EXPERIMENTS

AND

OBSERVATIONS

ON

ELECTRICITY,

MADE AT

PHILADELPHIA in AMERICA,

BY

BENJAMIN FRANKLIN, L. L. D. and F. R. S.

To which are added,

LETTERS and PAPERS

ON

PHILOSOPHICAL SUBJECTS.

The Whole corrected, methodized, improved, and now first collected into one Volume,

AND

Illustrated with COPPER PLATES.

LONDON:

Printed for DAVID HENRY; and sold by FRANCIS NEWBERY, at the Corner of St. Paul's Church-Yard.

MDCCLXIX.

1769

National Oceanic and Atmospheric Administration

ERRATA NOTICE

One or more conditions of the original document may affect the quality of the image, such as:

Discolored pages
Faded or light ink
Binding intrudes into the text

This has been co-operative project between the NOAA Central Library and the Climate Database Modernization Program, National Climate Data Center (NCDC). To view the original document contact the NOAA Central Library in Silver Spring, MD at (301) 713-2607 x 124 or Library.Reference@noaa.gov.

EXPERIMENTS

AND

OBSERVATIONS

ON

ELECTRICITY,

MADE AT

PHILADELPHIA in AMERICA.

PREFACE

To the First Edition.

It may be necessary to acquaint the Reader, that the following observations and experiments were not drawn up with a view to their being made publick, but were communicated at different times, and most of them in letters wrote on various topicks, as matters only of private amusement.

But some persons to whom they were read, and who had themselves been conversant in electrical disquisitions, were of opinion, they contained so many curious and interesting particulars relative to this affair, that it would be doing a kind of injustice to the public, to confine them solely to the limits of a private acquaintance.

The Editor was therefore prevailed upon to commit such extracts of letters, and other detached pieces as were in his hands to the press, without waiting for the ingenious author's permission so to do; and this was done with the less hesitation, as it was apprehended the author's engagements in other affairs would scarce afford him leisure to give the publick his reflections and experiments on the subject, finished with that care and precision, of which the treatise before us shews he is alike studious and capable.

The experiments which our author relates are most of them peculiar to himself; they are conducted with judgment, and the inferences from them plain and conclusive; though sometimes proposed under the terms of suppositions and conjectures.

And indeed the scene he opens, strikes us with a pleasing astonishment, while he conducts us by a train of facts and judicious reflections, to a probable cause of those phænomena, which are at once the most awful, and, hitherto, accounted for with the least verisimilitude.

He

He exhibits to our consideration, an invisible, subtle matter, disseminated through all nature in various proportions, equally unobserved, and, whilst all those bodies to which it peculiarly adheres are alike charged with it, inoffensive.

He shews, however, that if an unequal distribution is by any means brought about; if there is a coacervation in one part of space, a less proportion, vacuity, or want, in another; by the near approach of a body capable of conducting the coacervated part to the emptier space, it becomes perhaps the most formidable and irresistible agent in the universe. Animals are in an instant struck breathless, bodies almost impervious by any force yet known, are perforated, and metals fused by it, in a moment.

From the similar effects of lightning and electricity, our author has been led to make some probable conjectures on the cause of the former; and, at the same time, to propose some rational experiments in order to secure ourselves, and those things on which its force is often directed, from its pernicious effects; a circumstance of no small importance to the publick, and therefore worthy of the utmost attention.

It has, indeed, been of late the fashion to ascribe every grand or unusual operation of nature, such as lightning and earthquakes, to electricity; not, as one would imagine from the manner of reasoning on these occasions, that the authors of these schemes have discovered any connection betwixt the cause and effect, or saw in what manner they were related; but, as it would seem, merely because they were unacquainted with any other agent, of which it could not positively be said the connection was impossible.

But of these, and many other interesting circumstances, the reader will be more satisfactorily informed in the following letters, to which he is therefore referred by

The EDITOR.

E R R A T A.

PAGE 13, *Line* 8, *for*, at top, *read*, at the top.
50,——15, *add*, becauſe the blood and other humours containing ſo much water, are more ready conductors.
62——10 *for* wrote, *read* written.
73,——10, *for* prevades, *read*, pervades.
96,—— 7, *from the bottom*: *add*, (page 54 of this Edit.)
127,——22, *for*, diſcoveries, you *read* diſcoveries, you.
134,—— 3, *for Lyden*, *read Leyden*.
141,——20, *for* give it a ſhock, *read*, give a ſhock.
143,——11, *for* experimen, *read*, experiment.
158,——22, *for* they, *read*, you.
189, —— 5, *for* infenſibly, *read*, inſenſibly.
199,——15, *for* fettle, *read* ſettle.
225,—— 3, *add*, See plate II.
327—— 7, *for* by frequent, *read*, by the frequent.
344,—— 3, *from the bottom*, *begin a new paragraph at* There.
345,—— 4, *from the bottom*, *begin a new paragraph at* How.
346——20, *dele the comma at* reproduced, *and put one at* ſeparation.
347,——11, *for* has the ſame, *read*, has nearly the ſame.
354,—— 2, *from the bottom*, *for* 280, *read* 180.
389——11, *add* See plate VI.
403,——15, *for* VI. *read* VII.
475,—— 2, *for* endeabour, *read* endeavour.
490,——18, *dele the ſemicolon between* warmer *and* ſituation.

In numbering of the pages, 112, 113, are repeated; as are alſo pages 465 to 472.

