead be, ſave one, the heavieſt ſolid we know of, and though quick-ſilver be the heavieſt body in the world, except gold;

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yet trials have assured us, that quick-silver itself being united by amalgamation with a small proportion of lead, will, by a fire, that is none of the violentest, and in close vessels, be made to carry over with it some of the lead. As we clearly found by the increased weight of the quick-silver, that passed into the receiver; which, by the way, may make us cautious, how we conclude quick-silver to be pure, merely from its having been distilled over.

THERE remains but one body more heavy than those I come from naming, and that is gold; which, being also of a fixity so great, that it is indeed admirable, I do not wonder, that, not only the more wary naturalists, but the more severe among the chemists themselves, should think it incapable of being volatilized. But yet, if we consider, how very minute parts gold may be rationally supposed to consist of, and to be divisible into, methinks it should not seem impossible, that, if men could light on volatile salts endowed with figures fit to stick fast to the corpuscles of the gold, they would carry up with them bodies, whose solidity can scarce be more extraordinary, than their minuteness is: and, in effect, we have made more than one menstruum, with which some particles of gold may be carried up. But when I employed that, which I recommended to you formerly, under the name of *menstruum peracutum*, (which consists mainly, and sometimes only of spirit of nitre, several times drawn from butter of antimony,) I was able, without a very violent fire, in a few hours, to elevate so much crude gold, as, in the neck of the retort, afforded me a considerable quantity of sublimate, which I have had red as blood, and whose consisting partly of gold, manifestly appeared by this, that I was able, with ease, to reduce that metal out of it.

IN reckoning up the instruments of volatilization, we must not quite leave out the mention of the air, which I have often observed to facilitate the elevation of some bodies, even in close vessels; wherein, though to fill them too full be judged, by many, a compendious practice, because the steams have a less way to ascend, yet experience has several times informed me, that, at least in some cases, they take wrong measures, and that (to pass by another cause of their disappointment) a large proportion of air, purposely left in the vessels, may more than compensate the greater space, that is to be ascended by the vapours or exhalations of the matter, that is to be distilled or sublimed. And if, in close vessels, the presence of the air may promote the ascension of bodies, it may well be expected, that the elevation of divers of them may be furthered, by being attempted in open vessels, to which the air has free access. And if we may give any credit to the probable relations of some chemists, the air does much contribute to the volatilization of some bodies, that are barely, though indeed for no short time, exposed to it. But the account, on which the air, by its bare presence, or peculiar operations, conduces to the volatilization of some bodies, is a thing very difficult to be determined, without having recourse to some notions about gravity and levity, and of the constitution of the corpuscles, that compose the air; which I take to be both very numerous, and no less various. And therefore I must not, in these occasional notes, launch out into such a subject, though, for fear I should be blamed for too much slighting my old acquaintance the air, I durst not quite omit the power it has to dispose some bodies to volatility.

A moderate attention may suffice, to make it be discerned, that, in what hath been hitherto delivered, I have, for the most part, considered the small portions of matter, to be elevated in volatilization, as entire corpuscles: and therefore it may be now pertinent, to intimate in a line or two, that there may be also cases, wherein a kind of volatilization, improperly so called, may be affected, by making use of such additaments, as break off, or otherwise divide the particles of the corpuscles to be elevated, and by adhering to, and so clogging one of the particles, to which it proves more congruous, enable the other, which is now brought to be more light, or disengaged, to ascend. This may be illustrated by what happens, when sal armoniac is well ground with lapis calaminaris, or with some fixed alkali, and then committed to distillation: for the sea-salt, that enters the composition of the sal armoniac, being detained by the stone or the alkali, there is a divorce made between the common salt and the urinous and fuliginous salts, that were incorporated with it, being now disengaged from it, are easily elevated. I elsewhere mention, that I have observed, in man's urine, a kind of native sal armoniac, much less volatile than the fugitive, that is sublimed from man's blood, hartshorn, &c. and therefore supposing, that a separation of parts may be made by an alkali, as well in this salt, as in the common factitious sal armoniac, I put to fresh urine a convenient proportion (which was a plentiful one) of salt of pot-ashes, (that being then at hand,) and distilling the liquor, it yielded, according to expectation, a spirit more volatile than the phlegm, and of a very piercing taste; which way of obtaining a spirit without any violence of fire, and without either previously abstracting the phlegm, (as we are fain to do in fresh urine) or tediously waiting for the fermentation of stale urine, I taught some chemists, because of the usefulness of spirit of urine; which being obtained this innocent way, would probably be employed with much less suspicion of corrosiveness, than if in the operation I had made use of quick-lime. Another illustration of what I was not long since saying, may be fetched from the experiment of making spirit of nitre, by mixing salt-petre with oil of vitriol, and distilling them together: for the oil does so divide or break the corpuscles of the nitre, that the now disposed particles of that salt, which amount to a great portion of the whole, will be made easily enough to ascend, even with a moderate fire of sand, and sometimes without any fire at all, in the form of spirits, exceeding unquiet, subtle, and apt to smoke away.

To which instances of this imperfect kind of volatilization more might be added, but that you may well think, I have detained you but too long already with indigested notes about one quality.

CHAP. VII.

THE last means of volatilizing bodies is, the operation of the fire or some other actual heat: but of this, which is obvious, it would be superfluous to discourse. Only this I shall intimate, that there may be bodies, which, in such degrees of fire, as are wont to be given in the vulgar operations of chemists, will not be elevated, which yet may be forced up by such violent and lasting fires, as are employed by the melters of ores, and founders of guns, and sometimes by glass-makers. And, on this consideration, I shall here observe to you, since I did not do it at my entrance on these notes, that chemists are wont to speak, and I have accordingly been led to treat of volatility and fixity, in a popular sense of those terms. For, if we would consider the matter more strictly, I presume we should find, that volatility and fixity are but relative qualities, which are to be estimated, especially the former of them, by the degree of fire, to which the body, whereto we ascribe one or other of those qualities, is exposed; and therefore it is much more difficult, than men are aware of, to determine accurately, when a body ought to be accounted volatile, and when not; since there is no determinate degree of heat agreed on, nor indeed easy to be devised, that may be as a standard, whereby to measure volatility and fixedness: and it is obvious, that a body, that remains fixed in one degree of fire, may be forced up by another. To which may be added, agreeably to what I lately began to observe, that a body may pass for absolutely fixed among the generality of chemists, and yet be unable to persevere in the fires of founders and glass-makers: which brings into my mind, that not having observed, that chemists have examined the fixity of other bodies, than metalline ones, by the cupel, I had the curiosity to put dry salt of tartar upon it, and found, as I expected, that, in no long time, it manifestly wasted in so vehement a heat, wherein also the air came freely at it, (though quick-lime, handled after the same way, lost not of its weight; and having well mixed one ounce of good salt of tartar with treble its weight of tobacco-pipe clay, we kept them but for two, or, at most, three hours, in a strong fire; yet, the crucible being purposely left uncovered, we found the salt of tartar so wasted, that the remaining mixture (which was not fluxed) afforded us not near a quarter of an ounce of salt. And indeed I scarce doubt, but that in strictness divers of those bodies, that pass for absolutely fixed, are but semi-fixed, or, at least, but comparatively and relatively fixed, that is, in reference to such degrees of fire, as they are wont to be exposed to in distillations, sublimations, &c. of chemists; not such as are given in the raging fires of founders and glass-makers. And perhaps, even the fires of glass-makers, and say-masters themselves, are not the most intense, that may possibly be made in a short time, provided there be but small portions of matter to be wrought on by them. And, in effect, I know very few bodies, besides gold, that will persevere totally fixed in the vehementest degrees of fire, that trials have made me acquainted with. And I elsewhere tell you, that, though tin, in our chemical reverberatories themselves, is wont to be reduced but into a calx, that is reputed very fixed; yet in those intense fires, that a virtuoso of my acquaintance uses in his tin-mines, there is not seldom found quantities of tin carried up to a notable height in the form of a whitish powder, which, being in good masses, forced off from the places to which it had fastened itself, does, by a skilful reduction, yield many a pound weight of good malleable metal, which seemed to me to be rather more, than less, fine than ordinary tin.

POSTSCRIPT,

Relating to page 612, and here annexed for their sakes, who have a mind to repeat the experiment there delivered, that so they may know the quantities employed in it.

WITH two parts of this crocus, we ground very well three parts of sal armoniac; and having sublimed them in a strong fire, we took off the high coloured sublimate, and put in either an equal weight, or a weight exceeding it by half, to the caput mortuum, we found, after the second sublimation, which was also high coloured, that, of an ounce of crocus, we had raised six drams, that is, three quarters of the whole weight.

EXPERIMENTAL NOTES

OF THE

Mechanical ORIGIN or PRODUCTION

OF

FIXEDNESS.

CHAP. I.

FIXITY being the oppofite quality to volatility, what we have difcourfed about the latter, will make the nature of the former more eafily underftood, and upon that account allow me to make fomewhat the quicker difpatch of what I have to fay of it.

THE qualifications, that conduce moft to the fixity of a portion of matter, feem to be thefe.

FIRST, the groffnefs, or the bulk of the corpufcles it confifts of. For, if thefe be too big, they will be too unweildy, and unapt to be carried up into the air by the action of fuch minute particles as thofe of the fire, and will alfo be unfit to be buoyed up by the weight of the air; as we fee, that vapours, whilft they are fuch, are fmall enough to fwim in the air, but can no longer be fuftained by it, when they convene into drops of rain, or flakes of fnow. But here it is to be obferved, that, when I fpeak of the corpufcles, that a fixed body confifts of, I mean not either its elementary or its hypoftatical principles, as fuch, but only thofe very little maffes or clufters of particles, of what kind foever they be, that ftick fo firmly to one another, as not to be divifible and diffipable by that degree of fire, in which the body is faid to be fixed; fo that each of thofe little concretions, though it may itfelf be made up of two, three, or more particles of a fimpler nature, is confidered here *per modum unius*, or as one entire corpufcle. And this is one qualification conducive to the fixednefs of a body.

THE next is the ponderoufnefs, or folidity of the corpufcles it is made up of. For if thefe be very folid, and (which folid and compact bodies ufually are) of a confiderable fpecifick gravity, they will be too heavy to be carried up by the effluvia or the action of the fire, and their ponderoufnefs will make them as unweildy, and indifpofed to be elevated by fuch agents, as the groffnefs of their bulk would make bigger corpufcles, but of a proportionably inferior fpecific weight. On which account, the calces of fome metals and minerals, as gold, filver, &c. though, by the operation of folvents, or of the fire, or of both, reduced to powders exceedingly fubtile, will refift fuch vehement fires, as will eafily drive up bigger, but lefs heavy and compact corpufcles, than thofe calces confift of.

THE third qualification, that conduces to the fixity of a body, belongs to its integral parts, not barely as they are feveral parts of it, but as they are aggregated or contexed into one body. For, the qualification, I mean, is the ineptitude of the component corpufcles for avolation, by reafon of their branchednefs, irregular figures, crookednefs, or other inconvenient fhape, which entangles the particles among one another, and makes them difficult to be extracted; by which means, if one of them do afcend, others, wherewith it is complicated, muft afcend with it; and, whatever be the account, on which divers particles ftick firmly together, the aggregate will be too heavy or unweildy to be raifed. Which I therefore take notice of, becaufe that, though ufually it is on the roughnefs and irregularity of corpufcles, that their cohefion depends; yet it fometimes happens, that the fmoothnefs and flatnefs of their furfaces makes them fo ftick together, as to refift a total divulfion; as may be illuftrated by what I have faid of the cohefion of polifhed marbles, and the plates of glafs, and by the fixity of glafs itfelf in the fire.

FROM this account of the caufes or requifites of fixity, may be deduced the following means of giving or adding fixation to a body, that was before either volatile, or lefs fixed. Thefe means may be reduced to two general heads; firft, the action of the fire, as the parts of the body, expofed to it are thereby made to operate varioufly on one another. And next, the affociation of the particles of a volatile body with thofe of fome proper additament: which term [of proper] I rather employ than that, one would expect [of fixed;] becaufe it will

will ere long appear, that, in certain cases, some volatile bodies may more conduce to the fixation of other volatile bodies, than some fixed ones do. But these two instruments of fixation being but general, I shall propose four or five more particular ones.

CHAP. II.

AND first, in some cases it may conduce to fixation, that, either by an additament, or by the operation of the fire, the parts of a body be brought to touch each other in large portions of their surfaces. For, that from such a contact, there will follow such a mutual cohesion, as will, at least, indispose the touching corpuscles to suffer a total divulsion, may appear probable from what we lately noted of the cohesion of pieces of marble and glass, and from some other phænomena belonging to the history of firmness, from which we may properly enough borrow some instances, at least, for illustration, in the doctrine of fixedness, in regard, that usually, though not always, the same things, that make a body firm, give it some degree of fixity, by keeping it from being dissipated by the wonted degrees of heat, and agitation it meets with in the air. But, to return to the contact we were speaking of, I think it not impossible, (though you may perhaps think it strange) that the bare operation of the fire may, in some cases, procure a cohesion among the particles, (and consequently make them more fixed) as well as in others disjoin them, and thereby make them more volatile. For, as in some bodies, the figures and sizes of the corpuscles may be such, that the action of the fire may rub or tear off the little beards or hooks, or other particles, that entangle them, and by that means make it more easy for the corpuscles to be disengaged and fly upwards; so, in other bodies, the size and shape of the corpuscles may be such, that the agitation, caused by the fire, may rub them one against the other, so as by mutual attrition to grind, as it were, their surfaces, and make them so broad and smooth, if not also so flat, as that the contact of the corpuscles shall come to be made according to a large portion of their superficies, from whence will naturally follow a firm cohesion. Which I shall illustrate by what we may observe among those, that grind glasses for telescopes and miscroscopes. For, these artificers, by long rubbing a piece of glass against a metalline dish, or concave vessel, do, by this attrition, at length bring the two bodies to touch one another in so many parts of their congruous surfaces, that they will stick firmly to one another, so as sometimes to oblige the workman to use violence to disjoin them. And this instance (which is not the sole I could alledge) may suffice to shew, how a cohesion of corpuscles may be produced by the mutual adaptation of their congruous surfaces. And if two grosser corpuscles, or a greater number of smaller, be thus brought to stick together, you will easily believe, their aggregate will prove too heavy or unwieldy for avolation. And to shew, that the fire may affect a lævigation in the surfaces of some corpuscles, I have sometimes caused minium, and some other calces, that I judged convenient, to be melted for a competent time, in a vehement fire conveniently administred; whereby, according to expectation, that, which was before a dull and incoherent powder, was reduced into much grosser corpuscles, multitudes of whose grains appeared smooth, glittering, and almost specular, like those of fine litharge of gold; and the masses, that these grains composed, were usually solid enough, and of difficult fusion. And when we make glass of lead *per se*, (which I elsewhere teach you how to do) it is plain, that the particles of the lead are reduced to a great smoothness; since, wheresoever you break the glass, the surfaces, produced at the crack, will not be jagged, but smooth, and considerably specular. Nor do I think it impossible, that even, when the fire does not make any great attrition of the corpuscles of the body to be fixed, it may yet occasion their sticking together, because by long tumbling them up and down in various manners, it may at length, after multitudes of revolutions, and differing occursions, bring those of their surfaces together, which, by reason of their breadth, smoothness, or congruity of figure, are fit for mutual cohesion; and when once they come to stick, there is no necessity, that the same causes, that were able to make them pass by one another, when their contact was but according to an inconsiderable part of their surfaces, should have the same effect now, when their contact is full; though perhaps, if the degree of fire were much encreased, a more vehement agitation would surmount this cohesion, and dissipate again these clusters of coalescent corpuscles.

THESE conjectures will, perhaps, appear less extravagant, if you consider what happens in the preparation of quicksilver precipitated *per se*. For there running mercury, being put into a conveniently shaped glass, is exposed to a moderate fire for a considerable time: (for I have sometimes found six or seven weeks to be too short a one.) In this degree of fire the parts are variously tumbled, and made many of them to ascend, till convening into drops on the sides of the glass, their weight carries them down again; but, at length, after many mutual occursions, if not also attritions, some of the parts begin to stick together in the form of a red powder, and then more and more mercurial particles are fastened to it, till at length all, or by much the greater part of the mercury, is reduced into the like præcipitate, which, by this cohesion of the parts, being grown more fixed, will not, with the same degree of heat, be made to rise and circulate, as the mercury would before; and yet, as I elsewhere note, I have found, by trial, that, with a greater and competent degree of heat, this præcipitate *per se*, would, without the help of any volatilizing additament, be easily reduced into running mercury again. Chemists and physicians, who agree in supposing this præcipitate to be made without any additament, will, perchance, scarce be able to give a more likely

likely account of the consistency and degree of fixity, that is obtained in the mercury; in which, since no body is added to it, there appears not to be wrought any but a mechanical change. And though, I confess, I have not been without suspicions, that in philosophical strictness this præcipitate may not be made *per se*, but that some penetrating igneous particles, especially saline, may have associated themselves with the mercurial corpuscles; yet even upon this supposition it may be said, that these particles contribute to the effect, that is produced, but by facilitating or procuring, by their opportune interposition, the mutual cohesion of corpuscles, that would not otherwise stick to one another.

PERHAPS it will not be altogether impertinent to add on this occasion, that, as for the generality of chemists, as well others, as Helmontians, that assert the transmutation of all metals into gold by the philosopher's stone, methinks, they may grant it to be probable, that a new and fit contexture of the parts of a volatile body may, especially by procuring a full contact among them, very much contribute to make it highly fixed. For to omit what is related by less credible authors, it is averred, upon his own trial, by *Helmont*, who pretended not to the elixir, that a grain of the powder, that was given him, transmuted a pound (if I misremember not) of running mercury; where the proportion of the elixir to the mercury was so inconsiderable, that it cannot reasonably be supposed, that every corpuscle of the quick-silver, that before was volatile, was made extremely fixed, merely by its coalition with a particle of the powder, since, to make one grain suffice for this coalition, the parts it must be divided into must be scarce conceiveably minute, and therefore each single part not likely to be fixed itself, or at least more likely to be carried up by the vehemently agitated mercury, than to restrain that from avolation; whereas, if we suppose the elixir to have made such a commotion among the corpuscles of the mercury, as (having made them perhaps somewhat change their figure, and expelled some inconvenient particles,) to bring them to stick to one another, according to very great portions of their surfaces, and entangle one another, it will not be disagreeable to the mechanical doctrine of fixity, that the mercury should endure the fire as well as gold, on the score of its new texture, which, supposing the story true, appears to have been introduced, by the new colour, specifick gravity, indissolubleness in aqua fortis, and other qualities, wherein gold differs from mercury, especially malleableness, which, according to our notes about that quality usually requires, that the parts, from whose union it results, be either hooked, branched, or otherwise adapted and fitted to make them take fast hold of one another, or stick close to one another. And since, in the whole mass of the factitious gold, all, save one grain, must be materially the same body, which, before the projection was made, was quick-silver, we may see, how great a portion of volatile matter may, by an inconsiderable quantity of fixing additament, acquire such a new disposition of its parts, as to become most fixed. And however, this instance will agree much better with the mechanical doctrine about fixity, than with that vulgar opinion of the chemists, (wherewith it will not at all comply,) that if, in a mixture, the volatile part do much exceed the fixed, it will carry up that, or at least, a good portion thereof, with it; and on the contrary. But though this rule holds in many cases, where there is no peculiar indisposition to the effect, that is aimed at; yet if the mechanical affections of the bodies be ill suited to such a purpose, our philosophical experiment manifestly proves, that the rule will not hold, since so great a multitude of grains of mercury, instead of carrying up with them one grain of the elixir, are detained by it in the strongest fire. And thus much for the first way of fixing volatile bodies.

CHAP. III.

THE second way of producing fixity is by expelling, breaking, or otherwise disabling those volatile corpuscles, that are too indisposed to be fixed themselves, or are fitted to carry up with them such particles, as would not, without their help, ascend. That the expulsion of such parts is a proper means to make the aggregate of those, that remain more fixed, I presume you will not put me solicitously to prove; and we have a manifest instance of it in soot, where, though many active parts were by the violence of the fire and current of the air carried up together by the more volatile parts; yet, when soot is well distilled in a retort, a competent time being given for the extricating and avolation of the other parts, there will at the bottom remain a substance, that will not now fly away, as it formerly did. And here let me observe, that the recess of the fugitive corpuscles may contribute to the fixation of a body, not barely because the remaining matter is freed from so many unfixed, if not also volatilizing parts; but, as it may often happen, that upon their recess the pores or intervals, they left behind them, are filled up with more solid or heavy matter, and the body becomes, as more homogeneous, so more close and compact. And whereas I intimated, that, besides the expulsion of unfit corpucles, they may be otherwise disabled from hindering the fixation of the mass they belong to, I did it, because it seems very possible, that in some cases they may, by the action of the fire, be so broken, as with their fragments to fill up the pores or intervals of the body they appertained to; or may make such coalitions with the particles of a convenient additament, as to be no impediment to the fixity of the whole mass, though they remain in it. Which possibly you will think may well happen, when you shall have perused the instances annexed to the fourth way of fixing bodies.

THE third means of fixing, or lessening the volatility of bodies, is by preserving that rest among the parts, whose contrary is necessary to their volatilization. And this may be done by preventing or checking that heat, or other motion, which external agents strive to introduce into the parts of the proposed body. But this means tending rather to hinder the actual avolation of a portion of matter, or, at most, procure a temporary abatement of its volatility, than to give it a stable fixity, I shall not any longer insist on it.

THE fourth way of producing fixity in a body, is by putting to it such an appropriated additament, whether fixed or volatile, that the corpuscles of the body may be put among themselves, or with those of the additament, into a complicated state, or entangled contexture. This being the usual and principal way of producing fixity, we shall dwell somewhat the longer upon it, and give instances of several degrees of fixation. For, though they do not produce that quality in the strictest acceptation of the word, fixity; yet it is useful in our present enquiry, to take notice, by what means that volatility comes to be gradually abated, since that may facilitate our understanding, how the volatility of a body comes to be totally abated, and consequently the body to be fixed.

CHAP. IV.

AND first, we find, that a fixed additament, if its parts be conveniently shaped, may easily give a degree of fixity to a very volatile body. Thus spirit of nitre, that will of itself easily enough fly away in the air, having its saline particles associated with those of fixed nitre, or salt of tartar, will with the alcali compose a salt of a nitrous nature, which will endure to be melted in a crucible, without being deprived even of its spirits. And I have found, that the spirits of nitre, that abound in aqua-fortis, being concoagulated with the silver they corrode, though one would not expect, that such subtile corpuscles should stick fast to so compact and solid a body as silver; yet crystals produced by their coalition, being put into a retort, may be kept a pretty while in fusion, before the metal will let go the nitrous spirits. When we poured oil of vitriol upon the calx of vitriol, though many phlegmatick and other sulphureous particles were driven away by the excited heat; yet the saline parts, that combined with the fixed ones of the colcothar, stuck fast enough to them, not to be easily driven away. And if oil of vitriol be in a due proportion dropped upon salt of tartar, there results a *tartarum vitriolatum*, wherein the acid and alkalizate parts cohere so strongly, that it is not an ordinary degree of fire will be able to disjoin them. Insomuch, that divers chemists have (though very erroneously) thought this compounded salt to be indestructible. But a less heavy liquor than the ponderous oil of vitriol may, by an alkali, be more strongly detained, than that oil itself; experience having assured me, that spirit of salt being dropped to satiety upon a fixed alkali, (I used either that of nitre or of tartar,) there would be made so strict an union, that, having, without additaments, distilled the resulting salt with a strong and lasting fire, it appeared not at all considerably to be wrought upon, and was not so much as melted.

BUT it is not the bare mixture or commistion of volatile particles with fixed ones, (yea though the former be predominant in quantity,) that will suffice to elevate the latter. For, unless the figures of the latter be congruous and fitted to fasten to the other, the volatile parts will fly away in the heat, and leave the rest as fixed as before: as when sand or ashes are wetted or drenched with water, they quickly part with that water, without parting with any degree of their fixity. But on the other side, it is not always necessary, that the body, which is fitted to destroy, or much abate the volatility of another substance, should be itself fixed. For, if there be a skilful or lucky coaptation of the figures of the particles of both the bodies, these particles may take such hold of one another, as to compose corpuscles, that will neither by reason of their strict union be divided by heat; nor by reason of their resulting grossness be elevated even by a strong fire, or at least by such a degree of heat, as would have sufficed to raise more indisposed bodies than either of the separate ingredients of the mixture. This observation, if duly made out, does so much favour our doctrine about the mechanical origin of fixation, and may be of such use, not only to chemists, in some of their operations, but to philosophers, in assigning the causes of divers phænomena of nature, that it may be worth while to exemplify it by some instances.

THE first whereof I shall take from an usual practice of the chemists themselves: which I the rather do, to let you see, that such known experiments are too often over-looked by them, that make them, but yet may hint or confirm theories to those, that reflect on them. The instance, I here speak of, is that, which is afforded by the vulgar preparation of bezoardicum minerale. For, though the rectified butter, or oil of antimony, and the spirit of nitre, that are put together to make this white præcipitate, are both of them distilled liquors; yet the copious powder, that results from their union, is, by that union of volatile parts, so far fixed, that, after they have edulcorated it with water, they prescribe the calcining of it in a crucible for five or six hours: which operation it could not bear, unless it had attained to a considerable fixation. This discourse supposes with the generality of chemists, that the addition of a due quantity of spirit of nitre is necessary to be employed in making the bezoardicum minerale. But if it be a true observation, which is attributed to the learned *Guntherus Billichius*, (but which I had no furnace at hand to examine, when I heard of it,) if, I say, it be true, that a bezoardicum minerale may be obtained, without spirit of nitre, barely by a slow evaporation, made in a glass-dish, of the more fugitive parts of the oil of antimony; this instance will not indeed be proper in this place, but yet will belong to the second of the foregoing ways of introducing fixity. I proceed now to alledge other particulars, in favour of the above-mentioned observation.

IF you take strong spirit of salt, that, when the glass is unstopped, will smoke of itself in the cold air, and satiate it with the volatile spirit of urine, the superfluous moisture being abstracted, you will obtain by this preparation (which, you may remember, I long since communicated to you, and divers other virtuosi,) a compounded salt, scarce, if at all, distinguishable from sal armoniac, and which will not, as the salts it consists of will do, before their coalition, easily fly up of itself into the air, but will require a not despicable degree of fire to sublime it.

OF these semivolatile compositions of salt I have made, and elsewhere mentioned, others, which I shall not here repeat, but pass on to other instances, pertinent to our present design.

I lately mentioned, that the volatility of the spirits of nitre may be very much abated, by bringing them to coagulate into crystals, with particles of corroded silver; but I shall now add, that I guessed, and by trial found, that these nitrous spirits may be made much more fixed by the addition of the spirit of salt, which, if it be good, will of itself smoke in the air. For, having dissolved a convenient quantity of crystals of silver in distilled water, and precipitated them, not with a solution of salt, but the spirit of salt; the phlegm being abstracted, and some few of the loose saline particles; though the remaining mass were pressed with a violent fire, that kept the retort red-hot for a good while; yet the nitrous and saline spirits would by no means be driven away from the silver, but continued in fusion with it; and when the mass was taken out, these spirits did so abound in it, that it had no appearance of a metal, but looked rather like a thick piece of horn.

THE next instance I shall name is afforded us by that kind of turbith, which may be made by oil of vitriol, instead of the aqua-fortis employed in the common turpethum minerale. For, though oil of vitriol be a distilled liquor, and mercury a body volatile enough; yet, when we abstracted four or five parts of oil of vitriol from one of quick-silver, (especially if the operation were repeated,) and then washed off as much as we could of the saline particles of the oil of vitriol; yet those, that, remained adhering to the mercury made it far more fixed, than either of the liquors had been before, and enabled it even in a crucible, to endure such a degree of fire, before it could be driven away, as, I confess, I somewhat wondered at. The like turbith may be made with oil of sulphur *per campanam*. But this is nothing to what *Helmont* tells us of the operation of his alkahest, where he affirms, that that menstruum, which is volatile enough, being abstracted from running mercury, not only coagulates it, but leaves it fixed, so that it will endure the brunt of fires actuated by bellows, (*omnem follium ignem*.) If this be certain, it will not be a slender proof, that fixity may be mechanically produced; and however, the argument will be good in reference to the Helmontian Spagyrists. For if, as one would expect, there do remain some particles of the menstruum with those of the metal, it will not be denied, that two volatile substances may perfectly fix one another. And if, as *Helmont* seems to think, the menstruum be totally abstracted, this supposition will the more favour our doctrine about fixity; since, if there be no material additament left with the quick-silver, the fixation cannot so reasonably be ascribed to any thing, as to some new mechanical modification, and particularly to some change of texture introduced into the mercury itself.

AND that you may think this the less improbable, I will now proceed to some instances, whereof the first shall be this; that, having put a mixture made of a certain proportion of two dry, as well as volatile bodies, (viz. sal armoniac, and flower or very fine powder of sulphur,) to half its weight of common running mercury, and elevated this mixture three or four times from it, (in a conveniently shaped, and not over-wide glass) the mercury, that lay in the bottom, in the form of a ponderous and somewhat purplish powder, was, by this operation, so fixed, that it long endured a strong fire, which at length was made so strong, that it melted the glass, and kept it melted, without being strong enough to force up the mercury: which, by some trials, not so proper to be here mentioned, seemed to have its salivating and emetick powers extraordinarily infringed, and sometimes quite suppressed. But this only upon the bye. In all the other instances, (wherewith I shall conclude these notes,) I shall employ one menstruum, oil of vitriol, and shew you the efficacy of it in fixing some parts of volatile bodies with some parts of itself; by which examples it may appear, that a volatile body may not only lessen the volatility of another body, as in the lately mentioned case of our spirituous sal armoniac; but that two substances, that apart were volatile, may compose a third, that will not only be less volatile, but considerably, if not altogether, fixed.

WE mixed then, by degrees, about equal parts of oil of vitriol and oil of turpentine: and though each of them single, especially the latter, will ascend with a moderate fire in a sand-furnace; yet, after the distillation was ended, we had a considerable quantity, sometimes, if I misremember not, a fifth or sixth part, of a caput mortuum, black as a coal, and whereof a great part was of a scarce to be expected fixedness in the fire.

TO give a higher proof of the disposition, that oil of vitriol has to let some of its parts grow fixed by combination with those of an exceeding volatile additament, I mixed this liquor with an equal or double weight of highly rectified spirit of wine, and not only after, but sometimes without, previous digestion, I found, that the fluid parts of the mixture being totally abstracted, there would remain a pretty

quantity

quantity of a black ſubſtance ſo fixed, as to afford juſt cauſe of wonder.

AND becauſe camphire is eſteemed the moſt fugitive of conſiſtent bodies, in regard that, being but laid in the free air, without any help of the fire, it will fly all away; I tried, what oil of vitriol, abſtracted from camphire, would do; and found at the bottom of the retort a greater quantity, than one would expect, of a ſubſtance as black as pitch, and almoſt as far from the volatility, as from the colour of camphire, though it appeared not, that any of the gum had ſublimed into the neck of the retort.

FROM all which inſtances it ſeems manifeſtly enough to follow, that in many caſes there needs nothing to make aſſociated particles, whether volatile or not, become fixed, but either to implicate or entangle them among themſelves, or bring them to touch one another; according to large portions of their ſurfaces, or by both theſe ways conjointly, or by ſome others, to procure the firm cohæſion of ſo many particles, that the reſulting corpuſcles be too big or heavy to be, by the degree of fire, wherein they are ſaid to be fixed, driven up into the air.

EXPERIMENTS AND NOTES, ABOUT THE MECHANICAL ORIGIN or PRODUCTION OF CORROSIVENESS AND CORROSIBILITY.

SECTION I.

About the MECHANICAL ORIGIN *of* CORROSIVENESS.

I DO not, in the following notes, treat of corroſiveneſeſs in their ſtrict ſenſe of the word, who aſcribe this quality only to liquors, that are notably acid or ſour, ſuch as aqua fortit, ſpirir of ſalt, vinegar, juice of lemons, &c. but, that I may not be obliged to overlook urinous, oleous, and divers other ſolvents, or to coin new names for their differing ſolutive powers, I preſume to employ corroſiveneſs in a greater latitude, ſo as to make it almoſt equivalent to the ſolutive power of liquors, referring other menſtruums to thoſe, that are corroſive or fretting, (though not always as to the moſt proper, yet) as to the principal and beſt known ſpecies; which I the leſs ſcruple here to do, becauſe I have * elſewhere more diſtinctly enumerated and ſorted the ſolvents of bodies.

THE attributes, that ſeem the moſt proper to qualify a liquor to be corroſive, are all of them mechanical, being ſuch as are theſe, that follow:

FIRST, that the menſtruum conſiſt of, or abound with corpuſcles not too big to get in at the pores or commiſſures of the body to be diſſolved; nor yet be ſo very minute, as to paſs through them, as the beams of light do through glaſs; or to be unable, by reaſon of their great ſlenderneſs and flexibility, to disjoin the parts they invade.

SECONDLY, that theſe corpuſcles be of a ſhape fitting them to inſinuate themſelves, more or leſs, into the pores or commiſſures above-mentioned, in order to the diſſociating of the ſolid parts.

THIRDLY,

* This refers to an eſſay of the author's about the uſefulneſs of chemiſtry to, &c.

THIRDLY, that they have a competent degree of solidity to disjoin the particles of the body to be dissolved; which solidity of solvent corpuscles is somewhat distinct from their bulk, mentioned in the first qualification; as may appear, by comparing a stalk of wheat and a metalline wire of the same diameter, or a flexible wand of osier, of the bigness of one's little finger, with a rigid rod of iron of the same length and thickness.

FOURTHLY, that the corpuscles of the menstruum be agile and advantaged for motion, (such as is fit to disjoin the parts of the invaded body) either by their shape, or their minuteness, or their fitness to have their action befriended by adjuvant causes; such as may be (first) the pressure of the atmosphere, which may impel them into the pores of bodies not filled with a substance so resisting as common air: as we see, that water will, by the prevalent pressure of the ambient, whether air or water, be raised to the height of some inches in capillary glasses, and in the pores of spunges, whose consistent parts, being of easier cession than the sides of glass-pipes, those pores will be enlarged, and consequently those sides disjoined, as appears by the dilatation and swelling of the spunge: and (secondly) the agitation, that the intruding corpuscles may be fitted to receive in those pores or commissures, by the transcursion of some subtile aetherial matter; or by the numerous knocks and other pulses of the swimming or tumbling corpuscles of the menstruum itself, (which, being a fluid body, must have its small parts perpetually and variously moved) whereby the engaged corpuscles, like so many little wedges and leavers, may be enabled to wrench open, or force asunder the little parts between which they have insinuated themselves. But I shall not here prosecute this theory, (which, to be handled fully, would require a discourse apart) since these conjectures are proposed but to make it probable in the general, that the corrosiveness of bodies may be deduced from mechanical principles: but whether best from the newly proposed ones, or any other, need not be anxiously considered in these notes, where the things mainly intended and relied on are the experiments and phænomena themselves.

EXPERIMENT I.

IT is obvious, that, though the recently expressed juice of grapes be sweet, whilst it retains the texture, that belongs to it, as it is new, (especially, if it be made of some sorts of grapes, that grow in hot regions;) yet, after fermentation, it will, in tract of time, as it were spontaneously, degenerate into vinegar. In which liquor, to a multitude of the more solid corpuscles of the must, their frequent and mutual attritions may be supposed to have given edges like those of the blades of swords or knives; and in which, perhaps, the confused agitation, that preceded, extricated, or, as it were, unsheathed some acid particles, that (derived from the sap of the vine, or, perchance, more originally from the juice of the earth,) were at first in the must, but lay concealed, and, as it were, sheathed among the other particles, wherewith they were associated, when they were pressed out of the grapes. Now this liquor, that by the forementioned, or other like mechanical changes, is become vinegar, does so abound with corpuscles, which, on the account of their edges, or their otherwise sharp and penetrative shape, are acid and corrosive, that the better sort of it will, without any preparation, dissolve coral, crab's-eyes, and even some stones, lapis stellaris in particular, as also minium, or the calx of lead, and even crude copper, as we have often tried. And not only the distilled spirit of it will do those things more powerfully, and perform some other things, that mere vinegar cannot; but the saline particles, wont to remain after distillation, may, by being distilled and cohobated *per se*, or by being skilfully united with the foregoing spirit, be brought to a menstruum of no small efficacy in the dissolution, and other preparations of metalline bodies, too compact for the mere spirit itself to work upon.

FROM divers other sweet things also may vinegar be made; and even of honey, skilfully fermented with a small proportion of common water, may be made a vinegar stronger than many of the common wine-vinegars; as has been affirmed to me by a very candid physician, who had occasion to deal much in liquors.

EXPERIMENT II.

NOT only several dry woods, and other bodies, that most of them pass for insipid, but honey and sugar themselves afford by distillation acid spirits, that will dissolve coral, pearls, &c. and will also corrode some metals and metalline bodies themselves; as I have often found by trial. So that the violent operation of the fire, that destroys what they call the form of the distilled body, and works, as a mechanical agent, by agitating, breaking, dissipating, and under a new constitution reassembling the parts, procures for the distiller an acid corrosive menstruum; which, whether it be brought to pass by making the corpuscles rub one another into the figure of little sharp blades, or by splitting some solid parts into sharp or cutting corpuscles, or by unsheathing, as it were, some parts, that, during the former texture of the body, did not appear to be acid; or whether it be rather effected by some other mechanical way, may in due time be further considered.

EXPERIMENT III.

IT is observed by refiners, goldsmiths and chemists, that aqua fortis and aqua regia, which are corrosive menstruums, dissolve metals, the former of them silver, and the latter gold, much more speedily and copiously, when an external heat gives their intestine motions a new degree of vehemency or velocity, which is but a mechanical thing; and yet this superadded measure of agitation is not only in the above-

abovementioned inſtances a powerfully aſſiſtant cauſe in the ſolutions made by the lately mentioned corroſive liquors, but is that, without which ſome menſtruums are not wont ſenſibly to corrode ſome bodies at all, as we have tried in keeping quick-ſilver in three or four times its weight of oil of vitriol; ſince in this menſtruum I found not the mercury to be diſſolved, or corroded, though I kept it a long time in the cold: whereas, when the oil of vitriol was excited by a convenient heat, (which was not faint) it corroded the mercury into a fine white calx or powder, which by the affuſion of fair water, would be preſently turned into a yellowiſh calx of the colour and nature of a turbith. I remember alſo, that having, for trial's ſake, diſſolved in a weak ſpirit of ſalt a fourth part of weight of fine cryſtals of nitre, we found, that it would not in the cold (at leaſt during a good while, that we waited for its operation) diſſolve leaf-gold; but when the menſtruum was a little heated at the fire, the ſolution proceeded readily enough. And in ſome caſes, though the external heat be but ſmall, yet there may intervene a briſk heat, and much cooperate in the diſſolution of a body; as for inſtance, of quick-ſilver in aqua fortis. For it is no prodigy to find, that when a full proportion of that fluid metal has been taken, the ſolution, though at firſt altogether liquid, and as to ſenſe uniform, comes to have, after a while, a good quantity of coagulated or cryſtallized matter at the bottom, of which the cauſe may be, that in the very act of corroſion there is excited an intenſe degree of heat, which conferring a new degree of agitation to the menſtruum, makes it diſſolve a good deal more, than afterwards, when the conflict is over, it is able to keep up.

EXPERIMENT IV.

WE have obſerved alſo, that agitation does in ſome caſes ſo much promote the diſſolutive power of ſaline bodies, that though they be not reduced to that ſubtilty of parts, to which a ſtrong diſtillation brings them; yet they may in their groſſer and cruder form have the power to work on metals; as I elſewhere ſhew, that by barely boiling ſome ſolutions of ſalts of a convenient ſtructure, as nitre, ſal armoniac, &c. with foliated gold, ſilver, &c. we have corroded theſe metals, and can diſſolve ſome others. And by boiling crude copper (in filings) with ſublimate and common water, we were able, in no long time, to make a ſolution of the metal.

EXPERIMENT V.

SOMETIMES alſo, ſo languid an agitation, as that, which ſeems but ſufficient to keep a liquor in the ſtate of fluidity, may ſuffice to give ſome dry bodies a corroding power, which they could not otherwiſe exerciſe; as in the way of writing one's name (or a motto) upon the blade of a knife with common ſublimate: for, if having very thinly overlaid which ſide you pleaſe with bees-wax, you write with a bodkin, or ſome pointed thing upon it; the wax being thereby removed from the ſtrokes made by the ſharp body, it is eaſy to etch with ſublimate; ſince you need but ſtrew the powder of it upon the place bared of the wax, and wet it well with mere common water; for ſtrong vinegar is not neceſſary. For, after a while, all the parts of the blade, that ſhould not be fretted, being protected by the caſe or film of wax, the ſublimate will corrode only where way has been made for it by the bodkin, and the letters will be more or leſs deeply engraven (or rather etched) according to the time the ſublimate is ſuffered to lie on. And if you aim only at a legible impreſſion, a few minutes of an hour (as four or five) may ſerve the turn.

EXPERIMENT VI.

THIS brings into my mind an obſervation I have ſometimes had occaſion to make, that I found more uſeful than common; and it is, that divers bodies, whether diſtilled or not diſtilled, that are not thought capable of diſſolving other bodies, becauſe in moderate degrees of heat they will not work on them, may yet, by intenſe degrees of heat, be brought to be fit ſolvents for them. To which purpoſe I remember, that having a diſtilled liquor, which was rather ſweet to the taſte, than either acid, lixiviate, or urinous, though for that reaſon it ſeemed unfit to work on pearls, and accordingly did not diſſolve them in a conſiderable time, wherein they were kept with it in a more than ordinarily warm digeſtion; yet the glaſs being for many hours (amounting perhaps to ſome days) kept in ſuch an heat of ſand as made the liquor boil, we had a diſſolution of pearls, that uniting with the menſtruum, made it a very valuable liquor. And though the ſolvents of crude gold, wont to be employed by chemiſts, are generally diſtilled liquors, that are acid, and in the lately mentioned ſolvent, made of crude ſalts and common water, acidity ſeemed to be the predominant quality (which makes the uſe of ſolutions made in aqua regia, &c. ſuſpected by many phyſicians and chemiſts;) yet fitly choſen alcalizate bodies themſelves, as repugnant as they uſe to be to acids, without the help of any liquor, will be enabled, by a melting fire, in no long time, to penetrate and tear aſunder the parts even of crude gold; ſo that it may afterwards be eaſily taken up in liquors, that are not acid, or even by water itſelf.

EXPERIMENT VII.

THE tract about ſalt-petre, that gave occaſion to theſe annotations, may furniſh us with an eminent inſtance of the production of ſolvents. For, though pure ſalt-petre itſelf, when diſſolved in water, is not obſerved to be a menſtruum for the ſolution of the metals hereafter to be named, or ſo much as of coral itſelf; yet when, by a convenient diſtillation, its parts are ſplit, if I may ſo ſpeak, and by attrition, or other mechanical ways of working on them, reduced to the ſhapes of acid and alcalizate ſalts, it then affords two ſorts of menſtruums, of very differing natures, which, betwixt them, diſſolve or corrode a great number and variety of bodies; as the ſpirit of nitre, without addition, is a ſolvent for moſt metals, as ſilver, mercury, copper, lead, &c. and alſo divers mineral bodies, as tin-glaſs, ſpelter, lapis calaminaris, &c. and the fixed ſalt of nitre operates upon ſulphureous minerals, as common ſulphur, antimony, and divers other bodies, of which I elſewhere make mention.

EXPERIMENT VIII.

BY the former trials it has appeared, that the encreaſe of motion, in the more penetrating corpuſcles of a liquor, contributes much to its ſolutive power; and I ſhall now add, that the ſhape and ſize, which are mechanical affections, and ſometimes alſo the ſolidity of the ſame corpuſcles does eminently concur to qualify a liquor to diſſolve this or that particular body. Of this, even ſome of the more familiar practices of chemiſts may ſupply us with inſtances. For there is no account ſo probable as may be given upon this ſuppoſition, why aqua fortis, which will diſſolve ſilver, without meddling with gold, ſhould, by the addition of a fourth part of its weight of ſal armoniac, be turned into aqua regia, which, without meddling with ſilver, will diſſolve gold. But there is no neceſſity of having recourſe to ſo groſs and compounded a body as ſal armoniac, to enable aqua fortis to diſſolve gold: for, the ſpirit of common ſalt alone, being mingled in a due proportion, will ſuffice for that purpoſe. Which (by the way) ſhews, that the volatile ſalt of urine and ſoot, that concur to the making up of ſal armoniac, are not neceſſary to the diſſolution of gold, for which a ſolvent may be made with aqua fortis and crude ſea ſalt. I might add, that the mechanical affections of a menſtruum, may have ſuch an intereſt in its diſſolutive power, that even mineral or metalline corpuſcles may become uſeful ingredients of it, though, perhaps, it be a diſtilled liquor; as might be illuſtrated by the operations of ſome compounded ſolvents, ſuch as is the oil of antimony made by repeated rectifications of what chemiſts call its butter, which, whatever ſome ſay to the contrary, does much abound in antimonial ſubſtance.

EXPERIMENT IX.

BUT I ſhall return to our aqua regia, becauſe the mention I had occaſion to make of that ſolvent, brought into my mind what I deviſed, to make it probable, that a ſmaller change, than one would lightly imagine, of the bulk, ſhape, or ſolidity of the corpuſcles of a menſtruum, may make it fit to diſſolve a body it would not work on before. And this I the rather attempted, becauſe the warier ſort of chemiſts themſelves are very ſhy of the inward uſe or preparations made of gold by the help of aqua fortis, becauſe of the odious ſtink they find, and the venenoſity they ſuſpect in that corroſive menſtruum: whereas ſpirit of ſalt we look upon as a much more innocent liquor, whereof, if it be but diluted with fair water, or any ordinary drink, a good doſe may be ſafely given inwardly, though it have not wrought upon gold, or any other body, to take off its acrimony. But, whether or no this prove of any great uſe in phyſick, wherein, perhaps, if any quantity of gold be to be diſſolved, a greater proportion of ſpirit of nitre would be needed; the ſucceſs will not be unfit to be mentioned, in reference to what we were ſaying of ſolvents. For, whereas we find not, that our ſpirit of ſalt here, in *England*, will at all diſſolve crude gold, we found, that by putting ſome leaf-gold into a convenient quantity of good ſpirit of ſalt, when we had dropped in ſpirit of nitre, (ſhaking the glaſs at each drop) till we perceived, that the mixture was juſt able, in a moderate heat, to diſſolve the gold, we found, that we had been obliged to employ but after the rate of twelve drops of the latter liquor to an ounce of the former; ſo that, ſuppoſing each of theſe drops to weigh a grain, the fortieth part of ſpirit of nitre being added, ſerved to turn the ſpirit of ſalt into a kind of aqua regia. But to know the proportion otherwiſe than by gueſs, we weighed ſix other drops of the ſame ſpirit of ſalt, and found them to amount not fully to three grains and an half: whence it appeared, that we added but about a ſeventieth part of the nitrous ſpirit to that of ſalt.

THE experiments, that have been hitherto recited, relate chiefly to the production of corroſive menſtruums; and therefore I ſhall now add an account of a couple of trials, that I made manifeſtly to leſſen, or quite to deſtroy corroſiveneſs in liquors very conſpicuous for that quality.

EXPERIMENT X.

WHEREAS one of the moſt corroſive menſtruums, that is yet known, is oil of vitriol, which will fret in pieces both divers metals and minerals, and a great number and variety of animal and vegetable bodies; yet if you digeſt with it, for a while only, an equal weight of highly-rectified ſpirit of wine, and afterwards diſtil the mixture very warily, (for elſe the experiment may very eaſily miſcarry,) you may obtain a pretty deal of liquor not corroſive at all, and the remaining ſubſtance will be reduced partly into a liquor, which, though acid, is not more ſo than one part of good oil of vitriol will make ten times as much common water, by being well mingled with it; and partly into a dry ſubſtance, that has ſcarce any taſte at all, much leſs a corroſive one.

EXPERIMENT XI.

AND though good aqua fortis be the moſt generally employed of corroſive menſtruums, as being capable of diſſolving or corroding, not only many minerals, as tin-glaſs, antimony, zink, &c. but all metals, except gold, (for, though it make not a permanent ſolution

ſolution of crude tin, it quickly frets the parts aſunder, and reduces it to an immalleable ſubſtance;) yet, to ſhew how much the power of corroding may be taken away, by changing the mechanical texture of a menſtruum, even without ſeeming to deſtroy the fretting ſalts, I practiſed (and communicated to divers virtuoſi) the following experiment, elſewhere mentioned to other purpoſes.