ADVERTISEMENT

Concerning this Fourth Edition.

ALL the Philosophical Letters and Papers of the same Author, that have been inserted at different Times in the Philosophical Transactions of the Royal Society, or in the Magazines, or printed in separate Pamphlets, are collected and added to this Edition; together with a Number of others on various Subjects, never before printed, that have passed between the Author and his Friends. Many Errors in the preceding Editions, are now corrected; some of the Letters, which had been transposed, are restored to their proper places; and sundry Passages are more fully explained by Notes.----There is also added, a compleat Index to the whole.

EXTRACT OF LETTER I.

FROM

BENJ. FRANKLIN, *Esq*; at *Philadelphia*,

TO

PETER COLLINSON, Esq; F. R. S. *London*.

SIR, *Philadelphia*, *March* 28, 1747.

YOUR kind present of an electric tube, with directions for using it, has put several of us on making electrical experiments, in which we have observed some particular phænomena that we look upon to be new. I shall, therefore communicate them to you in my next, though posſibly they may not be new to you, as among the numbers daily employed in thoſe experiments on your ſide the water, 'tis probable ſome one or other has hit on the ſame obſervations. For my own part, I never was before engaged in any ſtudy that ſo totally engroſſed my attention and my time as this has lately done; for what with making experiments when I can be alone, and repeating them to my Friends and Acquaintance, who, from the novelty of the thing, come continually in crouds to ſee them, I have, during ſome months paſt, had little leiſure for any thing elſe.

I am, &c.

B. FRANKLIN.

LET-

LETTER II.

FROM

Mr BENJ. FRANKLIN, in *Philadelphia*,

TO

PETER COLLINSON, Eſq; F. R. S. *London*.

S I R, *July* 11, 1747.

IN my laſt I informed you that, in purſuing our electrical enquiries, we had obſerved ſome particular Phænomena, which we looked upon to be new, and of which I promiſed to give you ſome account, though I apprehended they might poſſibly not be new to you, as ſo many hands are daily employed in electrical experiments on your ſide the water, ſome or other of which would probably hit on the ſame obſervations.

The firſt is the wonderful effect of pointed bodies, both in *drawing off* and *throwing off* the electrical fire. For example,

Place an iron ſhot of three or four inches diameter on the mouth of a clean dry glaſs bottle. By a fine ſilken thread from the cieling, right over the mouth of the bottle, ſuſpend a ſmall cork-ball, about the bigneſs of a marble; the thread

thread of ſuch a length, as that the cork-ball may reſt againſt the ſide of the ſhot. Electrify the ſhot, and the ball will be repelled to the diſtance of four or five inches, more or leſs, according to the quantity of Electricity.—When in this ſtate, if you preſent to the ſhot the point of a long ſlender ſharp bodkin, at ſix or eight inches diſtance, the repellency is inſtantly deſtroy'd, and the cork flies to the ſhot. A blunt body muſt be brought within an inch, and draw a ſpark, to produce the ſame effect. To prove that the electrical fire is *drawn off* by the point, if you take the blade of the bodkin out of the wooden handle, and fix it in a ſtick of ſealing-wax, and then preſent it at the diſtance aforeſaid, or if you bring it very near, no ſuch effect follows; but ſliding one finger along the wax till you touch the blade, and the ball flies to the ſhot immediately.—If you preſent the point in the dark, you will ſee, ſometimes at a foot diſtance, and more, a light gather upon it, like that of a fire-fly, or glow-worm; the leſs ſharp the point, the nearer you muſt bring it to obſerve the light; and at whatever diſtance you ſee the light, you may draw off the electrical fire, and deſtroy the repellency.—If a cork-ball ſo ſuſpended be repelled by the tube, and a point be preſented quick to it, tho' at a conſiderable diſtance, 'tis ſurprizing to ſee how ſuddenly it flies back to the tube. Points of wood will do near as well as thoſe of iron, provided the wood is not dry; for perfectly dry wood will no more conduct Electricity than ſealing-wax.

To

To ſhew that points will *throw off* * as well as *draw off* the electrical fire ; lay a long ſharp needle upon the ſhot, and you cannot electriſe the ſhot, ſo as to make it repel the cork-ball †.—Or fix a needle to the end of a ſuſpended gun-barrel, or iron-rod, ſo as to point beyond it like a little bayonet ; and while it remains there, the gun-barrel, or rod, cannot by applying the tube to the other end be electriſed ſo as to give a ſpark, the fire continually running out ſilently at the point. In the dark you may ſee it make the ſame appearance as it does in the caſe before-mentioned.

The repellency between the cork-ball and the ſhot is likewiſe deſtroy'd. 1. By ſifting fine ſand on it ; this does it gradually. 2. By breathing on it. 3. By making a ſmoke about it from burning wood ‡. 4. By candle light, even though the candle is at a foot diſtance : theſe do it ſuddenly.—The light of a bright coal from a wood fire ; and

* This power of points to *throw off* the electrical fire, was firſt communicated to me by my ingenious friend Mr *Thomas Hopkinſon*, ſince deceaſed, whoſe virtue and integrity, in every ſtation of life, public and private, will ever make his Memory dear to thoſe who knew him, and knew how to value him.

† This was Mr *Hopkinſon*'s Experiment, made with an expectation of drawing a more ſharp and powerful ſpark from the point, as from a kind of focus, and he was ſurprized to find little or none.

‡ We ſuppoſe every particle of ſand, moiſture, or ſmoke, being firſt attracted and then repelled, carries off with it a portion of the electrical fire ; but that the ſame ſtill ſubſiſts in thoſe particles, till they communicate it to ſomething elſe, and that it is never really deſtroyed.——So when water is thrown on common fire, we do not imagine the element is thereby deſtroyed or annihilated, but only diſperſed, each particle of water carrying off in vapour its portion of the fire, which it had attracted and attached to itſelf.

the

the light of red-hot iron do it likewiſe; but not at ſo great a diſtance. Smoke from dry roſin dropt on hot iron, does not deſtroy the repellency; but is attracted by both ſhot and cork-ball, forming proportionable atmoſpheres round them, making them look beautifully, ſomewhat like ſome of the figures in *Burnet*'s or *Whiſton*'s theory of the earth.

N. B. This experiment ſhould be made in a cloſet, where the air is very ſtill, or it will be apt to fail.

The light of the ſun thrown ſtrongly on both cork and ſhot by a looking-glaſs for a long time together, does not impair the repellency in the leaſt. This difference between fire-light and ſun-light is another thing that ſeems new and extraordinary to us *.

We had for ſome time been of opinion, that the electrical fire was not created by friction, but collected, being really an element diffus'd among, and attracted by other matter, particularly by water and metals. We had even diſcovered and demonſtrated its afflux to the electrical ſphere, as well as its efflux, by means of little light windmill wheels made of ſtiff paper vanes, fixed obliquely and turning freely on fine wire axes. Alſo by little wheels of the ſame matter, but formed like water-wheels. Of the

* This different Effect probably did not ariſe from any difference in the light, but rather from the particles ſeparated from the candle, being firſt attracted and then repelled, carrying off the electric matter with them; and from the rarefying the air, between the glowing coal or red-hot iron, and the electriſed ſhot, through which rarified air the electric fluid could more readily paſs.

difdiſpoſition and application of which wheels, and the various phænomena reſulting, I could, if I had time, fill you a ſheet †. The impoſſibility of electriſing one's ſelf (though ſtanding on wax) by rubbing the tube, and drawing the fire from it; and the manner of doing it, by paſſing the tube near a perſon or thing ſtanding on the floor, *&c.* had alſo occurred to us ſome months before Mr *Watſon*'s ingenious *Sequel* came to hand, and theſe were ſome of the new things I intended to have communicated to you.— But now I need only mention ſome particulars not hinted in that piece, with our reaſonings thereupon; though perhaps the latter might well enough be ſpared.

1. A perſon ſtanding on wax, and rubbing the tube, and another perſon on wax drawing the fire, they will both of them, (provided they do not ſtand ſo as to touch one another) appear to be electriſed, to a perſon ſtanding on the floor; that is, he will perceive a ſpark on approaching each of them with his knuckle.

2. But if the perſons on wax touch one another during the exciting of the tube, neither of them will appear to be electriſed.

3. If they touch one another after exciting the tube, and drawing the fire as aforeſaid, there will be a ſtronger

† Theſe experiments with the wheels were made and communicated to me by my worthy and ingenious friend Mr *Philip Syng*; but we afterwards diſcovered that the motion of thoſe wheels was not owing to any afflux or efflux of the electric fluid, but to various circumſtances of attraction and repulſion. 1750.

ſpark

ſpark between them, than was between either of them and the perſon on the floor.

4. After ſuch ſtrong ſpark, neither of them diſcover any electricity.

Theſe appearances we attempt to account for thus: We ſuppoſe, as aforeſaid, that electrical fire is a common element, of which every one of the three perſons abovementioned has his equal ſhare, before any operation is begun with the tube. *A*, who ſtands on wax and rubs the tube, collects the electrical fire from himſelf into the glaſs; and his communication with the common ſtock being cut off by the wax, his body is not again immediately ſupply'd. *B*, (who ſtands on wax likewiſe) paſſing his knuckle along near the tube, receives the fire which was collected by the glaſs from *A*; and his communication with the common ſtock being likewiſe cut off, he retains the additional quantity received.—To *C*, ſtanding on the floor, both appear to be electriſed: for he having only the middle quantity of electrical fire, receives a ſpark upon approaching *B*, who has an over quantity; but gives one to *A*, who has an under quantity. If *A* and *B* approach to touch each other, the ſpark is ſtronger, becauſe the difference between them is greater: After ſuch touch there is no ſpark between either of them and *C*, becauſe the electrical fire in all is reduced to the original equality. If they touch while electriſing, the equality is never deſtroy'd, the fire only circulating. Hence have ariſen ſome new terms among us: we ſay, *B*, (and bodies like circumſtanced) is electriſed

poſitively;

positively; *A*, *negatively*. Or rather, *B* is electrised *plus*; *A*, *minus*. And we daily in our experiments electrise bodies *plus* or *minus*, as we think proper.—To electrise *plus* or *minus*, no more needs to be known than this, that the parts of the tube or sphere that are rubbed, do, in the instant of the friction, attract the electrical fire; and therefore take it from the thing rubbing: the same parts immediately, as the friction upon them ceases, are disposed to give the fire they have received, to any body that has less. Thus you may circulate it, as Mr *Watson* has shewn; you may also accumulate or subtract it upon, or from any body, as you connect that body with the rubber or with the receiver, the communication with the common stock being cut off. We think that ingenious gentleman was deceived when he imagined (in his *Sequel*) that the electrical fire came down the wire from the cieling to the gun-barrel, thence to the sphere, and so electrised the machine and the man turning the wheel, *&c.* We suppose it was *driven off*, and not brought on through that wire; and that the machine and man, *&c.* were electrised *minus*; *i. e.* had less electrical fire in them than things in common.