WE took equal parts of good aqua fortis, and highly dephlegmed ſpirit of wine, and having mingled them warily, and by degrees, (without which caution the operation may prove dangerous) we united them by two or three diſtillations of the whole mixture; which afterwards we found not to have the leaſt fretting taſte, and to be ſo deprived of its corroſive nature, that it would not work upon ſilver, though by precipitation, or otherwiſe, reduced to very ſmall parts; nay, it would ſcarce ſenſibly work, in a good while, on filings of copper, or upon other bodies, which mere vinegar, or, perhaps, rheniſh wine, will corrode. Nay, I remember, that, with another ſpirit, (that was not urinous) and afterwards, with alkohol of wine, we ſhewed a more ſurpriſing ſpecimen of the power of either deſtroying, or debilitating, the corroſiveneſs of a menſtruum, and checking its operation. For, having cauſed a piece of copper plate to be put into one ounce of aqua fortis, when this liquor was eagerly working upon the metal, I cauſed an ounce of the alkohol of wine, or the other ſpirit to be poured, (which it ſhould warily be) upon the agitated mixture; whoſe effervеſcence, at the firſt inſtant, ſeemed to be much encreaſed, but preſently after was checked, and the corroſivenſs of the menſtruum being ſpeedily diſabled or corrected, the remaining copper was left undiſſolved at the bottom.

NOR are theſe the only acid menſtruums, that I have, many years ſince, been able to correct by ſuch a way: for I applied it to others, as ſpirit of nitre, and even aqua regis itſelf; but it has not an equal operation upon all, and leaſt of all (as far as I can remember) upon ſpirit of ſalt; as, on the other ſide, ſtrong ſpirit of nitre was the menſtruum, upon which its effects were the moſt ſatisfactory.

MOST of the chemiſts pretend, that the ſolutions of bodies are performed by a certain cognation and ſympathy between the menſtruum and the body it is to work upon. And it is not to be denied, that, in divers inſtances, there is, as it were, a conſanguinity between the menſtruum and the body to be diſſolved; as when ſulphur is diſſolved by oils, whether expreſſed or diſtilled: but yet, as the opinion is generally propoſed, I cannot acquieſce in it, partly, becauſe there are divers ſolutions and other phænomena, where it will not take place; and partly, becauſe, even in thoſe inſtances, wherein it is thought moſt applicable, the effect ſeems to depend upon mechanical principles.

EXPERIMENT XII.

AND firſt, it will be difficult to ſhew, what conſanguinity there is between ſal gem, and antimony, and iron, and zink, and bread, and camphire, and lapis calaminaris, and fleſh of divers kinds, and oiſter-ſhells, and hartſhorn, and chalk, and quick-lime; ſome of which belong to the vegetable, ſome to the mineral, and ſome to the animal kingdom; and yet all of them, and divers others, as I have tried, may, even without the aſſiſtance of external heat, be diſſolved or corroded by one ſingle mineral menſtruum, oil of vitriol. And, which is not to be neglected on this occaſion, ſome of them may be bodies, ſuppoſed, by chemiſts, to have an antipathy to each other, in point of corroſion, or diſſolution.

EXPERIMENT XIII.

I Obſerve alſo, that a diſſolution may be made of the ſame body by menſtruums, to which the chemiſts attribute (as I juſt now obſerved they did to ſome bodies) a mutual antipathy, and which therefore are not like to have a ſympathy with the ſame third body; as I found by trial, that both aqua fortis, and ſpirit of urine, upon whoſe mixture there enſues a conflict, with a great effervеſcence, will, each of them apart, readily diſſolve crude zink, and ſo each of them will the filings of copper. Not to mention, that pure ſpirit of wine, and oil of vitriol, as great a difference as there is between them, in I know not how many reſpects, and as notable a heat as will enſue upon their commixture, will each of them diſſolve camphire; to which may be added, other inſtances of the like nature. As for what is commonly ſaid, that oils diſſolve ſulphur, and ſaline menſtruums metals, becauſe (as they ſpeak) *ſimile ſimili gaudet*; I anſwer, that, where there is any ſuch ſimilitude, it may be very probably aſcribed, not ſo much with the chemiſts, that favour *Ariſtotle*, to the eſſential forms of the bodies, that are to work on each other; nor, with the mere chemiſts, to their ſalt, or ſulphur, or mercury, as ſuch; but to the congruity between the pores and figures of the menſtruum, and the body diſſolved by it, and to ſome other mechanical affections of them.

EXPERIMENT XIV.

FOR ſilver, for example, not only will be diſſolved by nitre, which they reckon a ſalt, but be amalgamed with, and conſequently diſſolved by quickſilver, and alſo by the operation of brimſtone, be eaſily incorporated with that mineral, which chemiſts are wont to account of ſo oleaginous a nature, and inſoluble in aqua fortis.

EXPERIMENT XV.

AND as for thoſe diſſolutions, that are made with oily and inflammable menſtruums, of common ſulphur and other inflammable bodies, the diſſolution does not make for them ſo clearly as they imagine. For, if ſuch menſtruums operate, as is alledged, upon the account of their being, as well as the bodies they work upon, of a ſulphureous nature, whence

whence is it, that highly rectified ſpirit of wine, which, according to them, muſt be of a moſt ſulphureous nature, ſince, being ſet on fire, it will flame all away, without leaving one drop behind it, will not (unleſs, perhaps, after a tedious while) diſſolve even flowers of brimſtone, which eſſential, as well as expreſſed oils, will eaſily take up; as ſpirit of wine itſelf alſo will do almoſt in a trice, if, (as we ſhall ſee anon) by the help of an alkali, the texture of the brimſtone be altered, though the only thing, that is added to the ſulphur, being an incombuſtible ſubſtance, is nothing near of ſo ſulphureous a nature, as the flowers, and need have no conſanguinity upon the ſcore of its origin with ſpirit of wine, as it is alledged, that ſalt of tartar has; ſince I have tried, that fixed nitre, employed inſtead of it, will do the ſame.

EXPERIMENT XVI.

THE mention of nitre brings into my mind, that the ſalt-petre, being wont to be looked upon, by chemiſts, as a very inflammable body, ought, according to them, to be of a very ſulphureous nature; yet we find not, that it is in chemical oils, but in water, readily diſſolved. And whereas chemiſts tell us, that the ſolutions of alkalies, ſuch as ſalt of tartar, or of pot-aſhes in common oils, proceed from the great cognition between them, I demand, whence it happens, that ſalt of tartar will, by boiling, be diſſolved in the expreſſed oil of almonds, or of olives, and be reduced with it to a ſoapy body, and that yet, with the eſſential oil of juniper or anniſeeds, &c. where what they call the ſulphur is made pure and penetrant, being freed from the earthy, aqueous and feculent parts, which diſtillation diſcovers to be in the expreſſed oils, you may boil ſalt of tartar twenty times as long, without making any ſoap of them, or perhaps any ſenſible ſolution of the alkali. And chemiſts know, how difficult it is, and how unſucceſsfully it is wont to be attempted, to diſſolve pure ſalt of tartar in pure ſpirit of wine, by digeſting the not peculiarly prepared ſalt in the cognate menſtruum. I will not urge, that, though the moſt conſpicuous mark of ſulphur be inflammability, and is in an eminent degree to be found in oil, as well as ſulphur; yet an alkali and water, which are neither ſingly, nor united inflammable, will diſſolve common ſulphur.

EXPERIMENT XVII.

BUT, to make it probable againſt the chemiſts, (for I purpoſe it but as an argument *ad hominem*) that the ſolution of ſulphur in expreſſed oils depends upon ſomewhat elſe beſides the abundance of the ſecond principle, in both the bodies; I will add to what I ſaid before, an affirmation of divers chemical writers themſelves, who reckon aqua regis, which is plainly a ſaline menſtruum, and diſſolves copper, iron, coral, &c. like acid liquors, among the ſolvents of ſulphur, and by that power, among other things, diſtinguiſh it from aqua fortis. And, on the other ſide, if there be a congruity betwixt an expreſſed oil and another body, though it be ſuch as, by its eaſy diſſolubleneſs in acid ſalts, chemiſts ſhould pronounce to be of a ſaline nature, an expreſſed oil will readily enough work upon it; as I have tried, by digeſting even crude copper, in filings, with oil of ſweet almonds, which took up ſo much of the metal, as to be deeply coloured thereby, as if it had been a corroſive liquor: nay, I ſhall add, that, even with milk, as mild a liquor as it is, I have found, by trial, that, without the help of fire, a kind of diſſolution may, though not in few hours, be made of crude copper, as appeared by the greeniſh blue colour the filings acquired, when they had been well drenched in the liquor, and left for a certain time in the veſſel, where the air had very free acceſs to them.

EXPERIMENT XVIII.

BESIDES the argument *ad hominem*, newly drawn from aqua regia, it may be proper enough to urge another of the ſame kind, upon the generality of the Helmontians and Paracelſians, who admit what the heads of their ſects delivered concerning the operations of the alkaheſt. For whereas it is affirmed, that this irreſiſtible menſtruum will diſſolve all tangible bodies here below, ſo as they may be reduced into inſipid water; as, on the one ſide, it will be very hard to conceive, how a ſpecificated menſtruum, that is determined to be either acid, or lixiviate, or urinous, &c. ſhould be able to diſſolve ſo great a variety of bodies of differing, and perhaps contrary natures, in ſome whereof acids, in others, lixiviate ſalts, and in others, urinous are predominant; ſo, on the other ſide, if the alkaheſt be not a ſpecificated menſtruum, it will very much disfavour the opinion of the chemiſts, that will have ſome bodies diſſoluble only by acids as ſuch, others by fixed alkalies, and others again by volatile ſalts; ſince a menſtruum, that is neither acid, lixiviate, nor urinous, is able to diſſolve bodies, in ſome of which one, and in others, another of thoſe principles is predominant: ſo that, if a liquor be conveniently qualified, it is not neceſſary, that it ſhould be either acid to diſſolve pearl or coral, or alkalizate to diſſolve ſulphur. But upon what mechanical account an analyzing menſtruum may operate, is not neceſſary to be here determined. And I elſewhere offer ſome thoughts of mine about it.

EXPERIMENT XIX.

IF we duly reflect upon the known proceſs, that chemiſts are wont to employ in making mercurius dulcis, we ſhall find it very favourable to our hypotheſis. For though we have already ſhewn in the fifth experiment, and it is generally confeſſed, that common ſublimate made of mercury is a highly corroſive body; yet, if it be well ground with near an equal weight of quickſilver, and be a few times ſublimed, (to mix them the more exactly) it it will become so mild, that it will not so much as taste sharp upon the tongue; so that chemists are wont to call it mercurius dulcis: and yet this dulcification seems to be performed in a mechanical way. For most part of the salts, that made the sublimate so corrosive, abide in the mercurius dulcis; but by being compounded with more quicksilver, they are diluted by it, and (which is more considerable) acquire a new texture, which renders them unfit to operate, as they did before, when the fretting salts were not joined with a sufficient quantity of the mercury to inhibit their corrosive activity. It may perhaps somewhat help us to conceive, how this change may be made, if we imagine, that a company of mere knife-blades be first fitted with hafts, which will in some regard lessen their wounding power by covering or casing them at that end, which is designed for the handle; (though their insertion into those hafts, turning them into knives, makes them otherwise the fitter to cut and pierce) and, that each of them be afterwards sheathed, (which is, as it were, a hafting of the blades too;) for then they become unfit to cut or stab, as before, though the blades be not destroyed: or else we may conceive these blades without hafts or sheaths to be tied up in bundles, or as it were in little faggots with pieces of wood, somewhat longer than themselves, opportunely placed between them. For neither in this new constitution would they be fit to cut and stab as before. And by conceiving the edge of more or fewer of the blades to be turned inwards, and those, that are not, to have more or less of their points and edges to be sheathed, or otherwise covered by interposed bodies, one may be helped to imagine, how the genuine effects of the blades may be variously lessened or diversified. But, whether these, or any other like changes of disposition be fancied, it may by mechanical illustrations become intelligible, how the corrosive salts of common sublimate may lose their efficacy, when they are united with a sufficient quantity of quicksilver in mercurius dulcis: in which new state, the salts may indeed, in a chemical phrase, be said to be satiated; but this chemical phrase does not explicate, how this saturation takes away the corrosiveness from salts, that are still actually present in the sweet mercury. And by analogy to some such explication, as the above proposed, a possible account may be rendered, why fretting salts do either quite lose their sharpness, as alkalies, whilst they are imbodied with sand in common glass; or lose much of their corrosive acidity, as oil of vitriol does, when with steel it composes vitriolum martis; or else are transmuted or disguised by conjunction with some corroded bodies of a peculiar texture, as when aqua fortis does with silver make an extremely bitter salt or vitriol, and with lead one, that is positively sweet, almost like common saccharum saturni.

EXPERIMENT XX.

TO shew, how much the efficacy of a menstruum may depend even upon such seemingly slight mechanical circumstances, as one would not easily suspect any necessity of, I shall employ an experiment, which, though the unpractised may easily fail of making well, yet, when I tried it after the best manner, I did it with good success. I put then upon lead a good quantity of well rectified aqua-fortis, in which the metal, as I expected, continued undissolved; though, if the chemists say truly, that the dissolving power of the menstruum consists, only in the acid salts, that it abounds with, it seems naturally to follow, that the more abundance of them there is in a determinate quantity of the liquor, it should be the more powerfully able to dissolve metalline and mineral bodies. And in effect, we see, that, if corrosive menstruums be not sufficiently dephlegmed, they will not work on divers of them. But, notwithstanding this plausible doctrine of the chemists, conjecturing, that the saline particles, that swam in our aqua fortis, might be more thronged together, than was convenient for a body of such a texture of saline parts, and such intervals between them, I diluted the menstruum, by adding to it what I thought fit of fair water, and then found, that the desired congruity betwixt the agent and the patient emerged, and the liquor quickly began to fall upon the metal, and dissolve it. And if you would try an experiment to the same purpose, that needs much less circumspection to make it succeed, you may, instead of employing lead, reiterate what I elsewhere mention my self to have tried with silver, which would not dissolve in too strong aqua-fortis, but would be readily fallen upon by that liquor, when I had weakened it with common water.

AND this it may suffice to have said at present of the power or faculty, that is found in some bodies of corroding or dissolving others. Whereof I have not found among the Aristotelians, I have met with, so much as an offer at an intelligible account. And I the less expect the vulgar chemists will from their hypostatical principles afford us a satisfactory one, when, besides the particulars, that from the nature of the things, and *Helmont*'s writings, have been lately alledged against their hypothesis, I consider, how slight accounts they are wont to give us even of the familiar phænomena of corrosive liquors. For if, for example, you ask a vulgar chemist, why aqua-fortis dissolves silver and copper, it is great odds but he will tell you, it is because of the abundance of fretting salt, that is in it, and has a cognation with the salts of the metal. And if you ask him, why spirit of salt dissolves copper, he will tell you it is for the same reason; and yet, if you put spirit of salt, though very strong, to aqua-fortis, this liquor will not dissolve silver, because upon the mixture the liquors acquire a new constitution as to the saline particles, by virtue of which the mixture will dissolve, instead of silver, gold. Whence we may argue against the chemists, that the inability of this compounded liquor to work on silver does not proceed from its being weakened by the spirit of salt; as well because, according to them, gold is far the more compact metal of the two, and requires a more potent menstruum to work upon it, as because this same compounded liquor will readily dissolve copper. And to the same purpose, with this experiment I should alledge divers others, if I thought this the fittest place, wherein I could propose them.

SECTION II.

About the Mechanical Origin of CORROSIBILITY.

CORROSIBILITY being the quality, that answers corrosiveness, he, that has taken notice of the advertisement I formerly gave about my use of the term * corrosiveness, in these notes, may easily judge, in what sense I employ the name of the other quality; which, whether you will stile it opposite or conjugate, for want of a better word, I call corrosibility.

THIS corrosibility of bodies is, as well as their corrosiveness, a relative thing; as we see, that gold, for instance, will not be dissolved by aqua fortis, but will by aqua regis; whereas silver is not soluble by the latter of these menstruums, but is by the former. And this relative affection, on whose account a body comes to be corrodible by a menstruum, seems to consist chiefly in three things, which all of them depend upon mechanical principles.

OF these qualifications, the first is, that the body to be corroded be furnished with pores of such a bigness and figure, that the corpuscles of the solvent may enter them, and yet not be much agitated in them, without giving brisk knocks or shakes to the solid parts, that make up the walls, if I may so call them, of the pores. And it is for want of this condition, that glass is penetrated in a multitude of places, but not dissipated or dissolved by the incident beams of light, which permeate its pores without any considerable resistance; and though the pores and commissures of a body were less minute, and capable of letting in some grosser corpuscles, yet if these were, for want of solidity or rigidness, too flexible, or were of a figure incongruous to that of the pores they should enter, the dissolution would not ensue; as it happens, when pure spirit of wine is in the cold put upon salt of tartar, or when aqua fortis is put upon powder or sulphur.

THE second qualification of a corrodible body is, that its consistent corpuscles be of such a bulk and solidity, as does not render them uncapable of being disjoined by the action of the insinuating corpuscles of the menstruum. Agreeable to this, and the former observation, is the practice of chemists, who oftentimes, when they would have a body to be wrought on by a menstruum, otherwise too weak for it in its crude estate, dispose it to receive the action of the menstruum by previously opening it, (as they speak) that is, by enlarging the pores, making a comminution of the corpuscles, or weakening their cohesion. And we see, that divers bodies are brought by fit preparations to be resoluble in liquors, that would not work on them before. Thus, as was lately noted, lime-stone by calcination becomes, in part, dissoluble in water; and some metalline calces will be so wrought on by solvents, as they would not be by the same agents, if the preparation of the metalline or other body had not given them a new disposition. Thus, though crude tartar, especially in lumps, is very slowly and difficultly dissoluble in cold water, yet when it is burnt, it may be presently dissolved in that liquor; and thus, though the filings and the calx of silver will not be at all dissolved by common water, or spirit of wine; yet if by the interposition of the saline particles of aqua fortis, the lunar corpuscles be so disjoined, and suffer such a comminution as they do in crystals of lune, the metal thus prepared and brought with its saline additament into a new texture, will easily enough dissolve, not only in water, but, as I have tried, in well rectified spirit of wine. And the like solubility I have found in the crystals of lead, made with spirit of verdigrease, or good distilled vinegar, and in those of copper made with aqua fortis.

THE last disposition to corrosibility consists in such a cohesion of the parts, whereof a body is made up, as is not too strict to be superable by the action of the menstruum. This condition, though of kin to the former, is yet somewhat differing from it, since a body may consist of parts, either bulky or solid, which yet may touch one another in such small portions of their surfaces, as to be much more easily dissociable than the minute or less solid parts of another body, whose contact is more full and close, and so their cohesion more strict.

BY what has been said, it may seem probable, that, as I formerly intimated, the corrosibility of bodies is but a mechanical relation, resulting from the mechanical affections and contexture of its parts, as they intercept pores of such sizes and figures, as make them congruous to those of the corpuscles of the menstruum, that are to pierce between them, and disjoin them.

THAT the quality, that disposes the body it affects to be dissolved by corrosive and other menstruums, does, as hath been declared, in many cases depend upon the mechanical texture

* See the beginning of the first Section.

ture and affections of the body in reference to the menstruum, that is to work upon it, may be made very probable, by what we are in due place to deliver concerning the pores of bodies and figures of corpuscles. But yet in compliance with the design of these notes, and agreeably to my custom on other subjects, I shall subjoin a few experiments on this occasion also.

EXPERIMENT I.

IF we put highly rectified spirit of wine upon crude sulphur, or even flowers of sulphur, the liquor will lie quietly thereon, especially in the cold, for many hours and days, without making any visible solution of it; and if such exactly dephlegmed spirit were put on very dry salt of tartar, the salt would lie in an undissolved powder at the bottom: and yet, if before any liquor be employed, the sulphur be gently melted, and then the alkali of tartar be by degrees put to it, and incorporated with it; as there will result a new texture discoverable to the eye by the new colour of the composition, so there will emerge a disposition, that was not before in either of the ingredients, to be dissolved by spirit of wine; insomuch, that though the mixture be kept till it be quite cold, or long after too, provided it be carefully secured from the access of the air, the spirit of wine being put to it, and shaken with it, will, if you have gone to work aright, acquire a yellow tincture in a minute of an hour; and perhaps in less than half a quarter of an hour a red one, being richly impregnated with sulphureous particles discoverable by the smell, taste, and divers operations.

EXPERIMENT II.

[IT is known to several chemists, that spirit of salt does not dissolve crude mercury in the cold; and I remember, I kept them for a considerable time in no contemptible heat without finding any solution following. But I suppose, many of them will be gratified by an experiment once mentioned to me by an ingenious German gentleman, namely, that if mercury be precipitated *per se*, that is, reduced to a red powder without additament, by the mere operation of the fire, the texture will be so changed, that the abovementioned spirit will readily dissolve it; for I found it upon trial to do so; nay, sometimes so readily, that I scarce remember, that I ever saw any menstruum so nimbly dissolve any metalline body whatsoever.

EXPERIMENT III.

THE former experiment is the more remarkable, because, that though oil of vitriol will in a good heat corrode quicksilver, (as we have already related in the first section,) yet I remember I kept a precipitate *per se* for divers hours in a considerable degree of heat, without finding it to be dissolved or corroded by the menstruum. And yet having, for trial's sake, put another parcel of the same mercurial powder into some aqua fortis, or spirit of nitre, there ensued a speedy dissolution even in the cold.

AND that this disposition to be dissolved by spirit of salt, that mercury acquires by being turned into precipitate *per se*, that is, by being calcined, is not merely the effect of the operation of the fire upon it, but of some change of texture produced by that operation; may be probably argued from hence, that, whereas spirit of salt is a very proper menstruum, as I have often tried, for the dissolving of iron or steel; yet, when that metal is reduced by the action of the fire (especially if a kind of vitrification, and an irroration with distilled vinegar have preceded) to crocus martis, though it be thereby brought to a very fine powder, yet I found not, that, as spirit of salt will readily, and with heat and noise, dissolve filings of mars, so it would have the same or any thing near such an operation upon the crocus; but rather, after a good while, it would leave in the bottom of the glass a considerable, if not the greatest part of it scarce, if at all, sensibly altered. And the menstruum seemed rather to have extracted a tincture; than made an ordinary solution; since the colour of it was a high yellow or reddish, whereas mars, dissolved in spirit of salt, affords a green solution. Whether by repeated operations with fresh menstruum further dissolutions might in time be made, I had not occasion to try, and it may suffice for our present purpose, that mars, by the operation of the fire, did evidently acquire, not, as mercury had done, a manifest facility, but on the contrary, a great indisposition to be dissolved by spirit of salt.

To second this experiment, we varied it, by employing, instead of spirit of salt, strong oil of vitriol, which being poured on a little crocus martis made *per se*, did not, as that menstruum is wont do upon filings of crude mars, readily and manifestly fall upon the powder with froth and noise, but, on the contrary, rested for divers hours calmly upon it, without so much as producing with it any sensible warmth.

EXPERIMENT IV.

IT agrees very well with our doctrine about the dependants of the corrosibility of bodies upon their texture, that from divers bodies, whilst they are in conjunction with others, there result masses, and those homogeneous as to sense, that are easily dissoluble in liquors, in which a great part of the matter, if it were separated from the rest, would not be at all disolved. Thus we see, that common vitriol is easily dissolved in mere water; whereas if it be skilfully calcined, it will yield sometimes near half its first weight of insipid colcothar, which not only is not soluble in water, but which neither aqua fortis nor aqua regis, though sometimes they will colour themselves upon it, are able, as far as I have tried, to make solutions of. We see likewise, that simple water will, being boiled for a competent time with hart's-horn, dissolve it and make a jelly of it: and yet, when we have

taken

taken harts-horn throughly calcined to whiteneſs, not only we found, that common water was no longer a fit ſolvent for it, but we obſerved, that when we put oil of vitriol itſelf upon it, a good part of the white powder was even by that corroſive menſtruum left undiſſolved.

EXPERIMENT V.