As the vessel is just upon sailing, I cannot give you so large an account of *American* Electricity as I intended: I shall only mention a few particulars more.—We find granulated lead better to fill the phial with, than water, being easily warmed, and keeping warm and dry in damp air.—We fire spirits with the wire of the phial.—We light candles, just blown out, by drawing a spark among the ſmoke between the wire and ſnuffers.—We repreſent lightning, by paſſing the wire in the dark, over a china plate that has gilt flowers, or applying it to gilt frames of looking-glaſſes, *&c.* We electriſe a perſon twenty or more times running, with a touch of the finger on the wire, thus: He ſtands on wax. Give him the electriſed bottle in his hand. Touch the wire with your finger, and then touch his hand or face; there are ſparks every time *.—We increaſe the force of the electrical kiſs vaſtly, thus: Let *A* and *B* ſtand on wax; or *A* on wax, and *B* on the floor; give one of them the electriſed phial in hand; let the other take hold of the wire; there will be a ſmall ſpark; but when their lips approach, they will be ſtruck and ſhock'd. The ſame if another gentleman and lady, *C* and *D*, ſtanding alſo on wax, and joining hands with *A* and *B*, ſalute or ſhake hands. We ſuſpend by fine ſilk thread a counterfeit ſpider, made of a ſmall piece of burnt cork, with legs of linnen thread, and a grain or two of lead ſtuck in him, to give him more weight. Upon the table over which he hangs, we ſtick a wire upright, as high as the phial and wire, two or three inches from the ſpider: then we animate him, by ſetting the electrified phial at the ſame diſtance on the other ſide of him; he will immediately fly to the wire of the phial, bend his legs

* By taking a ſpark from the wire, the electricity within the bottle is diminiſhed; the outſide of the bottle then draws ſome from the perſon holding it, and leaves him in the negative ſtate. Then when his hand or face is touch'd, an equal quantity is reſtored to him from the perſon touching.

in

in touching it; then ſpring off, and fly to the wire in the table; thence again to the wire of the phial, playing with his legs againſt both, in a very entertaining manner, appearing perfectly alive to perſons unacquainted. He will continue this motion an hour or more in dry weather.—We electrify, upon wax in the dark, a book that has a double line of gold round upon the covers, and then apply a knuckle to the gilding; the fire appears every where upon the gold like a flaſh of lightning: not upon the leather, nor, if you touch the leather inſtead of the gold. We rub our tubes with buckſkin, and obſerve always to keep the ſame ſide to the tube, and never to ſully the tube by handling; thus they work readily and eaſily, without the leaſt fatigue, eſpecially if kept in tight paſteboard caſes, lined with flannel, and fitting cloſe to the tube *. This I mention becauſe the *European* papers on Electricity, frequently ſpeak of rubbing the tube, as a fatiguing exerciſe. Our ſpheres are fixed on iron axes, which paſs through them. At one end of the axis there is a ſmall handle, with which you turn the ſphere like a common grindſtone. This we find very commodious, as the machine takes up but little room, is portable, and may be encloſed in a tight box, when not in uſe. 'Tis true, the ſphere does not turn ſo ſwift as when the great wheel is uſed: but ſwiftneſs we think of little importance, ſince a few turns will charge the phial, *&c.* ſufficiently †. *I am, &c.* B. FRANKLIN.

* Our tubes are made here of green glaſs, 27 or 30 inches long, as big as can be graſped,

† This ſimple eaſily-made machine was a contrivance of Mr *Syng*'s.

LETTER III.

FROM

BENJ. FRANKLIN, *Esq*; at *Philadelphia*,

TO

PETER COLLINSON, F.R.S. *London*.

SIR, *Sept.* 1, 1747.

THE neceſſary trouble of copying long letters, which, perhaps, when they come to your hands, may contain nothing new, or worth your reading, (ſo quick is the progreſs made with you in Electricity) half diſcourages me from writing any more on that ſubject. Yet I cannot forbear adding a few obſervations on M. *Muſchenbroek*'s wonderful bottle.

1. The non-electric contain'd in the bottle differs when electriſed from a non-electric electriſed out of the bottle, in this: that the electrical fire of the latter is accumulated *on its ſurface*, and forms an electrical atmoſphere round it of con-

confiderable extent; but the electrical fire is crowded *into the fubftance* of the former, the glafs confining it *

2. At the fame time that the wire and top of the bottle, &c. is electrifed *pofitively* or *plus*, the bottom of the bottle is electrifed *negatively* or *minus*, in exact proportion: *i. e.* whatever quantity of electrical fire is thrown in at top, an equal quantity goes out of the bottom †. To underftand this, fuppofe the common quantity of electricity in each part of the bottle, before the operation begins, is equal to 20; and at every ftroke of the tube, fuppofe a quantity equal to 1 is thrown in; then, after the firft ftroke, the quantity contain'd in the wire and upper part of the bottle will be 21, in the bottom 19. After the fecond, the upper part will have 22, the lower 18, and fo on, till, after 20 ftrokes, the upper part will have a quantity of electrical fire equal to 40, the lower part none: and then the operation ends: for no more can be thrown into the upper part, when no more can be driven out of the lower part. If you attempt to throw more in, it is fpued back through the wire, or flies out in loud cracks through the fides of the bottle.