IN the fifteenth of the foregoing experiments I refer to a way of making the flower or powder of common ſulphur become eaſily diſſoluble, which otherwiſe it is far from being, in highly rectified ſpirit of wine. Wherefore I ſhall now add, that it is quickly performed by gently melting the ſulphur, and incorporating with it by degrees an equal or a greater weight of finely powdered ſalt of tartar, or of fixed nitre. For if the mixture be put warm into a mortar, that is ſo too; and as ſoon as it reduced to powder, but put into a glaſs, and well ſhaken with pure ſpirit of wine, it will, (as perhaps I may have elſewhere obſerved) in a few minutes acquire a yellow colour, which afterwards will grow deeper, and manifeſt itſelf by the ſmell and effects to be a real ſolution of ſulphur; and yet this ſolubleneſs in ſpirit of wine ſeems procured by the change of texture, reſulting from the commixtion of mere ſalt of tartar, which chemiſts know, to their trouble, to be itſelf a body almoſt as difficult as ſulphur to be diſſolved in phlegmleſs ſpirit of wine, unleſs the conſtitution of it be firſt altered by ſome convenient additament. Which laſt words I add, becauſe, though ſpirit of verdigreaſe be a menſtruum, that uſes to come off in diſtillation much more intirely than other acid menſtruums from the bodies it has diſſolved; yet it will ſerve well for an additament to open (as the chemiſts ſpeak) the body of the ſalt of tartar. For this purpoſe I employ ſpirit of verdigreaſe, not made firſt with ſpirit of vinegar, and then of wine, after the long and laborious way preſcribed by *Baſilius* and *Zwelfer*, but eaſily and expeditiouſly by a ſimple diſtillation of crude verdigreaſe of the better ſort. For when you have with this liquor (being, if there be need, once rectified) diſſolved as much good ſalt of tartar, as it will take up in the cold, if you draw off the menſtruum *ad ſiccitatem*, the remaining dry ſalt will be manifeſtly altered in texture even to the eye, and will readily enough, in high rectified ſpirit of wine, afford a ſolution, which I have found conſiderable in order to divers uſes, that concern not our preſent diſcourſe.

EXPERIMENT VI.

TO the conſideration of the followers of *Helmont* I ſhall recommend an experiment of that famous chemiſt's, which ſeems to ſute exceeding well with the doctrine propoſed in this ſection. For he tells us, that, if by a ſubtle menſtruum, to which he aſcribes that power, quickſilver be diveſted (or deprived) of its external ſulphur, as he terms it, all the reſt of the fluid metal, which he wittily enough ſtiles the kernel of mercury, will be no longer corroſible by it. So that upon this ſuppoſition, though common quickſilver be obſerved to be ſo obnoxious to aqua-fortis, that the ſame quantity of that liquor will diſſolve more of it, than of any other metal; yet, if by the deprivation of ſome portion of it the latent texture of the metal be altered, though not (that I remember) the viſible appearance of it; the body, that was before ſo eaſily diſſolved by aqua fortis, ceaſes to be at all diſſoluble by it.

EXPERIMENT VII.

AS for thoſe chemiſts of differing ſects, that agree in giving credit to the ſtrange things, that are affirmed of the operations of the alkaheſt, we may in favour of our doctrine urge them with what is delivered by *Helmont*, where he aſſerts, that all ſolid bodies, as ſtones, minerals, and metals themſelves, by having this liquor duly abſtracted or diſtilled off from them, may be changed into ſalt, equiponderant to the reſpective bodies whereon the menſtruum was put. So, that ſuppoſing the alkaheſt to be totally abſtracted, (as it ſeems very probable to be, ſince the weight of the body, whence it was drawn off, is not altered;) what other change, than of texture, can be reaſonably imagined to have been made in the tranſmuted bodies? and yet divers of them, as flints, rubies, ſapphires, gold, ſilver, &c. that were inſoluble before, ſome of them in any known menſtruums, and others in any but corroſive liquors, come to be capable of being diſſolved in common water.

EXPERIMENT VIII.

IT is a remarkable phænomenon, that ſuits very well with our opinion about the intereſt of mechanical principles in the corroſive power of menſtruums, and the corroſibility of bodies, that we produced by the following experiment: this we purpoſely made to ſhew, after how differing manners the ſame body may be diſſolved by two menſtruums, whoſe minute parts are very differingly conſtituted and agitated. For whereas it is known, that if we put large grains of ſea ſalt into common water, they will be diſſolved therein, calmly and ſilently, without any appearance of conflict; if we put ſuch grains of ſalt into good oil of vitriol, that liquor will fall furiouſly upon them, and produce for a good while a hiſſing noiſe with fumes, and a great ſtore of bubbles, as if a potent menſtruum were corroding ſome ſtubborn metal or mineral. And this experiment I the rather mention, becauſe it may be of uſe to us on divers other occaſions. For elſe it is not the only, though it be the remarkableſt, that I made to the ſame purpoſe.

EXPERIMENT IX.

FOR, whereas aqua fortis, or aqua regis, being poured upon filings of copper, will work upon them with much noiſe and ebullition, I have tried, that ſpirit of ſal armoniac or urine, being put upon the like filings, and left there without ſtopping the glaſs, will quickly begin to work on them, and quietly diſſolve them almoſt as water diſſolves ſugar. To which may be added, that even with oil of turpentine I have, though but ſlowly, diſſolved crude copper; and the experiment ſeemed to favour our conjecture the more, becauſe having tried it ſeveral times, it appeared, that common unrectified oil would perform the ſolution much quicker than that, which was purified and ſubtilized by rectification; which though more ſubtle and penetrant, yet was, it ſeems, on that account leſs fit to diſſolve the metal, than the groſſer oil, whoſe particles might be more ſolid, or more advantageouſly ſhaped, or on ſome other mechanical account better qualified for the purpoſe.

EXPERIMENT X.

TAKE good ſilver, and, having diſſolved it in aqua-fortis, precipitate it with a ſufficient quantity of good ſpirit of ſalt; then having waſhed the calx, which will be very white, with common water, and dried it well, melt it with a moderate fire into a fuſible maſs, which will be very much of the nature of what chemiſts call cornu lunæ, and which they make by precipitating diſſoved ſilver with a bare ſolution of common ſalt made in common water. And whereas both ſpirit of ſalt and ſilver, diſſolved in aqua fortis, will each of them apart readily diſſolve in ſimple water, our luna cornea not only will not do ſo, but is ſo indiſpoſed to diſſolution, that I remember I have kept it in digeſtion, ſome in aqua fortis, and ſome in aqua regia, and that for a good while, and in no very faint degree of heat, without being able to diſſolve it like a metal, the menſtruums having indeed tinged themſelves upon it, but left the compoſition undiſſolved at the bottom.

WITH this inſtance (of which ſort more might be afforded by chemical precipitations) I ſhall conclude what I deſigned to offer at preſent about the corroſibility of bodies, as it may be conſidered in a more general way. For as to the diſpoſition, that particular bodies have of being diſſolved in, or of reſiſting determinate liquors, it were much eaſier for me to enlarge upon that ſubject, than it was to provide the inſtances above recited. And theſe are not ſo few, but that it is hoped they may ſuffice to make it probable, that in the relation betwixt a ſolvent and a body it is to work upon, that, which depends upon the mechanical affections of one or both, is much to be conſidered, and has a great intereſt in the operations of one of the bodies upon the other.

OF THE MECHANICAL CAUSES OF CHEMICAL PRECIPITATION.

ADVERTISEMENT.

THOUGH I ſhall not deny, that, in grammatical ſtrictneſs, precipitation ſhould be reckoned among chemical operations, not qualities; yet I did not much ſcruple to inſert the following diſcourſe among the notes about particular qualities, becauſe many, if not moſt of the phænomena, mentioned in the enſuing eſſay, may be conſidered as depending, ſome of them, upon a power, that certain bodies have to cauſe precipitation, and ſome upon ſuch a diſpoſition, to be ſtruck down by others, as may, if men pleaſe, be called precipitability. And ſo theſe differing affections may, with (at leaſt) tolerable congruity, be referred to thoſe, that we have elſewhere ſtiled chemical qualities.

BUT though, I hope, I may, in theſe few lines, have ſaid enough concerning the name given to theſe attributes, yet, perhaps, it will be found in time, that the things themſelves may deſerve a larger diſcourſe, than my little leiſure would allow them. For, that is not a cauſeleſs intimation of the importance of the ſubject, wherewith I conclude the following tract; ſince, beſides that, many more inſtances might have been particularly referred to the heads treated of in the enſuing eſſay, there are improper kinds of precipitation, (beſides thoſe mentioned in the former part of the diſcourſe) to which one may not incongruouſly refer divers of the phænomena of nature, as well in the greater as in the leſſer world, whereof either no cauſes at all, or but improper ones, are wont to be given. And, beſides the ſimple ſpirits and ſalts uſually employed by chemiſts, there are many compounded and decompounded bodies, not only factitious, but natural, (and ſome ſuch, as one would ſcarce ſuſpect) that may, in congruous ſubjects, produce ſuch precipitations, as I ſpeak of. And the phænomena and conſequents of ſuch operations may, in divers caſes, prove conducive both to the diſcovery of phyſical cauſes, and the production of uſeful effects; though the particularizing of ſuch phænomena do rather belong to a hiſtory of precipitations, than to ſuch a diſcourſe as that, which follows, wherein I propoſed not ſo much to deliver the latent myſteries, as to inveſtigate the mechanical cauſes of precipitation.

Of the MECHANICAL CAUSES of CHEMICAL PRECIPITATION.

CHAP. I.

BY precipitation is here meant, ſuch an agitation or motion of a heterogeneous liquor, as in no long time makes the parts of it ſubſide, and that uſually in the form of a powder, or other conſiſtent body.

AS, on many occaſions, chemiſts call the ſubſtance, that is made to fall to the bottom of the liquor, the precipitate; ſo, for brevity-ſake, we ſhall call the body, that is put into the liquor to procure that ſubſiding, the precipitant; as alſo that, which is to be ſtruck down, the precipitable ſubſtance or matter, and the liquor, wherein

wherein it ſwims before the ſeparation, the menſtruum or ſolvent.

WHEN a haſty fall of a heterogeneous body is procured by a precipitant, the operation is called precipitation, in the proper or ſtrict ſenſe: but when the ſeparation is made without any ſuch addition, or the ſubſtance, ſeparated from the fluid part of the liquor, inſtead of ſubſiding emerges, then the word is uſed in a more comprehenſive, but leſs proper acceptation.

AS for the cauſes of precipitation, the very name itſelf, in its chemical ſenſe, having been ſcarce heard of in the peripatetick ſchools, it is not to be expected, that they ſhould have given us an account of the reaſons of the thing. And it is like, that thoſe few Ariſtotelians, that have, by their converſe with the laboratories or writings of chemiſts, taken notice of this operation, would, according to their cuſtom on ſuch occaſions, have recourſe for the explication of it to ſome ſecret ſympathy or antipathy between the bodies, whoſe action and re-action intervenes in this operation.

BUT if this be the way propoſed of accounting for it, I ſhall quickly have occaſion to ſay ſomewhat to it, in conſidering the ways propoſed by the chemiſts, who were wont to refer precipitation, either, as is moſt uſual, to a ſympathy betwixt the precipitating body and the menſtruum, which makes the ſolvent run to the embraces of the precipitant, and ſo let fall the particles of the body ſuſtained before; or (with others) to a great antipathy, or contrariety between the acid ſalt of the menſtruum and the fixed ſalt of the oil, or ſolution of calcined tartar, which is the moſt general and uſual precipitant they employ.

BUT I ſee not, how either of theſe cauſes will either reach to all the phænomena, that have been exhibited, or give a true account even of ſome of thoſe, to which it ſeems applicable. For firſt, in precipitations, wherein what they call a ſympathy between the liquors, is ſuppoſed to produce the effect, this admired ſympathy does not (in my apprehenſion) evince ſuch a myſterious occult quality, as is preſumed, but rather conſiſts in a greater congruity, as to bigneſs, ſhape, motion, and pores of the minute parts between the menſtruum and the precipitant, than between the ſame ſolvent and the body it kept before diſſolved. And though this ſympathy, rightly explained, may be allowed to have an intereſt in ſome ſuch precipitations, as let fall the diſſolved body in its priſtine nature and form, and only reduced into minute powder; yet I find not, that, in the generality of precipitations, this doctrine will hold; for in ſome, that we have made of gold and ſilver in proper menſtruums, after the ſubſiding matter had been well waſhed and dried, ſeveral precipitates of gold made, ſome with oil of tartar, which abounds with a fixed ſalt, and is the uſual precipitant, and ſome with an urinous ſpirit, which works by virtue of a ſalt highly fugitive, or volatile, I found the powder to exceed the weight of the gold and ſilver I had put to diſſolve; and the eye itſelf ſufficiently diſcovers ſuch precipitates not to be mere metalline powders, but compoſitions, whoſe conſiſting, not (as hath been by ſomebody ſuſpected) of the combined ſalts alone, but of the metalline parts alſo, may be ſtrongly concluded, not only from the ponderouſneſs of divers of them, in reference to their bulk, but alſo manifeſtly from the reduction of true malleable metals from ſeveral of them.

CHAP. II.

THE other chemical way of explicating precipitations may, in a right ſenſe, be made uſe of by a naturaliſt on ſome particular occaſions. But I think it much too narrow and defective, as it is in a general way propoſed, to be fit to be acquieſced in. For firſt it is plain, that it is not only ſalt of tartar, and other fixed alkalies, that precipitate moſt bodies, that are diſſolved in acid menſtruums; as in making of aurum fulminans, oil of tartar precipitates the gold out of aqua regis: but acid liquors themſelves do, on many occaſions, no leſs powerfully precipitate metals and other bodies out of one another. Thus, ſpirit of ſalt (as I have often tried) precipitates ſilver out of aqua fortis: the corroſive ſpirit of nitre copiouſly precipitates that white powder, whereof they make bezoardicum minerale: ſpirit or oil of ſulphur, made by a glaſs bell, precipitates corals, pearls, &c. diſſolved in ſpirit of vinegar, as is known to many chemiſts, who now uſe this oleum ſulphuris per campanam, to make the magiſtery of pearls, &c. for which vulgar chemiſts employ oleum tartari per deliquium.

I have ſometimes made a menſtruum, wherein, though there were both acid and alkalizate ſalts, yet I did not find, that either acid ſpirits, or oil of tartar, or even ſpirit of urine, would precipitate the diſſolved ſubſtances.

AND I have obſerved, both that ſalts of a contrary nature will precipitate bodies out of the ſame menſtruum, as not only ſalt of tartar, but ſea-ſalt, being diſſolved, will precipitate each other, and, each of them apart, will precipitate ſilver out of aqua fortis; and that even, where there is a confeſſed contrariety betwixt two liquors, it may be ſo ordered, that neither of them ſhall precipitate what is diſſolved by the other; of which I ſhall have occaſion to give, ere long, a remarkable inſtance.

BUT it will beſt appear, that the abovementioned theories of the Peripateticks and chemiſts are at leaſt inſufficient to ſolve the phænomena (many of which were probably not known to moſt of them, and perhaps not weighed by any,) if we proceed to obſerve the mechanical ways, by which precipitations may be accounted for; whereof I ſhall, at preſent, propoſe ſome number, and ſay ſomewhat of each of them apart; not that I think all of them to be equally important and comprehenſive, or that I abſolutely deny, that any one of them may be reduced to ſome of the other; but that I think, it may better elucidate the ſubject, to treat of them ſeverally, when I ſhall have premiſed, that I would not thence infer, that though, for the moſt part, nature does principally effect precipitations by one or other of theſe ways, yet, in divers caſes, ſhe may not employ

employ two, or more of them, about performing the operation.

To precipitate the corpuſcles of a metal out of a menſtruum, wherein, being once thoroughly diſſolved, it would, of itſelf, continue in that ſtate, the two general ways, that the nature of the thing ſeems to ſuggeſt to him, that conſiders it, are, either to add to the weight or bulk of the diſſolved corpuſcles, and thereby render them unfit to accompany the particles of the menſtruum in their motions; or to weaken the ſuſtaining power of the menſtruum, and thereby diſable it to keep the metalline particles ſwimming any longer: which falling of the deſerted parts of the metal, or other body, does oftentimes the more eaſily enſue, becauſe in many caſes, when the ſuſtaining particles of the menſtruum come to be too much weakened, that proves an occaſion to the metalline corpuſcles, diſturbed in the former motion, that kept them ſeparate, to make occurſions and coalitions among themſelves, and their fall becomes the effect, though not equally ſo, of both ways of precipitation; as on the other ſide, there are ſeveral occaſions on which the ſame precipitant, that brings the ſwimming particles of the metal to ſtick to one another, does likewiſe, by mortifying or diſabling the ſaline ſpirits, or other parts of the ſolvent, weaken the ſuſtaining power of that liquor.

CHAP. III.

TO deſcend now to the diſtinct conſiderations about theſe two ways: the firſt of the moſt general cauſes of precipitation is ſuch a coheſion procured by the precipitant in the ſolution, as makes the compounded corpuſcles, or at leaſt the aſſociated particles of the diſſolved body, too heavy to be ſuſtained, or too bulky to be kept in a ſtate of fluidity by the liquor.

THAT in many precipitations there is made a coalition betwixt the ſmall parts of the precipitant, and thoſe of the diſſolved metal, or other body and frequently alſo, with the ſaline ſpirits of the menſtruum, may be eaſily ſhewn, by the weight of the precipitate, which, though carefully waſhed and dried, often ſurpaſſes, and ſometimes very conſiderably, that of your crude metal, that was diſſolved; of which we lately gave an inſtance in aurum fulminans, and precipitated ſilver; and we may yet give a more conſpicuous one, in that, which chemiſts call luna cornea: for, if having diſſolved ſilver in good aqua fortis, you precipitate it with the ſolution of ſea ſalt in fair water, and from the very white precipitate waſh the looſe adhering ſalts, the remaining powder, being dried and ſlowly melted, will look much leſs like a metalline body, than like a piece of horn, whence alſo it takes its name; ſo conſiderable is the additament of the ſaline to the metalline particles.

AND that part of ſuch additaments is retained, may not only be found by weighing, but, in divers caſes, may be argued from what is obvious to the eye: as if you diſſolve mercury in aqua fortis, and into the philtrated ſolution drop ſpirit of ſalt, or ſalt water, or an urinous ſpirit, as of ſal armoniac, you will have a very white precipitate; but if, inſtead of any of theſe, you drop in deliquated ſalt of tartar, your precipitate will be of a brick, or orange colour. From which experiment, and ſome others, I would gladly take a riſe to perſuade chemiſts and phyſicians, that it is not ſo indifferent, as thoſe ſeem to think, who look on precipitation but as a kind of comminution, by what means the precipitation is performed. For, by reaſon of the ſtrict adheſion of divers ſaline particles of the precipitant and the ſolvent, the precipitated body, notwithſtanding all the wonted ablutions, may have its qualities much diverſified by thoſe of the particles of the liquors, when theſe are fitted to ſtick very faſt to it. Which laſt words I add, becauſe, though that ſometimes happens, yet it does not always, there being a greater difference, than every body takes notice of, between precipitations; as you will be induced to think, if you precipitate the ſolution of ſilver with copper, with ſpirit of ſal armoniac, with ſalt water, with oil of tartar, with quickſilver, with crude tartar, and with zink. And in the lately propoſed example, you will think it probable, that it is not all one, whether to diſſolved mercury or ſilver, you employ the ſubtile diſtilled ſpirits of ſalt, or the groſs body, whether in a dry form, or barely diſſolved in common water. And thus much of the conduciveneſs of weight to the ſtriking down the corpuſcles of a diſſolved body.

THAT alſo the bulk of a body may very much contribute to make it ſink or ſwim in a liquor, appears by obvious inſtances. Thus ſalt or ſugar, being put into water, either in lumps or even in powder, that is but groſs, falls at firſt to the bottom, and lies there, notwithſtanding the air, that may be intercepted between its parts, or externally adhere to it. But when, by the inſinuating action of the water, it it is diſſolved into minute particles, theſe are carried up and down with thoſe of the liquor, and ſubſide not. The like happens, when a piece of ſilver is caſt into aqua fortis, and in many other caſes.

On the other ſide I have ſeveral times obſerved, that ſome bodies, that had long ſwam in a menſtruum, whilſt their minute parts were kept from convening in it, did afterwards, by the coalition of many of thoſe particles into bodies of a viſible bulk, coagulate and ſubſide, (though ſometimes, to hinder the evaporation of the menſtruum, the veſſels were kept ſtopped.) Of this I elſewhere mention divers examples; and particularly in urinous and animal ſpirits, well dephlegmed, I have found, that after all had, for a conſiderable time, continued in the form of a perfect liquor, and as to ſenſe homogeneous, ſtore of ſolid corpuſcles, convening together, ſettled at the bottom of the glaſſes in the form of ſaline cryſtals. Having alſo long kept a very red ſolution of ſulphur firſt unlocked, (as they ſpeak) made with highly rectified ſpirit of urine, I obſerved, that at length the ſulphureous particles, making little concretions between themſelves, totally ſubſided, and left the liquor almoſt devoid of tincture. By which you may ſee, that it was not impertinent to mention (as I lately did) among the ſubordinate cauſes of precipitation, the aſſociating of the particles of a diſſolved body with one another. Of which I elſewhere give a notable example in the ſhining powder, that I obtained from gold diſſolved in a peculiar menſtruum, without any precipitant, by the coalition of the metalline particles, to which a tract of time gave opportunity to meet and adhere in a convenient manner.

If in what the chemiſts call *præcipitate per ſe*, the mercury be indeed brought to loſe its fluidity, and become a powder without being compounded with any additional body, (which doubt I elſewhere ſtate and diſcourſe of,) it will afford us a notable inſtance to prove, that the coalitions of particles into cluſters of the ſelf-ſame matter will render them unfit for the motion requiſite to fluidity. For, in this odd precipitation by fire, wherein the ſame menſtruum is both the liquor and the precipitate, being not all made at once, the corpuſcles, that firſt diſcloſe themſelves by their redneſs, are rejected by thoſe of the mercury, that yet remains fluid, as unable to accompany them in the motions, that belong to mercury as ſuch.

CHAP. IV.

BEFORE I diſmiſs that way of precipitating, that depends upon the unwieldineſs, which the precipitant gives to the body it is to ſtrike down, it may not be impertinent, eſpecially in reference to the foregoing part of this paper, to conſider, that perhaps, in divers caſes, the corpuſcles of a diſſolved body may be made unfit to be any longer ſuſtained in the menſtruum, though the precipitant adds very little to their bulk, or at leaſt, much more to their ſpecific weight than to it. For I have elſewhere ſhewn, that in divers ſolutions made of bodies by acid menſtruums, there are either generated or extricated many ſmall aerial particles; and it will be eaſily granted, that theſe may be ſmall enough to be detained in the pores of the liquor, and be inviſible there, if we conſider, what a multitude of aerial and formerly imperceptible bubbles is afforded by common water in our pneumatical receivers, when the incumbent air, that before preſſed the liquor, is pumped out. And if the corpuſcles of the diſſolved body have any little cavities or pores fit to lodge aerial particles, or have aſperous ſurfaces, between whoſe prominent parts the generated air may conveniently lie; in ſuch caſes, I ſay, theſe inviſible bubbles may be looked upon, as making with the ſolid corpuſcles they adhered to, little aggregates much lighter in ſpecie than the corpuſcles themſelves would be; and conſequently if the precipitant conſiſt of particles of ſuch a ſize and ſhape as are fit to expel theſe little bubbles, and lodge themſelves in the cavities poſſeſſed by them before, there will be produced new aggregates compoſed of the corpuſcles of the diſſolved body and the particles of the precipitant; which aggregates, though they do take up very little, or perhaps not at all more room (taking that word in a popular ſenſe) than thoſe, whereof the aerial bubbles made a part, will yet be ſpecifically heavier than the former aggregates were, and may thereby overcome the ſuſtaining power of the menſtruum.

One thing more may be fit to be taken notice of before we paſs on further, namely, that it is upon the ſcore of the ſpecifick gravity of a body, and not barely upon the action of the precipitant, that an aggregate, or a convention, of particles, does rather fall to the bottom than riſe to the top. For, though the agents, that procured the coalition, make the cluſter of particles become of a bulk too unwieldy to continue in the liquor as parts of it; yet if each of them be lighter in ſpecie than an equal bulk of the menſtruum, or if they ſo convene, as to intercept a ſufficient number of little bubbles or aerial corpuſcles between them, and ſo become lighter than as much of the menſtruum, as they take up the room of, they will not be precipitated but emerge; as may be ſeen in the preparation of thoſe magiſteries of vegetables, I elſewhere mention; where ſome deeply coloured plants being made to tinge plentifully the lixivium they are boiled in, are afterwards by the addition of alum made to curdle, as it were, into coloured concretions, which being (totally or in part) too big to ſwim, as they did before they convened, and too light in compariſon of the menſtruum to ſubſide, emerge to the top, and float there. An eaſier and neater example to the ſame purpoſe, I remember I ſhewed by diſſolving camphire in highly rectified ſpirit of wine, till the ſolution was very ſtrong. For though the camphire, when put in lumps into the ſpirit, ſunk to the bottom of it; yet, when good ſtore of water, (a liquor ſomewhat heavier in ſpecie than camphire,) was poured upon the ſolution, the camphire quickly concreted and returned to its own nature, and within a while emerged to the top of the mingled liquors and floated there. Theſe particulars I was willing to mention here, that I might give an inſtance or two of thoſe precipitations, that I formerly ſpake of as improperly ſo called. And here I muſt not decline taking notice of a phænomenon, that ſometimes occurs in precipitations, and at firſt ſight may ſeem contrary to our doctrine about them. For now and then it happens, that after ſome drops of the precipitant have begun a precipitation at the top or bottom of the ſolvent, one ſhakes the veſſel, that the precipitant may be the ſooner diffuſed through the other liquor, but then they are quickly ſurprized to find, that inſtead of haſtening the compleat precipitation, the matter already precipitated diſappears, and the ſolvent returns to be clear, or, as to ſenſe, as uniform, as it was before the precipitant was put into it. But this phænomenon does not at all croſs our theory. For, when this happens, though that part of the ſolvent, to which the precipitant reaches, is diſabled for reaſons mentioned in this diſcourſe to ſupport the diſſolved body, yet this quantity of the precipitant is but ſmall in proportion to the whole bulk of the ſolvent. And

therefore

therefore, when the agitation of the veſſel diſperſes the cluſters of looſly concreted particles through the whole liquor, (which is ſeldom ſo exactly proportioned to the body it was to work on, as to be but juſt ſtrong enough to diſſolve it) that greater part of the liquor, to which before the ſhaking of the veſſel the precipitant did not reach, may well be looked upon as a freſh menſtruum, which is able to mortify or overpower the ſmall quantity of the precipitant, that is mingled with it, and ſo to deſtroy its late operation on the body diſſolved, by which means the ſolution returns, as to ſenſe, to its former ſtate. Which may be illuſtrated by a not unpleaſant experiment, I remember, I have long ſince made by precipitating a brick-coloured powder out of a ſtrong ſolution of ſublimate made in fair water. For this ſubſiding matter, being laid to dry in the philter, by which it was ſeparated from the water, would retain a deep, but ſomewhat dirty colour; and if then, putting it into the bottom of a wine glaſs, I poured upon it, either clear oil of vitriol, or ſome other ſtrong acid menſtruum, the alcalizate particles being diſabled and ſwallowed up by ſome of the acid ones of the menſtruum, the other acid ones would ſo readily diſſolve the reſidue of the powder, that in a trice the colour of it would diſappear, and the whole mixture be reduced into a clear liquor, without any ſediment at the bottom.

THUS much may ſuffice at preſent about the firſt general way of precipitating bodies out of the liquors they ſwam in.

CHAP. V.

THE other of the two principal ways, by which precipitations may be effected, is the diſabling the ſolvent to ſuſtain the diſſolved body.

THERE may be many inſtances, wherein this ſecond way of effecting precipitations may be aſſociated by nature with the firſt way formerly propoſed; but notwithſtanding the caſes, wherein nature may (as I formerly noted) employ both the ways therein, yet in moſt caſes they ſufficiently differ, in regard that in the former way the ſubſiding of the diſſolved body is chiefly, if not only, cauſed by the additional weight, as well as action of the external precipitant; whereas, in moſt of the inſtances of the later way, the effect is produced either without ſalt of tartar, or any ſuch precipitant, or by ſome other quality of the precipitant more than by its weight, or at leaſt beſides the weight it adds: though I forget not, that I lately gave an example of a ſhining powder of gold, that fell to the bottom of a menſtruum without the help of an external precipitant: but that was done ſo ſlowly, that it may be diſputed, whether it were a true precipitation; and I alledged it not as ſuch, but to ſhew, that the increaſed bulk of particles may make them unfit to ſwim in menſtruums, wherein they ſwam, whilſt they were more minute. And the like anſwer may be accommodated to the precipitate *per ſe* newly mentioned.

THIS premiſed, I proceed now to obſerve, that the general way, I laſt propoſed, contains in it ſeveral ſubordinate ways, that are more particular; of which I ſhall now mention the chief, that occur to me, and, though but briefly, illuſtrate each of them by examples. And firſt a precipitation may be made, if the ſaline or other diſſolving particles of the menſtruum are mortified or rendered unfit for their former function, by particles of a precipitant, that are of a contrary nature.

THUS gold and ſome other minerals, being diſſolved in aqua regis, will be precipitated with ſpirit of urine and other ſuch liquors abounding with volatile and ſalino-ſulphureous corpuſcles, upon whoſe account it is, that they act; whence theſe ſalts themſelves, though caſt into a menſtruum in a dry form, will ſerve to make the like precipitations. And I the rather on this occaſion mention urinous ſpirits than ſalt of tartar, becauſe thoſe volatile particles add much leſs of weight to the little concretions, which compoſe the precipitated powder.

UPON inſtances of this kind, many of the modern chemiſts have built that antipathy betwixt the ſalts of the ſolvent and thoſe of the menſtruum, to which they aſcribe almoſt all precipitations. But againſt this I have repreſented ſomething already, and ſhall partly now, and partly in the ſequel of this diſcourſe, add ſome farther reaſons of my not being ſatisfied with this doctrine. For beſides, that it is inſufficient to reach many of the phænomena of precipitation (as will ere long be ſhown) and beſides that it is not eaſy to make out, that there is any real antipathy betwixt inanimate bodies; I conſider, firſt, that ſome of thoſe menſtruums, to which this antipathy is attributed, do, after a ſhort commotion (whereby they are diſpoſed to make convenient occurſions and coalitions) amicably unite into concretions participating of both the ingredients; as I have ſomewhere ſhewn by an example purpoſely deviſed to make this out; to do which I dropped a clear ſolution of fixed nitre, inſtead of the uſual one of common ſalt, upon a ſolution of ſilver, in aqua fortis: for the ſaline particles of the ſolvent and thoſe of the precipitant, will, as I have elſewhere recited, for the moſt part friendly unite into ſuch cryſtals of nitre for the main, as they were obtained from: and though this notion of the chemiſts, if well explained, be applicable to far more inſtances then the propoſers of it ſeemed to have thought on, and may be made good uſe of in practice; yet I take it to be ſuch as is not true univerſally, and, where it is true, ought to be explicated according to mechanical principles. For if the particles of the menſtruum and thoſe of the precipitant be ſo framed, that upon the action of the one upon the other, there will be produced corpuſcles too big and unweildy to continue in the ſtate of fluidity, there will enſue a precipitation: but if the conſtitution of the corpuſcles of the precipitating and of the diſſolved body be ſuch, that the precipitant alſo itſelf is fit to be a menſtruum to diſſolve that body in; then, though there

there be an union of the ſalt of the precipitant and the metal (or other ſolutum) and perhaps of the ſolvent too, yet a precipitation will not neceſſarily follow, though the ſaline particles of the two liquors ſeemed, by the heat and ebullition excited between them upon their meeting, to exerciſe a great and mutual antipathy. To ſatisfy ſome ingenious men about this particular, I diſſolved zink or ſpeltar in a certain urinous ſpirit; (for there are more than one, that may ſerve the turn;) and then put to it a convenient quantity of a proper acid ſpirit; but though there would be a manifeſt conflict thereby occaſioned betwixt the two liquors; yet the ſpeltar remained diſſolved in the mixture. And I remember, that for the ſame purpoſe I deviſed another experiment, which is ſomewhat more eaſy and more clear. I diſſolved copper calcined *per ſe*, or even crude, in ſtrong ſpirit of ſalt; (for unleſs it be ſuch, it will not be ſo proper,) and having put to it by degrees a good quantity of ſpirit of ſal-armoniac, or fermented urine, though there would be a great commotion with hiſſing and bubbles produced, the copper would not be precipitated, becauſe this urinous ſpirit will, as well as the ſalt, (and much more readily) diſſolve the ſame metal, and it would be kept diſſolved notwithſtanding their operation on one another; the intervening of which, and their action upon the metalline corpuſcles, may be gathered from hence, that the green ſolution, made with ſpirit of ſalt alone, will, by the ſupervening urinous ſpirits, be changed either into a blueiſh green, or, if the proporportion of this ſpirit be very great, into a rich blue almoſt like ultramarine. And from theſe two experiments we may probably argue, that when the precipitation of a metal, &c. enſues, it is not barely on the account of the ſuppoſed antipathy betwixt the ſalts, but becauſe the cauſes of that ſeeming antipathy do likewiſe, upon a mechanical account, diſpoſe the corpuſcles of the confounded liquors ſo to cohere, as to be too unweildy for the fluid part.

CHAP. VI.

ANOTHER way, whereby the diſſolving particles of a menſtruum may be rendered unfit to ſuſtain the diſſolved body, is to preſent them another, that they can more eaſily work on.

A notable experiment of this you have in the common practice of refiners, who, to recover the ſilver out of lace, and other ſuch mixtures wherein it abounds, uſe to diſſolve it in aqua fortis, and then in the ſolution leave copper plates for a whole night (or many hours.) But if you have a mind to ſee the experiment, without waiting ſo long, you may employ the way, whereby I have often quickly diſpatched it. As ſoon then as I have diſſolved a convenient quantity, which needs not be a great one, of ſilver in cleanſed aqua fortis, I add twenty or twenty-five times as much of either diſtilled water, or rain water; (for though common water will ſometimes do well, yet it ſeldom does ſo well;) and then into the clear ſolution, I hang by a ſtring a clean piece of copper, which will be preſently covered with little ſhining plates almoſt like ſcales of fiſh, which one may eaſily ſhake off, and make room for more. And this may illuſtrate what we formerly mentioned about the ſubſiding of metalline corpuſcles, when they convene in liquors, wherein, whilſt they were diſperſed in very minute parts, they ſwam freely. For, in this operation, the little ſcales of ſilver ſeemed to be purely metalline, and there is no ſaline precipitant, as ſalt of tartar or of urine, employed to make them ſubſide. Upon the ſame ground, gold and ſilver, diſſolved in their proper menſtruums, may be precipitated with running mercury; and if a ſolution of blue vitriol (ſuch as the Roman, Eaſt-Indian, or other of the like colours) be made in water, a clean plate of ſteel or iron being immerſed in it, will preſently be over-laid with a very thin caſe of copper, which, after a while, will grow thicker; but does not adhere to the iron ſo looſely as to be ſhaken off, as the precipitated ſilver newly mentioned may be from the copper-plates whereto it adheres. And that, in theſe operations, the ſaline particles may really quit the diſſolved body, and work upon the precipitant, may appear by the lately mentioned practice of refiners, where the aqua fortis, that forſakes the particles of the ſilver, falls a working upon the copper-plates employed about the precipitation, and diſſolves ſo much of them, as to acquire the greeniſh blue colour of a good ſolution of that metal. And the copper we can eaſily again, without ſalts, obtain by precipitation out of that liquor with iron, and that too, remaining diſſolved in its place, we can precipitate with the taſteleſs powder of another mineral.

BESIDES theſe two ways of weakening the menſtruum, namely, by mortifying its ſaline particles, or ſeducing them to work on other bodies, and to forſake thoſe they firſt diſſolved, there are ſome other ways of weakening the menſtruum.

A third way of effecting this, is by leſſening or diſturbing the agitation of the ſolvent. And indeed, ſince we find by experience, that ſome liquors, when they are heated, will either diſſolve ſome bodies they would not diſſolve at all when they were cold, or diſſolve them more powerfully or copiouſly when hot than cold; it is not unreaſonable to ſuppoſe, that what conſiderably leſſens that agitation of the parts of the menſtruum, that is neceſſary to the keeping the diſſolved body in the ſtate of fluidity, ſhould occaſion the falling of it again to the bottom. In ſlow operations, I could give divers examples of the precipitating power of cold; there being divers ſolutions, and particularly that of ambergreaſe, that I had kept fluid all the ſummer, which in the winter would ſubſide. And the like may be ſometimes obſerved in far leſs time in the ſolutions of brimſtone, made in certain oleaginous menſtruums; and I have now and then had ſome ſolutions, and particularly one of benzoin, made in ſpirit of wine, that would ſurprize me with the turbidneſs (which begins the ſtate of precipitation) it would acquire upon a ſudden change of of the weather towards cold, though it were not in the winter ſeaſon.

ANOTHER way of weakening the menſtruum, and ſo cauſing the precipitation of a body diſſolved in it, is the diluting or leſſening the tenacity of it, whether that tenacity proceed from viſcoſity, or the competent number and conſtipation of the parts.

OF this we have an inſtance in the magiſteries (as many chemiſts are pleaſed to call them) of jalap, benzoin, and of divers others reſinous and gummous bodies diſſolved in ſpirit of wine. For, by the affuſion of common water, the menſtruum, being too much diluted, is not able to keep thoſe particles in the ſtate of fluidity, but muſt ſuffer them to ſubſide, (as they uſually do, in the form of white powder) or, (as it may happen ſometimes,) make ſome parts emerge. Examples alſo of this kind are afforded us by the common preparations of mercurius vitæ. For, though in oil of antimony, made by the rectification of the butter, the ſaline particles are ſo numerous, and keep ſo cloſe to one another, that they are able to ſuſtain the antimonial corpuſcles they carried over with them in diſtillation, and keep them together with themſelves in the form of a liquor; yet, when by the copious affuſion of the water, thoſe ſuſtaining particles are ſeparated, and removed to a diſtance from each other, the antimonial corpuſcles, and the mercurial, (if any ſuch there were) being of a ponderous nature, will eaſily ſubſide into that emetic powder, which (when well waſhed) the chemiſts, flatteringly enough, call mercurius vitæ.

BUT here I muſt interpoſe an advertiſement, which will help to ſhew us, how much precipitations depend upon the mechanical contextures of bodies. For, though not only in the newly recited examples, but in divers others, the affuſion of water, by diluting the ſalts, and weakening the menſtruum, makes the metal, or other diſſolved body, fall precipitately to the bottom; yet if the ſaline particles of the ſolvent, and thoſe of the body, be fitted for ſo ſtrict an union, that the corpuſcles reſulting from their coalitions, will not ſo eaſily be ſeparated by the particles of water, as ſuffer themſelves to be carried up and down with them, whether becauſe of the minuteneſs of theſe compounded corpuſcles, or becauſe of ſome congruity betwixt them and thoſe of the water; they will not be precipitated out of the weakened ſolution, but ſtill continue a part of it; as I have tried partly with ſome ſolution of ſilver and gold, made in acid menſtruums, but much more ſatisfactorily in ſolutions of copper, made in the urinous ſpirit of ſal armoniac. For, though that blue ſolution were diluted with many thouſand times as much diſtilled water, as the diſſolved metal weighed, yet its ſwimming corpuſcles did, by their colour, manifeſtly appear to be diſperſed through the whole liquor.

CHAP. VII.

BUT, to proſecute our former diſcourſe, which we broke off after the mention of mercurius vitæ, it will now be ſeaſonable to add, that we have made divers other precipitations, by the bare affuſion of water, out of ſolutions, and ſometimes out of diſtilled liquors; which, for brevity-ſake, I here omit, that I may haſten to the laſt way I ſhall now ſtay to mention.

ANOTHER way then, whereby precipitations of bodies may be produced by debilitating the menſtruum they ſwim in, is by leſſening the proportion of the ſolvent to the ſolutum, without any evaporation of the liquor. Theſe laſt words I add, becauſe that, when there is an obſtruction, or any other expulſion of the menſtruum by heat, if it be total, it is called exſiccation, as when dry ſalt of tartar is obtained from the filtrated lixivium of the calcined tartar; and though the evaporation be not total, yet the effects of it are not wont to be reckoned amongſt precipitations. And although the way I am about to propoſe, if it be attentively conſidered, has much affinity with the foregoing, and the phænomena may, perhaps, in ſome ſort, be reduced to them; yet the inſtances, that I ſhall name, having not, that I know, been thought of by others, and being ſuch as every one would not deduce from what I have been mentioning, I ſhall add a word of the inducements I had to make the trials, as well as of the ſucceſs of them.

CONSIDERING then, that water will not diſſolve ſalts indefinitely, but when it has received its due proportion, it will then diſſolve no more; but, if they be put into it, let them fall to the ground, and continue undiſſolved; and that if, when water is ſatiated, any of the liquor be evaporated, or otherwiſe waſted, it will, in proportion, let fall the ſalt it had already taken up; I conclude, that, if I could mingle with water any liquor, with which its particles would more readily aſſociate than with thoſe of ſalt, the depriving the ſolution of ſo many of its aqueous particles, would be equivalent to the evaporation of as much water, or thereabouts, as they, by being united, could compoſe. Wherefore, making a lixivium of diſtilled water, or clean rain water, and of ſalt of tartar, ſo ſtrong, that if a grain more were caſt in it, it would lie undiſſolved at the bottom; I put a quantity of this fiery lixivium into a ſlender cylindrical veſſel, till it had therein reached ſuch a height, as I thought fit; then taking as much as I thought ſufficient of ſtrong ſpirit of wine, that would burn every drop away, that ſo it might have no phlegm nor water of its own, I poured this upon the ſaline ſolution; and ſhaking the liquors pretty well together, to bring them to mix as well as I could, I laid the tube in a quiet place, and afterwards found, as I expected, that there was a pretty quantity of white ſalt of tartar fallen to the bottom of the veſſel, which ſalt had been merely forſaken by the aqueous particles, that ſuſtained it before, but forſook it to paſs into the ſpirit of wine, wherewith they were more diſpoſed to aſſociate themſelves; which I concluded, becauſe having, before I poured on this laſt named liquor, made a mark on the glaſs, to ſhew how far the lixivium reached, I found,

found, (what I looked for) that, after the precipitation, the lixivium, that remained yet ſtrong enough to continue unmixed with the incumbent ſpirit, had its ſurface, not where the mark ſhewed it had been before, but a conſiderable diſtance beneath it, the ſpirit of wine having gained in extent what it loſt in ſtrength, by receiving ſo many aqueous particles into it. I choſe to make this trial, rather with a lixivium of ſalt of tartar, than with oil of tartar per deliquium, becauſe, in this laſt named liquor, the aqueous and ſaline particles are more cloſely combined, and therefore more difficult to be ſeparated, than I thought they would be in a lixivium haſtily made, though very ſtrong. And though, by much agitation, I have ſometimes obtained ſome ſalt of tartar from the above-mentioned oil; yet the experiment ſucceeded nothing near ſo well with that liquor, as with a lixivium.

I made alſo the like trial with exceedingly dephlegmed ſpirit of wine, and as ſtrong a brine as I could make of common ſalt diſſolved, without heat, in common water; and I thereby obtained no deſpicable proportion of finely figured ſalt, that was let fall to the bottom. But this experiment, to be ſucceſsful, requires greater care in him, that makes it, than the former needs.

To confirm, and ſomewhat to vary this way of precipitation, I ſhall add, that having made a clear ſolution of choice gum Arabic in common water, and poured upon it a little high rectified ſpirit of wine; on this occaſion there was alſo made, and that in a trice, a copious precipitation of a light and purely white ſubſtance, not unpleaſant to behold. And, for further confirmation, I diſſolved a full proportion of myrrh in fair water, and into the filtrated ſolution, which was tranſparent, but of a high brown colour, I dropped a large proportion (which circumſtance is not to be omitted) of carefully dephlegmed ſpirit of wine, which, according to expectation, made a copious precipitate of the gum. And theſe inſtances I the rather ſet down in this place, becauſe they ſeem to ſhew, that ſimple water is a real menſtruum, which may have its diſſolving and ſuſtaining virtue weakened by the acceſſion of liquors, that are not doubted to be much ſtronger than it.

By ſpecifying the hitherto mentioned ways, whereby precipitations may be mechanically performed and accounted for, I would by no means be thought to deny, that there may be ſome omitted here, which either others, that ſhall conſider the matter with more attention, or I myſelf, if I ſhall have leiſure to do it, may think on. For I propoſe theſe but as the chief, that occur to my preſent thoughts; and I forbear to add more inſtances to exemplify them, becauſe I would not injure ſome of my other papers, that have a greater right to thoſe inſtances. Only this I ſhall note in general, that the doctrine and hiſtory of precipitations, if well delivered, will be a thing of more extent and moment, than ſeems hitherto to have been imagined; ſince not only ſeveral of the changes in the blood, and other liquors and juices of the human body, may thereby be the better underſtood; and they prevented, or their ill conſequences remedied; but in the practical part of mineralogy, divers uſeful things may probably be performed by the aſſiſtance of ſuch a doctrine and hiſtory. To keep which conjecture from ſeeming extravagant, I ſhall only here intimate, that it is not alone in bodies, that are naturally or permanently liquid, but in thoſe ſolid and ponderous bodies, that are for a ſhort time made ſo by the violence of the fire, that many of the things ſuggeſted by this doctrine may have place. For whilſt divers of thoſe bodies are in fuſion, they may be treated as liquors; and metals, and perhaps other heterogeneous bodies, may be obtained from them by fit, though dry precipitants, as in ſome other writings I partly did, and may elſewhere yet further declare,

EXPERIMENTS AND NOTES, ABOUT THE MECHANICAL PRODUCTION OF MAGNETISM.

ADVERTISEMENT concerning the following NOTES about OCCULT QUALITIES.

THE following paper (about magnetism and electricity) would appear with less disadvantage, if the author's willingness and promise, that this tome should be furnished with notes about some occult qualities, as well as about divers sorts of those, that are presumed to be manifest, did not prevail with him to let the ensuing notes appear, without those about the pores of bodies and figures of corpuscles, that should have preceded them, and some others, that should have accompanied them. But the author chose rather to venture these papers abroad in the condition, such as it is, they now appear in, than make those already printed about manifest quantities stay longer for accessions, which some troublesome accidents will not suffer him to hasten to the press; and without which, he now fears this tome may swell to a more than competent bulk.

EXPERIMENTS and NOTES about the Mechanical Production of MAGNETICAL QUALITIES.

THOUGH the virtues of the loadstone be none of the least famous of occult qualities, and are perhaps the most justly admired; yet I shall venture to offer something to make it probable, that some, even of these, may be introduced into bodies by the production of mechanical changes in them.

To make way for what I am to deliver to this purpose, it will be expedient to remove that general and settled prejudice, that has kept men from so much as thinking of any mechanical account of magnetisms, which is a belief, that these qualities do immediately flow from the substantial form of the loadstone, whose abstruse nature is disproportionate to our understandings.

EXPERIMENT I.

BUT for my part, I confess, I see no necessity of admitting this supposition; for I see, that a piece of steel fitly shaped and well excited will, like a loadstone, have its determinate poles, and with them point at the north and south; it will draw other pieces of iron and steel to it, and, which is more, communicate to them the same kind, though not degree, of attractive and directive virtue it had itself, and will possess these faculties, not as light and transient impressions, but as such settled and durable powers, that it may retain them for many years, if the loadstone, to which it has been duly applied, were vigorous enough; of which sort I remember I have seen one (and made

made ſome trials with it) that yielded an income to the owner, who received money from navigators and others, for ſuffering them to touch their needles, ſwords, knives, &c. at his excellent magnet. Now, in a piece of ſteel or iron thus excited, it is plain, that the magnetic operations may be regularly performed for whole years by a body, to which the form of a loadſtone does not belong, ſince, as it had its own form before, ſo it retains the ſame ſtill, continuing as malleable, fuſible, &c. as an ordinary piece of the ſame metal unexcited: ſo that, if there be introduced a fit diſpoſition into the internal parts of the metal by the action of the loadſtone, the metal, continuing of the ſame ſpecies it was before, will need nothing, ſave the continuance of that acquired diſpoſition to be capable of performing magnetical operations; and if this diſpoſition or internal conſtitution of the excited iron be deſtroyed, though the form of the metal be not at all injured, yet the former power of attraction ſhall be aboliſhed, as appears,

EXPERIMENT II.

WHEN an excited iron is made red hot in the fire, and ſuffered to cool again.

AND here give me leave to take notice of what I have elſewhere related to another purpoſe, namely,

EXPERIMENT III.

THAT a loadſtone may, as I have more than once tried, be eaſily deprived by ignition of its power of ſenſibly attracting martial bodies, and yet be ſcarce, if at all, viſibly changed, but continue a true loadſtone in other capacities, which, according to the vulgar philoſophy ought to depend upon its ſubſtantial form, and the loadſtone thus ſpoiled may, notwithſtanding this form, have its poles altered at pleaſure like a piece of iron; as I have elſewhere particularly declared.

AND I will confirm what I have been ſaying with an experiment, that you do not perhaps expect; namely, that though it be generally taken for granted (without being contradicted, that I know of, by any man) that, in a ſound loadſtone, that has never been injured by the fire, not only the attractive power, but the particular virtue, that it has to point conſtantly, when left to itſelf, with one of its determinate extremes to one determinate pole, flows immediately from the ſubſtantial, or at leaſt eſſential form; yet this form remaining undeſtroyed by fire, the poles may be changed, and that with eaſe and ſpeed. For among my notes about magnetical experiments, whence I borrow ſome paſſages of this paper, I find the following account.