3. The equilibrium cannot be reftored in the bottle by *inward* communication or contact of the parts; but it muft be done by a communication form'd *without* the bottle

* See this opinion rectified in Letter IV. § 16 and 17. The fire in the bottle was found by fubfequent experiments not to be contained in the non-electric, but *in the glafs*. 1748.

† What is faid here, and after, of the *top* and *bottom* of the bottle, is true of the *infide* and *outfide* furfaces, and fhould have been fo expreffed.

bottle between the top and bottom, by ſome non-electric, touching or approaching both at the ſame time; in which caſe it is reſtored with a violence and quickneſs inexpreſſible; or, touching each alternately, in which caſe the equilibrium is reſtored by degrees.

4. As no more electrical fire can be thrown into the top of the bottle, when all is driven out of the bottom, ſo in a bottle not yet electriſed, none can be thrown into the top, when none *can* get out at the bottom; which happens either when the bottom is too thick, or when the bottle is placed on an electric *per ſe*. Again, when the bottle is electriſed, but little of the electrical fire can be *drawn out* from the top, by touching the wire, unleſs an equal quantity can at the ſame time *get in* at the bottom *. Thus, place an electriſed bottle on clean glaſs or dry wax, and you will not, by touching the wire, get out the fire from the top. Place it on a non-electric, and touch the wire, you will get it out in a ſhort time; but ſooneſt when you form a direct communication as above.

So wonderfully are theſe two ſtates of Electricity, the *plus* and *minus*, combined and balanced in this miraculous bottle! ſituated and related to each other in a manner that I can by no means comprehend! If it were poſſible that a bottle ſhould in one part contain a quantity of air ſtrongly compreſt, and in another part a perfect vacuum, we know the equilibrium would be inſtantly reſtored *within*. But

* See the preceding note, relating to *top* and *bottom*.

here

here we have a bottle containing at the ſame time a *plenum* of electrical fire, and a *vacuum* of the ſame fire; and yet the equilibrium cannot be reſtored between them but by a communication *without!* though the *plenum* preſſes violently to expand, and the hungry vacuum ſeems to attract as violently in order to be filled.

5. The ſhock to the nerves (or convulſion rather) is occaſioned by the ſudden paſſing of the fire through the body in its way from the top to the bottom of the bottle. The fire takes the ſhorteſt courſe, as Mr *Watſon* juſtly obſerves: But it does not appear from experiment that in order for a perſon to be ſhocked, a communication with the floor is neceſſary: for he that holds the bottle with one hand, and touches the wire with the other, will be ſhock'd as much, though his ſhoes be dry, or even ſtanding on wax, as otherwiſe. And on the touch of the wire (or of the gun-barrel, which is the ſame thing) the fire does not proceed from the touching finger to the wire, as is ſuppoſed, but from the wire to the finger, and paſſes through the body to the other hand, and ſo into the bottom of the bottle.

EXPERIMENTS *confirming the above.*

EXPERIMENT I.

Place an electriſed phial on wax; a ſmall cork-ball ſuſpended by a dry ſilk-thread held in your hand, and brought

brought near to the wire, will firſt be attracted, and then repelled: when in this ſtate of repellency, ſink your hand, that the ball may be brought towards the bottom of the bottle; it will be there inſtantly and ſtrongly attracted, 'till it has parted with its fire.

If the bottle had a *poſitive* electrical atmoſphere, as well as the wire, an electrified cork would be repelled from one as well as from the other.

EXPERIMENT II.

Fig. 1. From a bent wire (*a*) ſticking in the table, let a ſmall linen thread (*b*) hang down within half an inch of the electriſed phial (*c*). Touch the wire of the phial repeatedly with your finger, and at every touch you will ſee the thread inſtantly attracted by the bottle. (This is beſt done by a vinegar cruet, or ſome ſuch belly'd bottle.) As ſoon as you draw any fire out from the upper part, by touching the wire, the lower part of the bottle draws an equal quantity in by the thread.

EXPERIMENT III.

Fig. 2. Fix a wire in the lead, with which the bottom of the bottle is armed (*d*) ſo as that bending upwards, its ring-end may be level with the top or ring-end of the wire in the cork (*e*), and at three or four inches diſtance. Then electricise the bottle, and place it on wax. If a cork ſuſpended by a ſilk thread (*f*) hang between theſe two wires, it will play inceſſantly from one to the other, 'till the bottle

is

Plate. 1.