EXPERIMENT IV.

TO ſhew, that the virtue, that a loadſtone hath by this determinate pole or extreme to attract, for example, the ſouth-end of a poiſed needle, and with the oppoſite extreme or pole the north-end of the ſame needle, I made, among other trials, the following experiment.

TAKING a very ſmall fragment of a loadſtone, I found, agreeably to my conjecture, that by applying ſometimes one pole, ſometimes the other, to that pole of (a ſmall, but) a very vigorous loadſtone, that was fit for my purpoſe, I could at pleaſure, in a few minutes, change the poles of the little fragment, as I tried by its operations upon a needle freely poiſed; though by applying a fragment a pretty deal bigger, (for in itſelf it appeared very ſmall,) I was not able, in far more hours than I employed minutes before, to make any ſenſible change of the poles.

THIS ſhort memorial being added to the preceding part of this diſcourſe, will, I hope, ſatisfy you, that how unanimouſly ſo ever men have deduced all magnetick operations from the form of the loadſtone, yet ſome internal change of pores, or ſome other mechanical alterations, or inward diſpoſition, either of the excited iron, or of the loadſtone itſelf, may ſuffice to make a body capable or uncapable of exerciſing ſome determinate magnetical operations; which may invite you to caſt a more unprejudiced eye upon thoſe few particulars, I ſhall now ſubjoin, to make it probable, that even magnetical qualities may be mechanically produced or altered.

EXPERIMENT V.

I HAVE often obſerved in the ſhops of artificers, as ſmiths, turners of metals, &c. that, when hardened and well tempered tools are well heated by attrition, if, whilſt they are thus warmed, you apply them to filings or chips, as they call them, or thin fragments of ſteel or iron, they will take them up, as if the inſtruments were touched with a loadſtone: but as they will not do ſo, unleſs they be thus excited by rubbing till they be warmed, by which means a greater commotion is made in the inner parts of the ſteel, ſo neither would they retain ſo vigorous a magnetiſm, as to ſupport the little fragments of ſteel, that ſtuck to them after they were grown cold again. Which may be confirmed by what, if I much miſremember not, I ſhewed ſome acquaintances of yours:

EXPERIMENT VI.

WHICH was, that, by barely rubbing a conveniently ſhaped piece of ſteel againſt the floor, till it had gained a ſufficient heat, it would, whilſt it continued ſo, diſcover a manifeſt, though but faint attractive power, which vaniſhed together with the adventitious heat.

EXPERIMENT VII.

WE elſewhere obſerve, which perhaps you alſo may have done, that the iron bars of of windows, by having ſtood very long in an erected poſture, may, at length, grow magnetical, ſo that, if you apply the north point of a poiſed and excited needle to the bottom of the bar, it will drive it away, and attract the ſouthern; and if you raiſe the magnetic needle to the upper part of the bar, and apply it as before, this will draw the northern extreme, which the other end of the bar expelled; probably becauſe, as it is elſewhere declared, the bar is in tract of time, by the continual action of the magnetical effluvia of the terraqueous globe, turned into a kind of magnet, whoſe lower end becomes the north-pole of it, and the other the ſouthern. Therefore, according to the magnetical laws, the former muſt expel the northern extreme of the needle, and the latter draw it.

EXPERIMENT VIII.

I HAVE found indeed, and I queſtion not but other obſervers may have done ſo too, that if a bar of iron, that has not ſtood long in an erected poſture, be but held perpendicular, the forementioned experiment will ſucceed (probably upon ſuch an account as that I lately intimated:) but then this virtue, diſplayed by the extremes of the bar of iron, will not be at all permanent, but ſo tranſient, that if the bar be but inverted and held again upright, that end, which juſt before was the uppermoſt, and drew the north-end of the needle, will now, being lowermoſt, drive it away, which, as was lately obſerved, will not happen to a bar, which has been ſome years, or other competent time, kept in the ſame poſition. So that, ſince length of time is requiſite to make the verticity of a bar of iron ſo durable and conſtant, that the ſame extreme will have the ſame virtues in reference to the magnetical needle, whether you make it the upper end or the lower end of the bar, it ſeems not improbable to me, that by length of time the whole magnetick virtue of this iron may be encreaſed, and conſequently ſome degree of attraction acquired.

AND by this conſideration I ſhall endeavour to explicate that ſtrange thing, that is reported by ſome moderns to have happened in *Italy*, where a bar of iron is affirmed to have been converted into a load-ſtone, whereof a piece was kept, among other rarities, in the curious *Aldrovandus*'s *Muſæum Metallicum*. For conſidering the greatneſs of its ſpecific gravity, the malleableneſs and other properties, wherein iron differs from load-ſtone, I cannot eaſily believe, that by ſuch a way, as is mentioned, a metal ſhould be turned into a ſtone. And therefore, having conſulted the book it ſelf, whence this relation was borrowed, I found the ſtory imperfectly enough delivered; the chiefeſt and cleareſt thing in it being, that at the top of the church of *Arimini* a great iron bar, that was placed there to ſupport a croſs of an hundred pound weight, was at length turned into a loadſtone. But whether the reality of this tranſmutation was examined, and how it appeared, that the fragment of the load-ſtone preſented to *Aldrovandus* was taken from that bar of iron, I am not fully ſatisfied by that narrative. Therefore, when I remember the great reſemblance I have ſometimes ſeen in colour, beſides other manifeſt qualities, betwixt ſome load-ſtones and ſome coarſe or almoſt ruſty iron, I am tempted to conjecture, that thoſe, that obſerved this iron bar, when broken, to have acquired a ſtrong magnetical virtue, which they dreamed not, that tract of time might communicate to it, might eaſily be perſuaded, by this virtue and the reſemblance of colour, that the iron was turned into load-ſtone: eſpecially they being prepoſſeſſed with that Ariſtotelian maxim, whence our author would explain this ſtrange phænomenon, that *inter ſymbolum habentia facilis eſt tranſmutatio*.

BUT leaving this as a bare conjecture, we may take notice, that what virtue an oblong piece of iron may need a long tract of time to acquire, by the help only of its poſition, may be imparted to it in a very ſhort time, by the intervention of ſuch a nimble agent as the fire.

EXPERIMENT IX.

AS may be often, though not always, obſerved in tongs, and ſuch like iron utenſils, that, having been ignited, have been ſet to cool, leaning againſt ſome wall or other prop, that kept them in an erected poſture, which makes it probable, that the great commotion of the parts, made by the vehement heat of the fire, diſpoſed the iron, whilſt it was yet ſoft, and had its pores more lax, and parts more pliable, diſpoſed it, I ſay, to receive much quicker impreſſions from the magnetical effluvia of the earth, than it would have done, if it had ſtill been cold.

EXPERIMENT X.

AND it is very obſervable to our preſent purpoſe, what differing effects are produced by the operation of the fire, upon two magnetic bodies, according to their reſpective conſtitutions. For, by keeping a loadſtone red-hot, though you cool it afterwards in a perpendicular poſture, you may deprive it of its former power of manifeſtly attracting: but a bar of iron being ignited, and ſet to cool perpendicularly, does thereby acquire a manifeſt verticity. Of which differing events I muſt not now ſtay to enquire, whether or no the true reaſon be, that the peculiar texture, or internal conſtitution, that makes a loadſtone ſomewhat more than an ordinary ore of iron, (which metal, as far as I have tried, is the uſual ingredient of loadſtones) being ſpoiled by the violence of the fire, this rude agent leaves it in the condition of common iron, or, perhaps, of ignited iron-ore: whereas the fire does ſoften the iron itſelf, (which is a metal, not an ore) agitating its parts, and making them the more flexible, and by relaxing its pores, diſpoſes it to be eaſily and plentifully pervaded by the magnetical ſteams of the earth, from which it may not improbably be thought to receive the verticity it acquires; and this the rather, becauſe, caufe, as I have often tried, and elfewhere mentioned,

EXPERIMENT XI.

IF an oblong loadftone, once fpoiled by the fire, be thoroughly ignited and cooled, either perpendicularly, or lying horizontally north and fouth, it will, as well as a piece of iron handled after the fame manner, be made to acquire new poles, or change the old ones, as the fkilful experimenter pleafes. But whatever be the true caufe of the difparity of the fire's operation upon a found loadftone and a bar of iron, the effect feems to ftrengthen our conjecture, that magnetical operations may much depend upon mechanical principles. And I hope you will find further probability added to it, by fome phænomena recited in another paper, to which I once committed fome promifcuous experiments and obfervations magnetical.

EXPERIMENT XII.

IF I may be allowed to borrow an experiment from a little tract *, that yet lies by me, and has been feen but by two or three friends, it may be added to the inftances already given about the production of magnetifm. For in that experiment I have fhewn, how having brought a good piece of a certain kind of Englifh oker, which yet, perhaps, was no fitter than other, to a convenient fhape, though, till it was altered by the fire, it difcovered no magnetical quality; yet, after it had been kept red-hot in the fire, and was fuffered to cool in a convenient pofture, it was enabled to exercife magnetical operations upon a poifed needle.

EXPERIMENT XIII.

AS for the abolition of the magnetical virtue in a body endowed with it, it may be made without deftroying the fubftantial, or the effential form of the body, and without fenfibly adding, diminifhing, or altering any thing, in reference to the falt, fulphur, and mercury, which chemifts prefume iron and fteel, as well as other mixed bodies, to be compofed of. For it has been fometimes obferved, that the bare continuance of a loadftone itfelf, in a contrary pofition to that, which, when freely placed, it feems to effect, has either corrupted, or fenfibly leffened the virtue of it. What I formerly obferved to this purpofe, I elfewhere relate, and fince that, having a loadftone, whofe vigour was looked upon, by fkilful perfons, as very extraordinary, and which, whilft it was in an artificer's hand, was therefore held at a high rate, I was careful, being by fome occafions called out of *London*, to lock it up, with fome other rarities, in a cabinet, whereof I took the key along with me, and ftill kept it in my own pocket. But my ftay abroad proving much longer than I expected, when, being returned to *London*, I had occafion to make ufe of this loadftone for an experiment, I found it indeed where I left it, but fo exceedingly decayed, as to its attractive power, which I had formerly examined by weight, by having lain almoft a year in an inconvenient pofture, that if it had not been for the circumftances newly related, I fhould have concluded, that fome body had purpofely got it out in my abfence, and fpoiled it by help of the fire, the virtue being fo much impaired, that I cared little to employ it any more about confiderable experiments.

EXPERIMENT XIV.

AND this corruption of the magnetical virtue, which may, in tract of time, be made in a loadftone itfelf, may, in a trice, be made by the help of that ftone in an excited needle. For, it is obferved by magnetical writers, and my own trials, purpofely made, have affured me of it, that a well-poifed needle, being, by the touch of a good loadftone, excited and brought to turn one of its ends to the north, and the other to the fouth, it may, by a contrary touch of the fame loadftone, be deprived of the faculty it had of directing its determinate extremes to determinate poles. Nay, by another touch (or the fame, and even without immediate contact, if the magnet be vigorous enough) the needle may prefently have its direction fo changed, that the end, which formerly pointed to the north pole, fhall now regard the fouth, and the other end fhall, inftead of the fouthern, refpect the northern pole.

EXPERIMENT XV.

AND to make it the more probable, that the change of the magnetifm, communicated to iron, may be produced, at leaft, in good part, by mechanical operations, procuring fome change of texture in the iron; I fhall fubjoin a notable experiment of the ingenious Dr. *Power*, which, when I heard of, I tried as well as I could; and though, perhaps for want of conveniency, I could not make it fully anfwer what it promifed, yet the fuccefs of the trial was confiderable enough to make it pertinent in this place, and to induce me to think, it might yet better fucceed with him, whofe experiment, as far as it concerns my prefent purpofe, imports, that if a puncheon, as fmiths call it, or a rod of iron, be, by being ignited and fuffered to cool north and fouth, and hammered at the ends, very manifeftly endowed with magnetical virtue, this virtue will, in a trice be deftroyed, by two or three fmart blows of a ftrong hammer, upon the middle of the oblong piece of iron.

BUT magnetifm is fo fertile a fubject, that, if I had now the leifure and conveniency to range among magnetical writers, I fhould fcarce doubt of finding, among their many experiments and obfervations, divers, that might be added to thofe above delivered, as being eafily applicable to my prefent argument. And I hope you will find farther probability added to what has been faid, to fhew, "that "magnetical operations may much depend up-"on mechanical principles," by fome phænomena recited in another paper, to which I once committed fome promifcuous experiments and obfervations magnetical.

EXPE-

* Relating to the magnetifm of the earth.

EXPERIMENTS AND NOTES,

ABOUT THE

Mechanical ORIGIN or PRODUCTION

OF

ELECTRICITY.

THAT it is not neceſſary to believe electrical attraction (which, you know, is generally liſted among occult qualities) to be the effect of a naked and ſolitary quality, flowing immediately from a ſubſtantial form; but that it may rather be the effect of a material effluvium, iſſuing from, and returning to the electrical body (and perhaps in ſome caſes aſſiſted in its operation by the external air) ſeems agreeable to divers things, that may be obſerved in ſuch bodies and their manner of acting.

THERE are differing hypotheſes (and all of them mechanical, propoſed by the moderns) to ſolve the phænomena of electrical attraction. Of theſe opinions the firſt is, that of the learned Jeſuit *Cabæus*, who, though a Peripatetick and commentator on *Ariſtotle*, thinks the drawing of light bodies by jet, amber, &c. may be accounted for, by ſuppoſing, that the ſteams, that iſſue, or, if I may ſo ſpeak, ſally out of amber, when heated by rubbing, diſcuſs and expel the neighbouring air; which after it has been driven off a little way, makes as it were a ſmall whirlwind, becauſe of the reſiſtance it finds from the remoter air, which has not been wrought on by the electrical ſteams; and that theſe, ſhrinking back ſwiftly enough to the amber, do in their returns bring along with them ſuch light bodies, as they meet with in their way. On occaſion of which hypotheſis I ſhall offer it to be conſidered, whether by the gravity of the atmoſpherical air, ſurmounting the ſpecifick gravity of the little and rarified atmoſphere, made about the amber by its emiſſions, and compriſing the light body faſtened on by them, the attraction may not in divers caſes be either cauſed or promoted.

ANOTHER hypotheſis is that propoſed by that ingenious gentleman, Sir *Kenelm Digby*, and embraced by the very learned Dr. *Browne*, (who ſeems to make our *Gilbert* himſelf to have been of it) and divers other ſagacious men. And, according to this hypotheſis, the amber, or other electrick, being chafed or heated, is made to emit certain rays or files of unctuous ſteams, which, when they come to be a little cooled by the external air, are ſomewhat condenſed, and having loſt of their former agitation, ſhrink back to the body, whence they ſallied out, and carry with them thoſe light bodies, that their further ends happen to adhere to, at the time of their retraction: as when a drop of oil or ſyrup hangs from the end of a ſmall ſtick, if that be dextrouſly and cautiouſly ſtruck, the viſcous ſubſtance will, by that impulſe, be ſtretched out, and preſently retreating, will bring along with it the duſt or other light bodies, that chanced to ſtick to the remoter parts of it.

AND this way of explaining electrical attractions is employed alſo by the learned *Gaſſendus*, who adds to it, that theſe electrical rays, if they may be ſo called, being emitted ſeveral ways, and conſequently croſſing one another, get into the pores of the ſtraw, or other light body to be attracted, and by means of their decuſſation, take the faſter hold of it, and have the greater force to carry it along with them, when they ſhrink back to the amber, whence they were emitted.

A third hypotheſis there is, which was deviſed by the acute *Carteſius*, who diſlikes the explications of others, chiefly becauſe he thinks them not applicable to glaſs, which he ſuppoſes unfit to ſend forth effluvia, and which is yet an electrical body; and therefore attempts to account for electrical attractions by the intervention of certain particles, ſhaped almoſt like ſmall pieces of ribband, which he ſuppoſes to be formed of this ſubtile matter harboured in the pores or creviſes of glaſs. But this hypotheſis, though ingenious in itſelf, yet depending upon the knowledge of divers of his peculiar principles, I cannot intelligibly propoſe it in few words, and therefore ſhall refer you to himſelf for an account of it: which I the leſs ſcruple to do, becauſe though it be not unworthy of the wonted acuteneſs of the author, yet he ſeems himſelf to doubt, whether it will reach all electrical bodies; and it ſeems to me, that the reaſon, why he rejects the way of explicating attraction by the emiſſion of the finer parts of the attrahent (to which hypotheſis, if it be rightly propoſed, I confeſs myſelf very inclinable) is grounded upon a miſtake, which, though a philoſopher may, for want of experience in that particular, without diſparagement fall into, is nevertheleſs a miſtake. For whereas our excellent author ſays, that electrical

Princip. part IV. art. 184.

cal effluvia, ſuch as are ſuppoſed to be emitted by amber, wax, &c. cannot be imagined to proceed from glaſs, I grant the ſuppoſition to be plauſible, but cannot allow it to be true. For as ſolid a body as glaſs is, yet if you but dexterouſly rub for two or three minutes a couple of pieces of glaſs againſt one another, you will find, that glaſs is not only capable of emitting effluvia, but ſuch ones, as to be odorous, and ſometimes to be rankly ſtinking.

BUT it is not neceſſary, that in this paper, where I pretend not to write diſcourſes but notes, I ſhould conſider all, that has been, or I think may be, ſaid for and againſt each of the abovementioned hypotheſes; ſince they all agree in what is ſufficient for my preſent purpoſe, namely, that electrical attractions are not the effects of a mere quality, but of a ſubſtantial emanation from the attracting body; and it is plain, that they all endeavour to ſolve the phænomena in a mechanical way, without recurring to ſubſtantial forms, and inexplicable qualities, or ſo much as taking notice of the hypoſtatical principles of the chemiſts. Wherefore it may ſuffice in this place, that I mention ſome phænomena, that in general make it probable, that amber, &c. draws ſuch light bodies, as pieces of ſtraw, hair, and the like, by virtue of ſome mechanical affections either of the attracting, or of the attracted bodies, or of both the one and the other.

1. THE firſt and moſt general obſervation is, that electrical bodies draw not, unleſs they be warmed; which rule, though I have now and then found to admit of an exception, (whereof I elſewhere offer an account,) yet as to the generality of common electricks, it holds well enough to give much countenance to our doctrine, which teaches the effects of electrical bodies to be performed by corporeal emanations. For it is known, that heat, by agitating the parts of a fit body, ſolicites it as it were to ſend forth its effluvia, as is obvious in odoriferous gums and perfumes, which, being heated, ſend forth their fragrant ſteams, both further and more copiouſly, than otherwiſe they would.

2. NEXT, it has been obſerved, that amber, &c. warmed by the fire, does not attract ſo vigorouſly, as if it acquire an equal degree of heat by being chafed or rubbed: ſo that the modification of motion in the internal parts, and in the emanations of the am ber, may, as well as the degree of it, contribute to the attraction. And my particular obſervations incline me to add, that the effect may oftentimes be much promoted, by employing both theſe ways ſucceſſively; as I thought I manifeſtly found, when I firſt warmed the amber at the fire, and preſently after chafed it a little upon a piece of cloth. For then a very few rubbings ſeemed to excite it more than many more would otherwiſe have done: as if the heat of the fire had put the parts into a general, but confuſed agitation; to which it was eaſy for the ſubſequent attrition (or reciprocation of preſſure) to give a convenient modification in a body, whoſe texture diſpoſes it to become vigorouſly electrical.

3. ANOTHER obſervation, that is made about theſe bodies, is, that they require terſion as well as attrition; and though I doubt whether the rule be infallible, yet I deny not, but that weaker electricks require to be as well wiped as chafed; and even good ones will have their operation promoted by the ſame means. And this is very agreeable to our doctrine, ſince terſion, beſides that it is, as I have ſometimes manifeſtly known it, a kind or degree of attrition, frees the ſurface from thoſe adherences, that might choak the pores of the amber, or at leaſt hinder the emanation of the ſteams to be ſo free and copious, as otherwiſe they would be.

4. IT is likewiſe obſerved, that whereas the magnetical ſteams are ſo ſubtile, that they penetrate and perform their operation through all kind of mediums hitherto known to us; electrical ſteams are like thoſe of ſome odoriferous bodies, eaſily checked in their progreſs, ſince it is affirmed by learned writers, who ſay they ſpeak upon particular trial, that the interpoſition of the fineſt linnen, or ſarſnet, is ſufficient to hinder all the operation of excited amber upon a ſtraw or feather placed never ſo little beyond it.

5. IT has been alſo obſerved, that the effects of electrical attraction are weakened, if the air be thick and cloudy; and eſpecially if the ſouth wind blows; and that electricks diſplay their virtue more faintly by night than by day, and more vigorouſly in clear weather, and when the winds are northerly. All which the learned *Kircherus* aſſerts himſelf to have found true by experience; inſomuch, that thoſe bodies, that are but faintly drawn, when the weather is clear, will not, when it is thick and cloudy, be at all moved.

6. WE have alſo obſerved, that divers concretes, that are notably electrical, do abound in an effluviable matter, (if I may ſo call it) which is capable of being manifeſtly evaporated by heat and rubbing. Thus we ſee, that moſt reſinous gums, that draw light bodies, do alſo, being moderately ſollicited by heat, (whether this be excited by the fire, or by attrition or contuſion) emit ſteams. And in pieces of ſulphur conveniently ſhaped, I found, upon due attrition, a ſulphureous ſtink. And that piece of amber, which I moſt employ, being ſomewhat large, and very well poliſhed, will, being rubbed upon a piece of woollen cloth, emit ſteams, which the noſtrils themſelves may perceive; and they ſometimes ſeem to me not unlike thoſe, that I took notice of, when I kept in my mouth a drop or two of the diluted tincture (or ſolution of the finer parts) of amber made with ſpirit of wine, or of ſal armoniac.

7. IT agrees very well with what has been ſaid of the corporeal emanations of amber, that its attractive power will continue ſome time after it has been once excited. For the attrition having cauſed an inteſtine commotion in the parts of the concrete, the heat or warmth, that is thereby excited, ought not to ceaſe, as ſoon as ever the rubbing is over, but to continue capable of emitting effluvia for ſome time afterwards, which will be longer or ſhorter, accord-

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ing to the goodneſs of the electric, and the degree of the antecedent commotion: which, joined together, may ſometimes make the effect conſiderable, inſomuch that in a warm day, about noon, I did, with a certain body, not much, if at all, bigger than a pea, but very vigorouſly attractive, move to and fro a ſteel needle, freely poiſed, about three minutes (or the twentieth part of an hour) after I had left off rubbing the attrahent.

8. THAT it may not ſeem impoſſible, that electrical effluvia ſhould be able to inſinuate themſelves into the pores of many other bodies, I ſhall add, that I found them ſubtile enough to attract not only ſpirit of wine, but that fluid aggregate of corpuſcles we call ſmoke. For having well lighted a wax taper, which I preferred to a common candle, to avoid the ſtink of the ſnuff, I blew out the flame; and, when the ſmoke aſcended in a ſlender ſtream, held, at a convenient diſtance from it, an excited piece of amber, or a chafed diamond, which would manifeſtly make the aſcending ſmoke deviate from its former line, and turn aſide, to beat, as it were, againſt the electric, which, if it were vigorous, would act at a conſiderable diſtance, and ſeemed to ſmoke for a pretty while together.

9. THAT it is not in any peculiar ſympathy between an electric and a body, whereon it operates, that electrical attraction depends, ſeems the more probable, becauſe amber, for inſtance, does not attract only one determinate ſort of bodies, as the loadſtone does iron, and thoſe bodies, wherein it abounds; but, as far as I have yet tried, it draws indifferently all bodies whatſoever, being placed within a due diſtance from it, (as my choiceſt piece of amber draws not only ſand and mineral powders, but filings of ſteel and copper, and beaten gold itſelf) provided they be minute or light enough, except, perhaps, it be fire: I employ the word perhaps, becauſe I am not yet ſo clear in this point. For having applied a ſtrong electric, at a convenient diſtance, to ſmall fragments of ignited matter, they were readily enough attracted, and ſhined, whilſt they were ſticking to the body, that had drawn them. But, when I looked attentively upon them, I found the ſhining ſparks to be, as it were, cloathed with light aſhes, which, in ſpite of my diligence, had been already formed about the attracted corpuſcles, upon the expiring of a good part of the fire; ſo that it remained ſomewhat doubtful to me, whether the ignited corpuſcles, whilſt they were totally ſuch, were attracted; or whether the immediate objects of the attraction were not the new formed aſhes, which carried up with them thoſe yet unextinguiſhed parts of fire, that chanced to be lodged in them. But, as for flame, our countryman *Gilbert* delivers, as his experiment, that an electric, though duly excited and applied, will not move the flame of the ſlendereſt candle; which ſome will think not ſo eaſy to be well tried with common electricks, as amber, hard wax, ſulphur, and the like unctuous concretes, that very eaſily take fire: therefore I choſe to make my trial, with a rough diamond, extraordinarily attractive, which I could, without injuring it, hold, as near as I pleaſed, to the flame of a candle, or taper; and though I was not ſatisfied, that it did either attract the flame, as it viſibly did the ſmoke, or manifeſtly agitate it; yet, granting, that *Gilbert*'s aſſertion will conſtantly hold true, and ſo, that flame is to be excepted from the general rule, yet this exception may well comport with the hypotheſis hitherto countenanced, ſince it may be ſaid, as it is, if I miſtake not, by *Kircherus*, that the heat of the flame diſſipates the effluvia, by whoſe means the attraction ſhould be performed. To which I ſhall add, that poſſibly the celerity of the motion of the flame upwards may render it very difficult, for the electrical emanations to divert the flame from its courſe.