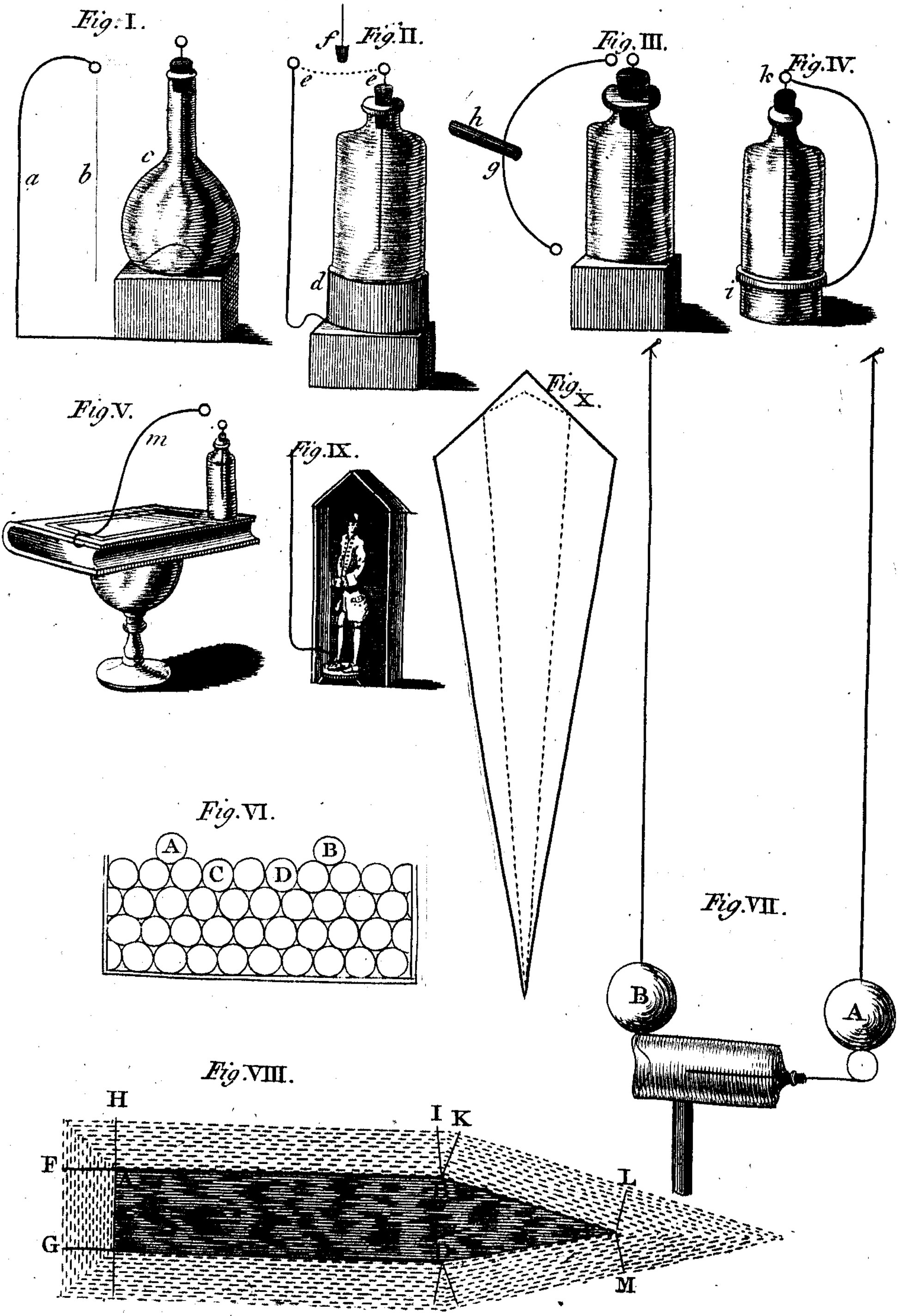


Fig. I.
a
b
c
Fig. II.
f
e
e
d
h
g
Fig. III.
Fig. IV.
k
i
Fig. V.
m
Fig. IX.
Fig. X.
Fig. VI.
A
B
C
D
Fig. VII.
B
A
Fig. VIII.
H
I
K
F
L
G
M

is no longer electriſed; that is, it fetches and carries fire from the top to the bottom* of the bottle, 'till the equilibrium is reſtored.

EXPERIMENT IV.

FIG. 3. Place an electriſed phial on wax; take a wire (*g*) in form of a *C*, the ends at ſuch a diſtance when bent, as that the upper may touch the wire of the bottle, when the lower touches the bottom: ſtick the outer part on a ſtick of ſealing-wax (*h*), which will ſerve as a handle; then apply the lower end to the bottom of the bottle, and gradually bring the upper end near the wire in the cork. The conſequence is, ſpark follows ſpark till the equilibrium is reſtored. Touch the top firſt, and on approaching the bottom with the other end, you have a conſtant ſtream of fire from the wire entering the bottle. Touch the top and bottom together, and the equilibrium will inſtantly be reſtored; the crooked wire forming the communication.

EXPERIMENT V.

FIG. 4. Let a ring of thin lead, or paper, ſurround a bottle (*i*) even at ſome diſtance from or above the bottom. From that ring let a wire proceed up, till it touch the wire of the cork (*k*). A bottle ſo fixt cannot by any means be electriſed: the equilibrium is never deſtroyed: for

* *i. e.* from the inſide to the outſide.

while the communication between the upper and lower parts of the bottle is continued by the outside wire, the fire only circulates: what is driven out at bottom, is constantly supply'd from the top †. Hence a bottle cannot be electrised that is foul or moist on the outside, if such moisture continue up to the cork or wire.

EXPERIMENT VI.

Place a man on a cake of wax, and present him the wire of the electrified phial to touch, you standing on the floor, and holding it in your hand. As often as he touches it, he will be electrified *plus*; and any one standing on the floor may draw a spark from him. The fire in this experiment passes out of the wire into him; and at the same time out of your hand into the bottom of the bottle.

EXPERIMENT VII.