10. WE have found by experiment, that a vigorous and well excited piece of amber will draw, not only the powder of amber, but leſs minute fragments of it. And as, in many caſes, one contrary directs to another, ſo this trial ſuggeſted a further, which, in caſe of good ſucceſs, would probably argue, that, in electrical attraction, not only effluvia are emitted by the electrical body, but theſe effluvia faſten upon the body to be drawn, and that in ſuch a way, that the intervening viſcous ſtrings, which may be ſuppoſed to be made up of thoſe cohering effluvia, are, when their agitation ceaſes, contracted or made to ſhrink inwards, towards both ends, almoſt as a highly ſtretched luteſtring does, when it is permitted to retreat into ſhorter Dimenſions. But the conjecture itſelf was much more eaſy to be made, than the experiment requiſite to examine it. For we found it no eaſy matter to ſuſpend an electric, great and vigorous enough, in ſuch a manner, that it might, whilſt ſuſpended, be excited, and be ſo nicely poiſed, that ſo faint a force as that, wherewith it attracts light bodies, ſhould be able to procure a local motion to the whole body itſelf. But after ſome fruitleſs attempts with other electricks, I had recourſe to the very vigorous piece of poliſhed amber, formerly mentioned; and when we had, with the help of a little wax, ſuſpended it by a ſilken thread, we chafed very well one of the blunt edges of it upon a kind of large pin-cuſhion, covered with a coarſe and black woollen ſtuff, and then brought the electric, as ſoon as we could, to ſettle, notwithſtanding its hanging freely at the bottom of the ſtring. This courſe of rubbing on the edge of the amber we pitched upon for more than one reaſon; for if we had chafed the flat ſide, the amber could not have approached the body it had been rubbed on, without making a change of place in the whole electric; and, which is worſe, without making it move (contrary to the nature of heavy bodies) ſomewhat upwards; whereas the amber having, by reaſon of its ſuſpenſion, its parts counterpoiſed by one another, to make the excited edge approach to another body, that edge needed not all aſcend, but only be moved horizontally, to which way of moving the gravity of the electric (which the ſtring kept from moving downwards) could be but little or no hinderance. And, agreeably to this, we found, that if, as ſoon as the ſuſpended and well rubbed electric was brought to ſettle freely, we applied to the chafed edge, but without touching

ing it, the lately mentioned cushion, which, by reason of its rough superficies and porosity, was fit for the electrical effluvia to fasten upon, the edge would manifestly be drawn aside by the cushion steadily held, and if this were slowly removed, would follow it a good way; and when this body no longer detained it, would return to the posture, wherein it had settled before. And this power of approaching the cushion, by virtue of the operation of its own steams, was so durable in our vigorous piece of amber, that, by once chafing it, I was able to make it follow the cushion no less than ten or eleven times. Whether from such experiments one may argue, that it is but, as it were, by accident, that amber attracts another body, and not this the amber; and whether these ought to make us question, if electricks may, with so much propriety, as has been hitherto generally supposed, be said to attract, are doubts, that my design does not here oblige me to examine.

SOME other phænomena might be added of the same tendency with those already mentioned, (as the advantage, that electrical bodies usually get by having well polished, or, at least, smooth surfaces;) but the title of this paper promising some experiments about the production of electricity, I must not omit to recite, how I have been sometimes able to produce or destroy this quality in certain bodies, by means of alterations, that appeared not to be other than mechanical.

EXPERIMENT I.

AND first, having, with a very mild heat, slowly evaporated about a fourth part of good turpentine, I found, that the remaining body would not, when cold, continue a liquor, but hardened into a transparent gum almost like amber, which, as I looked for, proved electrical.

EXPERIMENT II.

SECONDLY, by mixing two such liquid bodies, as petroleum and strong spirit of nitre, in a certain proportion, and then distilling them till there remained a dry mass, I obtained a brittle substance as black as jet; and whose superficies (where it was contiguous to the retort) was glossy, like that mineral, when polished; and, as I expected, I found it also to resemble jet, in being endowed with an electrical faculty.

EXPERIMENT III.

THIRDLY, having burnt antimony to ashes, and of those ashes, without any addition, made a transparent glass, I found, that, when rubbed, as electrical bodies ought to be to excite them, it answered my expectation, by manifesting a not inconsiderable electricity. And this is the worthier of notice, because, that as a vitrum antimonii, that is said to be purer than ordinary, may be made of the regulus of the same mineral, in whose preparation you know a great part of the antimonial sulphur is separated, and left among the scoriæ; so glass of antimony, made without additament, may easily, as experience has informed us, be in part reduced to a regulus, (a body not reckoned amongst electrical ones.) And that you may not think, that it is only some peculiar and fixed part of the antimony, that is capable of vitrification, let me assure you, that, even with the other part, that is wont to fly away, (namely, the flowers) an antimonial glass may, without an addition of other ingredients, be made.

EXPERIMENT IV.

FOURTHLY, the mention of a vitrified body brings into my mind, that I more than once made some glass of lead *per se*, (which I found no very easy work) that also was not wholly destitute of an electrical virtue, though it had but a very languid one. And it is not here to be overlooked, that this glass might easily be brought to afford again malleable lead, which was never reckoned, that I know of, among electrical bodies.

EXPERIMENT V.

FIFTHLY, having taken some amber, and warily distilled it, not with sand or powdered brick, or some such additament, as chemists are wont to use, for fear it should boil over, or break their vessels; but by itself, that I might have an unmixed caput mortuum: having made this distillation, I say, and continued it, till it had afforded a good proportion of phlegm, spirit, volatile salt, and oil, the retort was warily broken, and the remaining matter was taken out in a lump, which, though it had quite lost its colour, being burned quite black, and though it were grown strangely brittle, in comparison of amber, so that they, who believe the virtue of attracting light bodies to flow from the substantial form of amber, would not expect it in a body so changed and deprived of its noblest parts; yet this caput mortuum was so far from having lost its electrical faculty, that it seemed to attract more vigorously than amber itself is wont to do, before it be committed to distillation.

AND from the foregoing instances afforded us by the glass of antimony, we may learn, that when the form of a body seems to be destroyed by a fiery analysis, that dissipates the parts of it, the remaining substance may yet be endowed with electricity, as the *caput mortuum* of amber may acquire it; as in the case of the glass of antimony made of the calx and of the flowers. And from the second example abovementioned, and from common glass, which is electrical, we may also learn, that bodies, that are neither of them apart observed to be endowed with electricity, may have that virtue result in the compounded substance, that they constitute, though it be but a factitious body.

To the foregoing experiments, whose success is wont to be uniform enough, I shall add the recital of a surprising phænomenon, which, though not constant, may help to make it probable, that electrical attractions need not be supposed still to proceed from the substantial, or even from the essential form of the attrahent, but may be the effects of unheeded, and, as it were, fortuitous causes. And, however, I dare not

not ſuppreſs ſo ſtrange an obſervation, and therefore ſhall relate that, which I had the luck to make of an odd ſort of electrical attraction (as it ſeemed,) not taken notice of (that I know of) by any, either naturaliſt or other writer, and it is this.

EXPERIMENT VI.

THAT falſe locks, as they call them, of ſome hair, being by curling or otherwiſe brought to a certain degree of dryneſs, or of ſtiffneſs, will be attracted by the fleſh of ſome perſons, or ſeem to apply themſelves to it, as hair is wont to do to amber or jet excited by rubbing. Of this I had a proof in ſuch locks worn by two very fair ladies, that you know. For at ſome times I obſerved, that they could not keep their locks from flying to their cheeks, and (though neither of them made any uſe, or had any need of painting) from ſticking there. When one of theſe beauties firſt ſhewed me this experiment, I turned it into a complemental raillery, as ſuſpecting there might be ſome trick in it, though I after ſaw the ſame thing happen to the others locks too. But as ſhe is no ordinary virtuoſa, ſhe very ingeniouſly removed my ſuſpicions, and, as I requeſted, gave me leave to ſatisfy my ſelf further, by deſiring her to hold her warm hand at a convenient diſtance from one of thoſe locks taken off and held in the air. For as ſoon as ſhe did this, the lower end of the lock, which was free, applied itſelf preſently to her hand: which ſeemed the more ſtrange, becauſe ſo great a multitude of hair would not have been eaſily attracted by an ordinary electrical body, that had not been conſiderably large, or extraordinarily vigorous. This repeated obſervation put me upon enquiring among ſome other young ladies, whether they had obſerved any ſuch like thing; but I found little ſatisfaction to my queſtion, except from one of them eminent for being ingenious, who told me, that ſometimes ſhe had met with theſe troubleſome locks; but that all ſhe could tell me of the circumſtances, which I would have been informed about, was, that they ſeemed to her to fly moſt to her cheeks, when they had been put into a ſomewhat ſtiff curl, and when the weather was froſty*.

YOU will probably be the leſs diſpoſed to believe, that electrical attractions muſt proceed from the ſubſtantial forms of the attrahents, or from the predominancy of this or that chemical principle in them, if I acquaint you with ſome odd trials, wherein the attraction of light bodies ſeemed to depend upon very ſmall circumſtances. And though forbearing at preſent, to offer you my thoughts about the cauſe of theſe ſurpriſing phænomena, I propoſe it only as a problem to your ſelf, and your curious friends, yet the main circumſtances ſeeming to be of a mechanical nature, the recital of my trials will not be impertinent to the deſign and ſubject of this paper.

EXPERIMENT VII.

I TOOK then a large and vigorous piece of amber, conveniently ſhaped for my purpoſe, and a downy feather, ſuch as grows upon the bodies, not wings or tails of a ſomewhat large chicken: then having moderately excited the electrick, I held the amber ſo near it, that the neighbouring part of the feather was drawn by it, and ſtuck faſt to it; but the remoter parts continued in their former poſture. This done, I applied my fore-finger to theſe erected downy feathers, and immediately, as I expected, they left their preceding poſture, and applied themſelves to it, as if it had been an electrical body. And whether I offered to them my nail, or the pulpy part of my finger, or held my finger towards the right hand or the left, or directly over, theſe downy feathers, that were near the little quill, did nimbly, and, for aught appeared, equally turn themſelves towards it. And to ſhew, that the ſteams, that iſſued out of ſo warm a body as my finger, were not neceſſary to attract, as men ſpeak, the abovementioned feathers, inſtead of my finger, I applied to them, after the ſame manner, a little cylindrical inſtrument of ſilver, to which they bowed and faſtened themſelves, as they had done to my finger, though the tip of this inſtrument were preſented to them in ſeveral poſtures. The like ſucceſs I had with the end of an iron key, and the like alſo with a cold piece of poliſhed black marble; and ſometimes the feathers did ſo readily and ſtrongly faſten themſelves to theſe extraneous and unexcited bodies, that I have been able, though not eaſily, to make one of them draw the feather from the amber itſelf.

BUT it is diligently to be obſerved, that this unuſal attraction happened only, whilſt the electrical operation of the excited amber continued ſtrong enough to ſuſtain the feathers. For afterwards, neither the approach of my finger, nor that of the other bodies, would make the downy feathers change their poſture. Yet, as ſoon as ever the amber was by light affriction excited again, the feather would be diſpoſed to apply itſelf again to the abovementioned bodies.

AND leſt there ſhould be any peculiarity in that particular feather, I made the trials, with others,

* Some years after the making the experiments about the production of electricity, having a deſire to try, whether in the attractions made by amber, the motions excited by the air had a conſiderable intereſt, or whether the effect were not due rather to the emiſſion and retraction of effluvia, which being of a viſcous nature may conſiſt of particles either branched or hooked, or otherwiſe fit for ſome kind of coheſion, and capable of being ſtretched, and of ſhrinking again, as leather thongs are: to examine this, I ſay, I thought the fitteſt way, if it were practicable, would be, to try, whether amber would draw a light body in a glaſs whence the air was pumped out. And though the trial of this ſeemed very difficult to make, and we were ſomewhat diſcouraged by our firſt attempt, wherein the weight of the ambient air broke our receiver, which chanced to prove too weak, when the internal air had been with extraordinary diligence pumped out; yet having a vigorous piece of amber, which I had cauſed to be purpoſely turned and poliſhed for electrical experiments, I afterwards repeated the trial, and found, that in warm weather, it would retain a manifeſt power of attracting for ſeveral minutes (for it ſtirred a poiſed needle after above $\frac{1}{4}$ of an hour) after we had done rubbing it. Upon which encouragement we ſuſpended it, being firſt well chafed, in a glaſs receiver, that was not great, juſt over a light body; and making haſte with our air-pump to exhauſt the glaſs, when the air was withdrawn, we did by a contrivance let down the ſuſpended amber, till it came very near the ſtraw or feather, and perceived, as we expected, that in ſome trials, upon the leaſt contact it would lift it up; and in others, (for we repeated the experiment,) the amber would raiſe it without touching it, that is, would attract it.

others, (provided they were not long enough to exceed the ſphere of activity of the amber) and found the experiment to anſwer my expectation.

I made the experiment alſo at differing times, and with ſome months, if not rather years, of interval, but with the like ſucceſs.

AND leſt you ſhould think theſe phænomena proceed from ſome peculiarity in the piece of amber I employed, I ſhall add, that I found uniformity enough in the ſucceſs, when, in the place of amber, I ſubſtituted another electrick, and particularly a ſmooth maſs of melted brimſtone.

THESE are the phænomena I thought fit to mention, at preſent, of this unuſual way of drawing light bodies, and with this experiment I ſhould conclude my notes about electricity, but that, I think, it will not be amiſs, before I take leave of this ſubject, to give this advertiſement, that the event of electrical experiments is not always ſo certain as that of many others, being ſometimes much varied by ſeemingly ſlight circumſtances, and now and then by ſome, that are altogether over-looked. This obſervation may receive credit from ſome of the particulars above recited (eſpecially concerning the intereſt of the weather, &c. in electrical phænomena.) But now I ſhall add, that, not only there may happen ſome variations in the ſucceſs of trials made with electrical bodies, but that it is not ſo certain as many think, whether ſome particular bodies be or be not electrical. For the inquiſitive *Kircherus* reckons cryſtal among thoſe gems, to whom nature has denied the attractive power we are ſpeaking of; and yet I remember not, that among all the trials I have made with natural cryſtal, I have found any, that was deſtitute of the power he refuſes them. Alſo a late moſt learned writer reciting the electricks, reckoned up by our induſtrious countryman *Gilbert*, and increaſing their number by ſome obſerved by himſelf, (to which I ſhall now add, beſides white ſapphires, and white Engliſh amethyſts, the almoſt diaphanous ſpar of lead ore) denies electricity to a couple of tranſparent gems, the cornelian and the emerald. And I do the leſs wonder he ſhould do ſo to the former, becauſe I have myſelf, in vain, tried to make any attraction with a piece of cornelian ſo large and fair, that it was kept for a rarity; and yet with divers other fine cornelians I have been able to attract ſome light bodies very manifeſtly, if not briſkly; and I uſually wear a cornelian ring, that is richly enough endowed with electricity. But as for emeralds, as I thought it ſtrange, that nature ſhould have denied them a quality ſhe has granted to ſo many other diaphanous gems, and even to cryſtal, ſo I thought the aſſertion deſerved an examen, upon which I concluded, that, at leaſt, it does not univerſally and conſtantly hold true. I had, indeed, ſeen in a ring a ſtone of price and great luſtre, which, though green, I found to be, (as I gueſſed it would prove) vigorouſly enough electrical. But this experiment, though ſeemingly concluſive, I did not look upon as a fair trial, becauſe the ſtone was not a true emerald, but, which is rare, a green ſapphire. And I learned by enquiry of the ſkilful jeweller, that cut it, that it was ſo far from having the ſoftneſs of an emerald, that he found it harder than blue ſapphires themſelves, which yet are gems of great hardneſs, and by ſome reputed ſecond to none, but diamonds. Without therefore concluding any thing from this experiment, ſave that, if the aſſertion I was to examine were true, the want of an electrical faculty might be thought a concomitant rather of the peculiar texture of the emerald than of its green colour, I proceeded to make trial with three or four emeralds, whoſe being true was not doubted, and found them all ſomewhat, though not equally, endowed with electricity, which I found to be yet more conſiderable in an emerald of my own, whoſe colour was ſo excellent, that by ſkilful perſons it was looked on as a rarity. And though, by this ſucceſs of my enquiry, I perceived I could not, as elſe I have done, ſhew the curious a new way of judging of true and falſe emeralds, yet the like may be, though not always certain, yet oftentimes of uſe, in the eſtimating whether diamonds be true or counterfeit, eſpecially if, being ſet in rings, the ſureſt way of trying them cannot conveniently be employed. For whereas glaſs, though it have ſome electricity, ſeems, as far as I have obſerved, to have but a faint one, there are often found diamonds that have a very vigorous one. And I do not remember I met with any electrick of the ſame bulk, that was more vigorous than a rough diamond I have, which is the ſame, that I formerly mentioned to have moved a needle above three minutes after I had ceaſed to chafe it. And this brings into my mind, that it has been obſerved, that diamonds draw better whilſt rough, than they do after they are cut and poliſhed; which ſeeming to contradict what has been obſerved by others, and by us alſo, that amber, for inſtance, attracts more vigorouſly, if the ſurface be made very ſmooth than otherwiſe, it induces me to conjecture, that, if this obſervation about diamonds be true, as ſome of my trials have now and then inclined me to think it, and if it do not in ſome caſes conſiderably depend upon the loſs of the (electrical) ſubſtance of the ſtone, by its being cut and ground, the reaſon may poſſibly be, that the great rapidneſs, with which the wheels, that ſerve to cut and poliſh diamonds muſt be moved, does excite a great degree of heat, (which the ſenſes may eaſily diſcover) in the ſtone, and by that and the ſtrong concuſſion it makes of its parts, may force it to ſpend its effluviable matter, if I may ſo call it, ſo plentifully, that the ſtone may be impoveriſhed, and perhaps alſo, on the account of ſome little change in its texture, be rendered leſs diſpoſed to emit thoſe effluvia, that are inſtruments of electrical attraction. But as I willingly leave the matter of fact to further trial, ſo I do the cauſe of it, in caſe it prove true, to further enquiry.

The END of the THIRD VOLUME.

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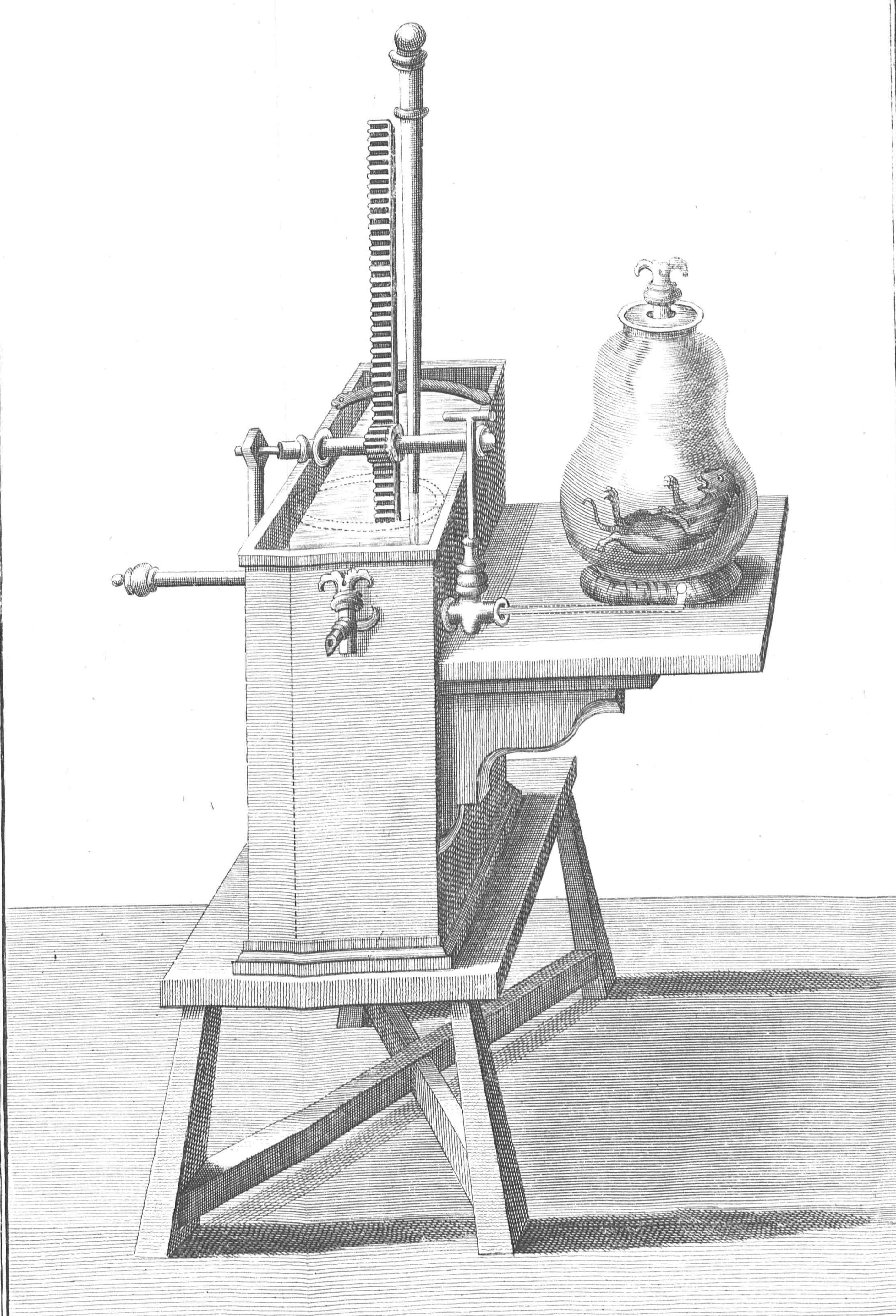