Give him the electrical phial to hold; and do you touch the wire; as often as you touch it he will be electrified *minus*, and may draw a spark from any one standing on the floor. The fire now passes from the wire to you, and from him into the bottom of the bottle.

EXPERIMENT VIII.

Lay two books on two glasses, back towards back, two or three inches distant. Set the electrified phial on one, and then touch the wire; that book will be electrified

* See the preceding note.

minus

minus; the electrical fire being drawn out of it by the bottom of the bottle. Take off the bottle, and holding it in your hand, touch the other with the wire; that book will be electriſed *plus*; the fire paſſing into it from the wire, and the bottle at the ſame time ſupplied from your hand. A ſuſpended ſmall cork-ball will play between theſe books 'till the equilibrium is reſtored.

EXPERIMENT IX.

When a body is electriſed *plus*, it will repel an electrified feather or ſmall cork-ball. When *minus* (or when in the common ſtate) it will attract them, but ſtronger when *minus* than when in the common ſtate, the difference being greater.

EXPERIMENT X.

Though, as in *Experiment* VI. a man ſtanding on wax may be electriſed a number of times by repeatedly touching the wire of an electriſed bottle (held in the hand of one ſtanding on the floor) he receiving the fire from the wire each time: yet holding it in his own hand, and touching the wire, though he draws a ſtrong ſpark, and is violently ſhocked, no Electricity remains in him; the fire only paſſing through him, from the upper to the lower part of the bottle. Obſerve, before the ſhock, to let ſome one on the floor touch him to reſtore the equilibrium in his body; for in taking hold of the bottom of the bottle, he ſometimes becomes a little electriſed *minus*, which will continue after the ſhock, as would alſo any *plus* Electricity, which he he might have given him before the ſhock. For, reſtoring the equilibrium in the bottle, does not at all affect the Electricity in the man through whom the fire paſſes; that Electricity is neither increaſed nor diminiſhed.

EXPERIMENT XI.

The paſſing of the electrical fire from the upper to the lower part* of the bottle, to reſtore the equilibrium, is rendered ſtrongly viſible by the following pretty experiment. Take a book whoſe covering is filletted with gold; bend a wire of eight or ten inches long, in the form of (*m*) Fig. 5. ſlip it on the end of the cover of the book, over the gold line, ſo as that the ſhoulder of it may preſs upon one end of the gold line, the ring up, but leaning towards the other end of the book. Lay the book on a glaſs or wax, and on the other end of the gold lines ſet the bottle electriſed; then bend the ſpringing wire, by preſſing it with a ſtick of wax till its ring approaches the ring of the bottle wire, inſtantly there is a ſtrong ſpark and ſtroke, and the whole line of gold, which completes the communication, between the top and bottom of the bottle, will appear a vivid flame, like the ſharpeſt lightning. The cloſer the contact between the ſhoulder of the wire, and the gold at one end of the line, and between the bottom of the bottle and the gold at the other end, the better the experiment ſucceeds. The room ſhould be darkened. If you would

* *i. e.* from the *inſide* to the *outſide*.

have

have the whole filletting round the cover appear in fire at once, let the bottle and wire touch the gold in the diagonally opposite corners.

I am, &c.

B. FRANKLIN.

LETTER IV.

FROM

BENJ. FRANKLIN, *Esq;* in *Philadelphia*,

TO

PETER COLLINSON, Esq; F. R. S. *London.*

Farther EXPERIMENTS *and* OBSERVATIONS *in* ELECTRICITY.

SIR, 1748.

§ 1. THERE will be the same explosion and shock if the electrified phial is held in one hand by the hook, and the coating touch'd with the other, as when held by the coating, and touch'd at the hook.

2. To

2. To take the charg'd phial safely by the hook, and not at the same time diminish its force, it must first be set down on an electric *per se*.

3. The phial will be electrified as strongly, if held by the hook, and the coating apply'd to the globe or tube; as when held by the coating, and the hook apply'd *.

4. But the *direction* of the electrical fire being different in the charging, will also be different in the explosion. The bottle charged through the hook, will be discharged through the hook; the bottle charged through the coating, will be discharged through the coating, and not otherways; for the fire must come out the same way it went in.

5. To prove this, take two bottles that were equally charged through the hooks, one in each hand: bring their hooks near each other, and no spark or shock will follow; because each hook is disposed to give fire, and neither to receive it. Set one of the bottles down on glass, take it up by the hook, and apply its coating to the hook of the other; then there will be an explosion and shock, and both bottles will be discharged.

6. Vary the experiment, by charging two phials equally, one through the hook, the other through the coating: hold that by the coating which was charged through the hook; and that by the hook which was charged through the coating: apply the hook of the first to the coating of the

* This was a Discovery of the very ingenious Mr *Kinnersley's*, and by him communicated to me.

other,

other, and there will be no ſhock or ſpark. Set that down on glaſs which you held by the hook, take it up by the coating, and bring the two hooks together: a ſpark and ſhock will follow, and both phials be diſcharged.

In this experiment the bottles are totally diſcharged, or the equilibrium within them reſtored. The *abounding* of fire in one of the hooks (or rather in the internal ſurface of one bottle (being exactly equal to the *wanting* of the other: and therefore, as each bottle has in itſelf the *abounding* as well as the *wanting*, the wanting and abounding muſt be equal in each bottle. See §. 8, 9, 10, 11. But if a man holds in his hands two bottles, one fully electrified, the other not at all, and brings their hooks together, he has but half a ſhock, and the bottles will both remain half electrified, the one being half diſcharged, and the other half charged.