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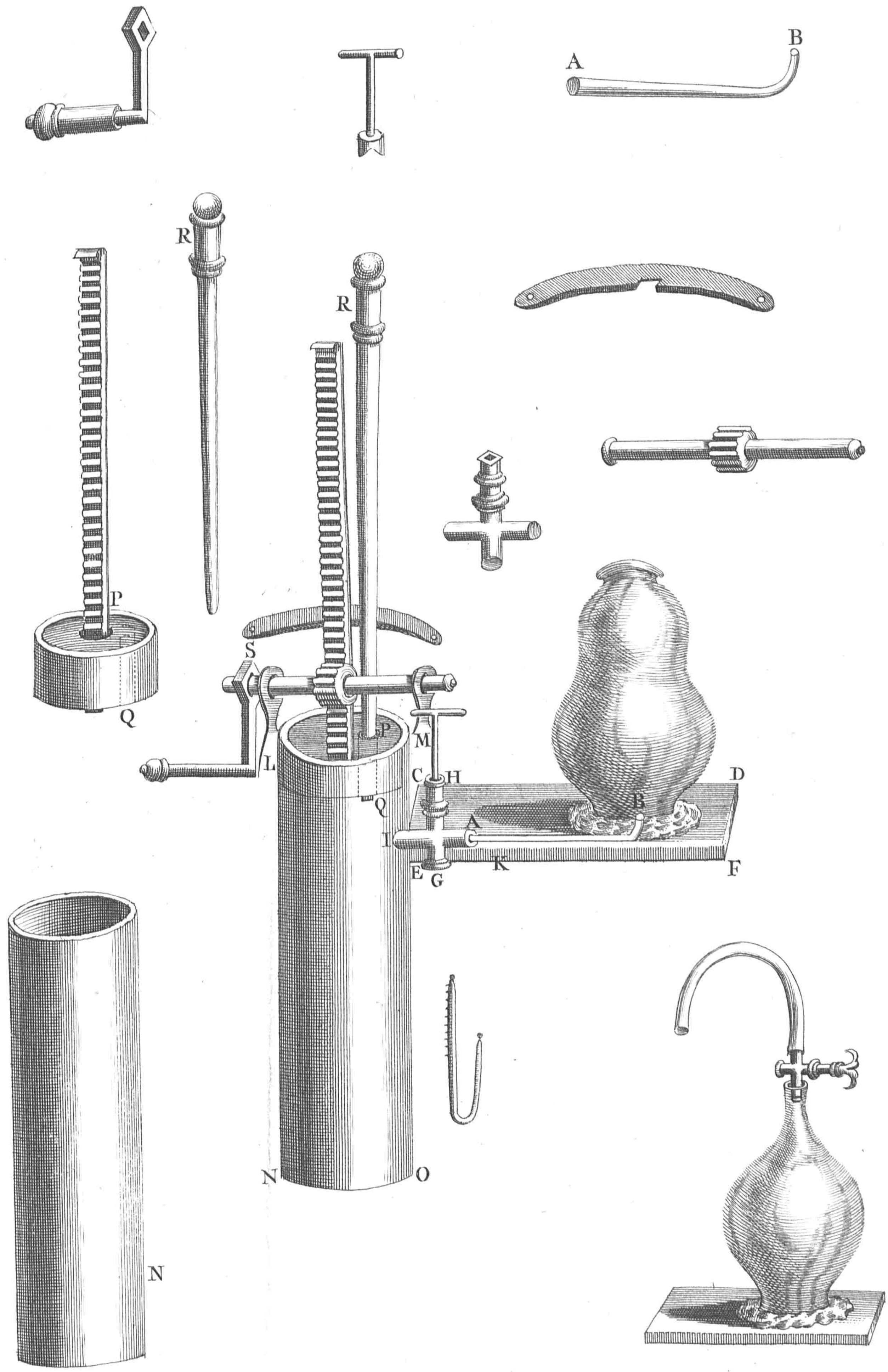

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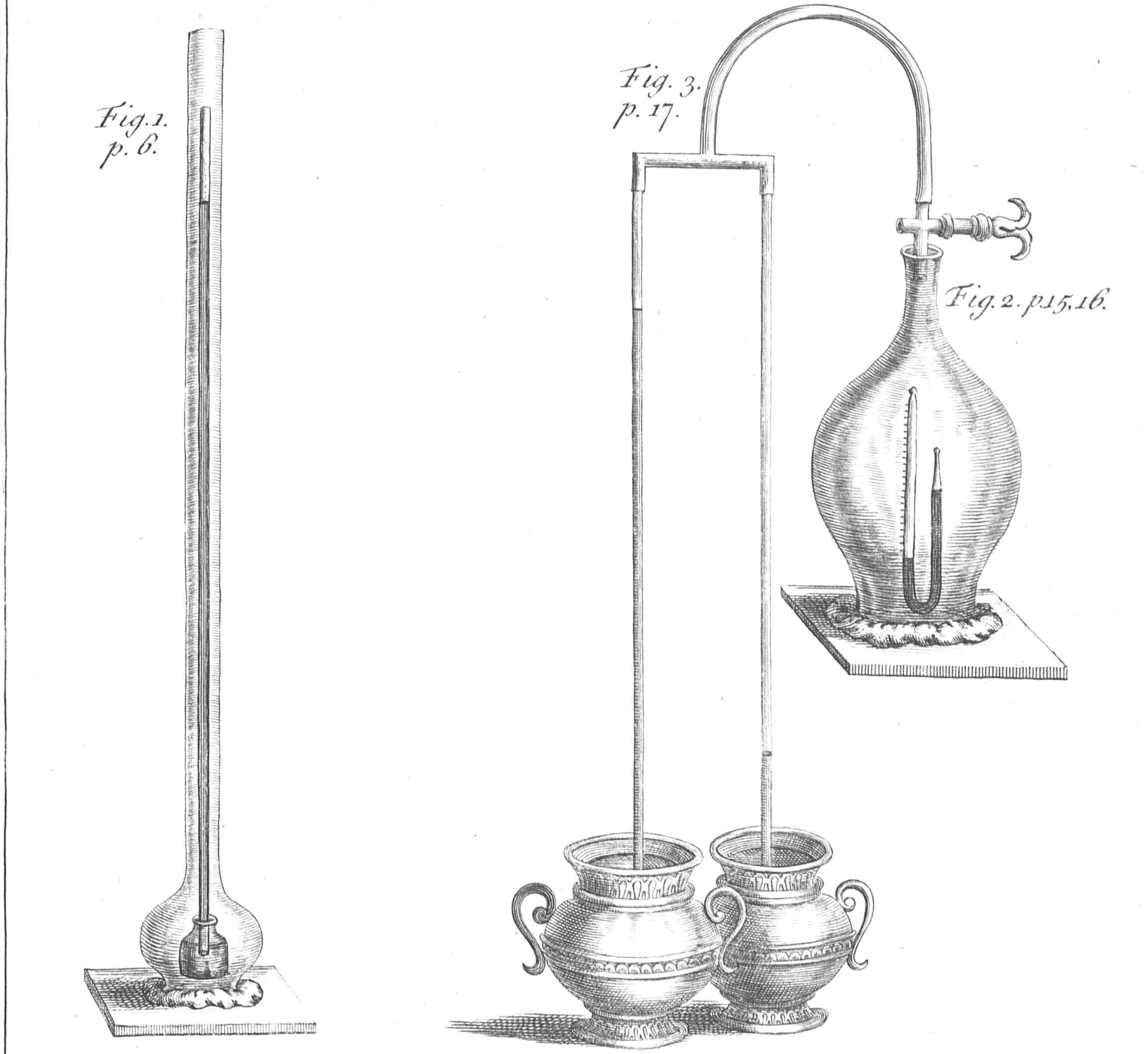

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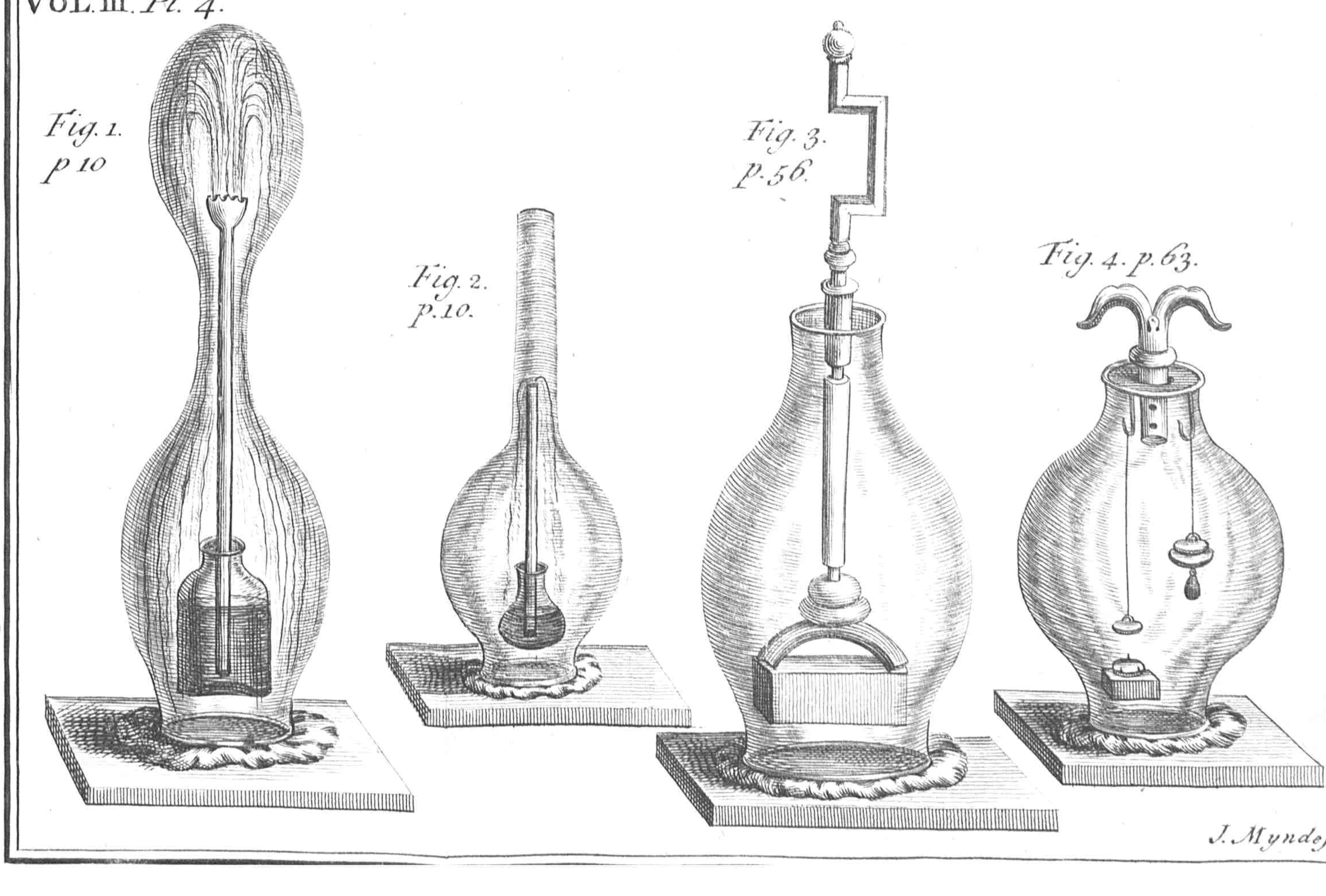

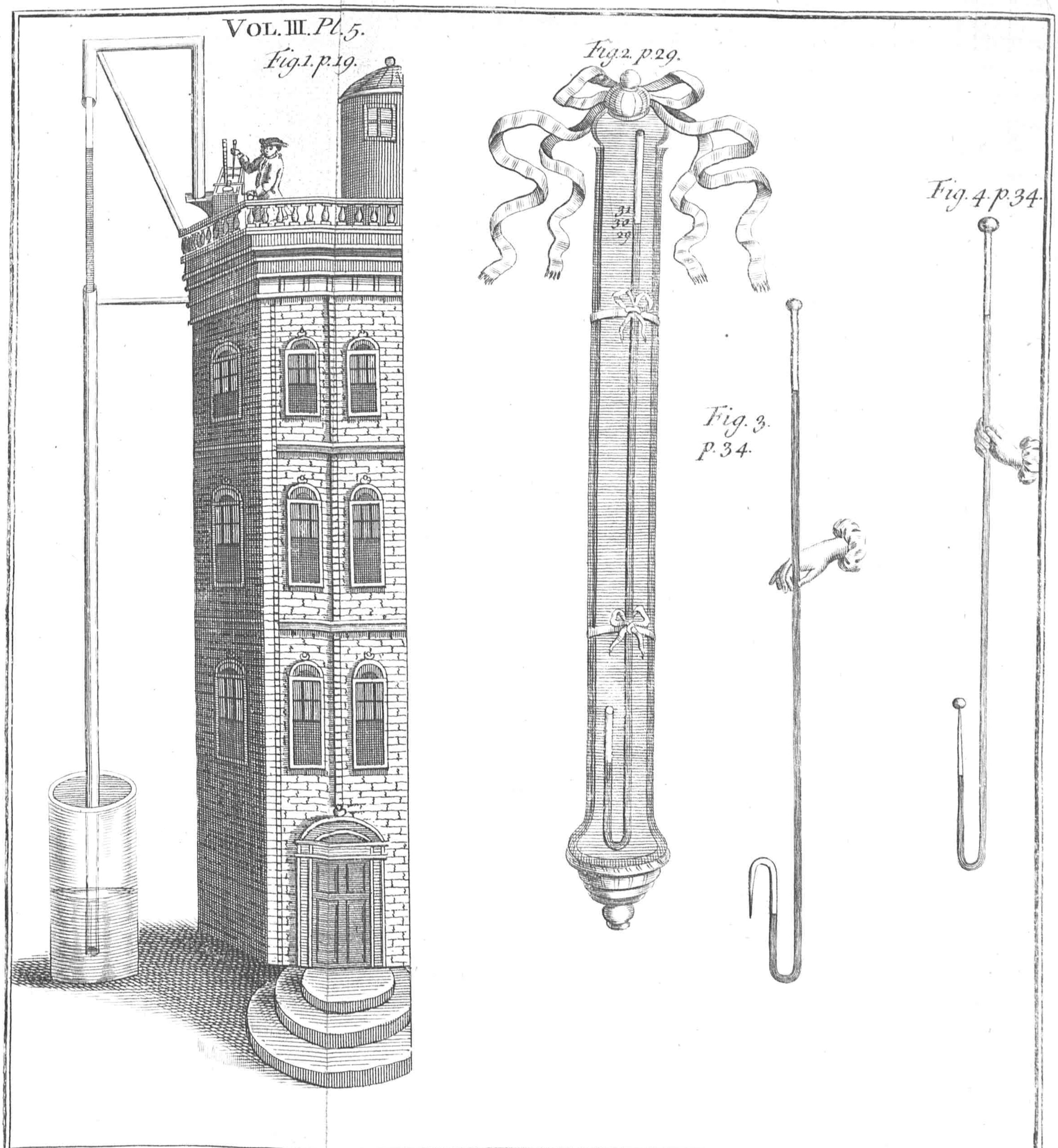
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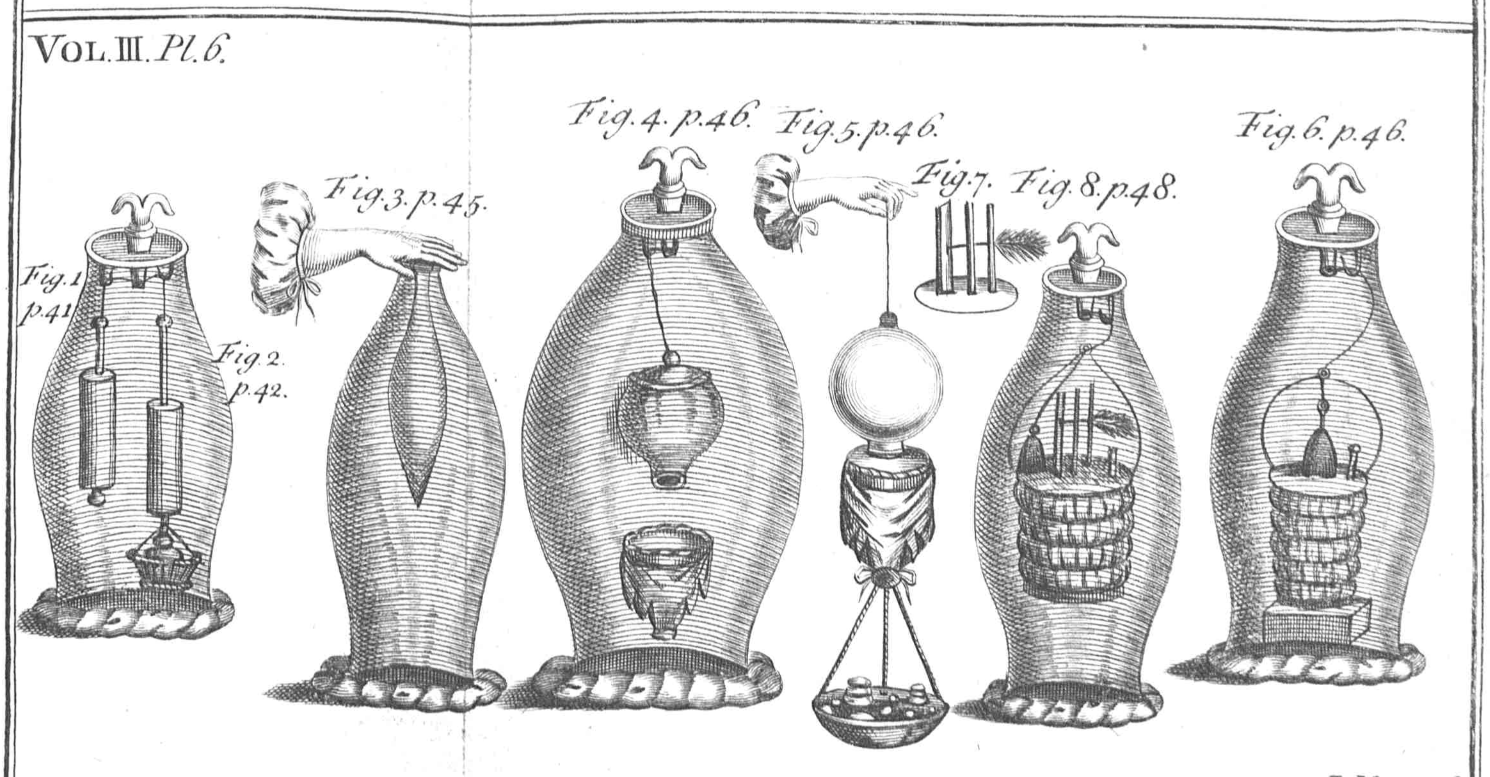
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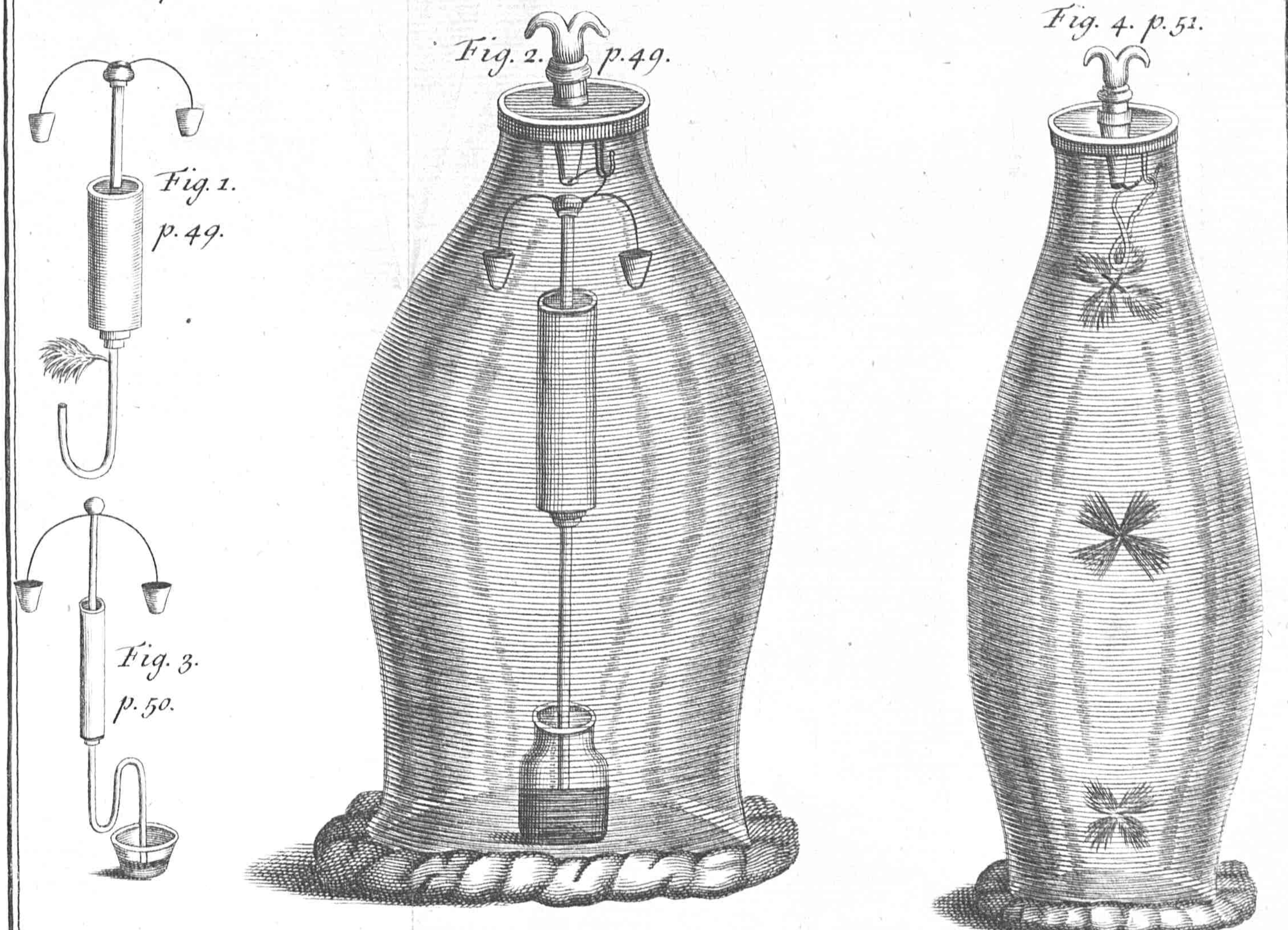

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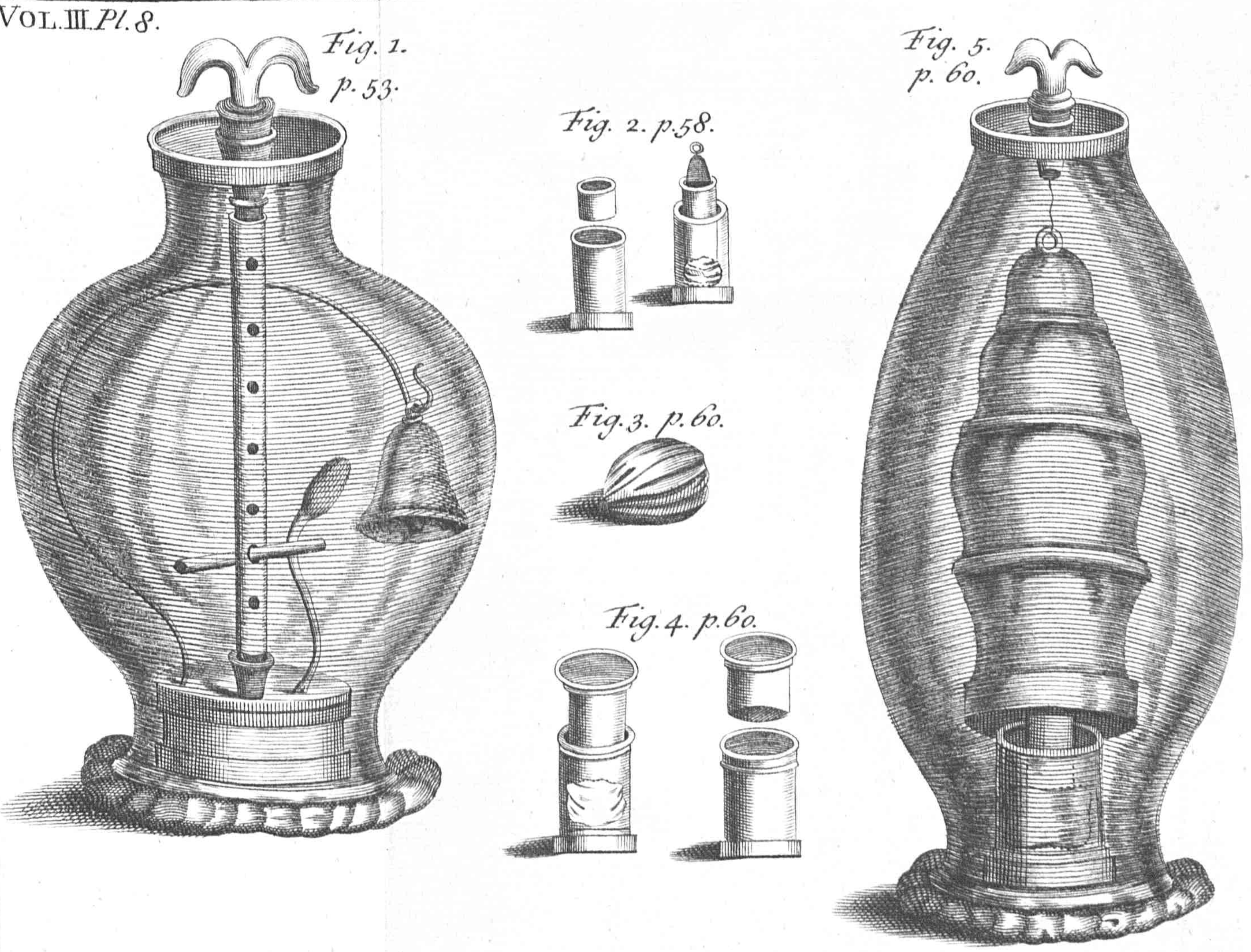

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