7. Place two phials equally charged on a table at five or ſix inches diſtance. Let a cork-ball, ſuſpended by a ſilk thread, hang between them. If the phials were both charged through their hooks, the cork, when it has been attracted and repelled by the one, will not be attracted, but equally repelled by the other. But if the phials were charged, the one through the hook, and the other * through the coating, the ball, when it is repelled from one hook,

* To charge a bottle commodiouſly through the coating, place it on a glaſs ſtand; form a communication from the prime conductor to the coating, and another from the hook to the wall or floor. When it is charged, remove the latter communication before you take hold of the bottle, otherwiſe great part of the fire will eſcape by it.

will

be as ſtrongly attracted by the other, and play vigorouſly between them, till both phials are nearly diſcharged.

8. When we uſe the terms of *charging* and *diſcharging* the phial, it is in compliance with cuſtom, and for want of others more ſuitable. Since we are of opinion that there is really no more electrical fire in the phial after what is called its *charging*, than before, nor leſs after its *diſcharging*; excepting only the ſmall ſpark that might be given to, and taken from the non-electric matter, if ſeparated from the bottle, which ſpark may not be equal to a five hundredth part of what is called the exploſion.

For if, on the exploſion, the electrical fire came out of the bottle by one part, and did not enter in again by another, then, if a man, ſtanding on wax, and holding the bottle in one hand, takes the ſpark by touching the wire hook with the other, the bottle being thereby *diſcharged*, the man would be *charged*; or whatever fire was loſt by one, would be found in the other, ſince there was no way for its eſcape: But the contrary is true.

9. Beſides, the phial will not ſuffer what is called a *charging*, unleſs as much fire can go out of it one way, as is thrown in by another. A phial cannot be charged ſtanding on wax or glaſs, or hanging on the prime conductor, unleſs a communication be formed between its coating and the floor.

10. But ſuſpend two or more phials on the prime conductor, one hanging to the tail of the other; and a wire from the laſt to the floor, an equal number of turns of the

wheel

wheel ſhall charge them all equally, and every one as much as one alone would have been. What is driven out at the tail of the firſt, ſerving to charge the ſecond; what is driven out of the ſecond charging the third; and ſo on. By this means a great number of bottles might be charged with the ſame labour, and equally high, with one alone, were it not that every bottle receives new fire, and loſes its old with ſome reluctance, or rather gives ſome ſmall reſiſtance to the charging, which in a number of botttles becomes more equal to the charging power, and ſo repels the fire back again on the globe, ſooner than a ſingle bottle would do.

11. When a bottle is charged in the common way, its *inſide* and *outſide* ſurfaces ſtand ready, the one to give fire by the hook, the other to receive it by the coating; the one is full, and ready to throw out, the other empty and extremely hungry; yet as the firſt will not *give out*, unleſs the other can at the ſame inſtant *receive in*; ſo neither will the latter receive in, unleſs the firſt can at the ſame inſtant give out. When both can be done at once, it is done with inconceivable quickneſs and violence.

12. So a ſtrait ſpring (though the compariſon does not agree in every particular) when forcibly bent, muſt, to reſtore itſelf, contract that ſide which in the bending was extended, and extend that which was contracted; if either of theſe two operations be hindered, the other cannot be done. But the ſpring is not ſaid to be *charg'd* with elaſticity

city when bent, and discharged when unbent; its quantity of elasticity is always the same.

13. Glass, in like manner, has, within its substance, always the same quantity of electrical fire, and that a very great quantity in proportion to the mass of glass, as shall be shewn hereafter.

14. This quantity, proportioned to the glass, it strongly and obstinately retains, and will have neither more nor less though it will suffer a change to be made in its parts and situation; *i. e.* we may take away part of it from one of the sides, provided we throw an equal quantity into the other.

15. Yet when the situation of the electrical fire is thus altered in the glass; when some has been taken from one side, and some added to the other, it will not be at rest or in its natural state, till it is restored to its original equality.—And this restitution cannot be made through the substance of the glass, but must be done by a non-electric communication formed without, from surface to surface.

16. Thus, the whole force of the bottle, and power of giving a shock, is in the GLASS ITSELF; the non-electrics in contact with the two surfaces, serving only to *give* and *receive* to and from the several parts of the glass; that is, to give on one side, and take away from the other.

17. This was discovered here in the following manner: Purposing to analyse the electrified bottle, in order to find wherein its strength lay, we placed it on glass, and drew out the cork and wire which for that purpose had been loose-

loosely put in. Then taking the bottle in one hand, and bringing a finger of the other near its mouth, a strong spark came from the water, and the shock was as violent as if the wire had remained in it, which shewed that the force did not lie in the wire. Then to find if it resided in the water, being crouded into and condensed in it, as confin'd by the glass, which had been our former opinion, we electrified the bottle again, and placing it on glass, drew out the wire and cork as before; then taking up the bottle, we decanted all its water into an empty bottle, which likewise stood on glass; and taking up that other bottle, we expected, if the force resided in the water, to find a shock from it; but there was none. We judged then that it must either be lost in decanting, or remain in the first bottle. The latter we found to be true; for that bottle on trial gave the shock, though filled up as it stood with fresh unelectrified water from a tea-pot.—To find, then, whether glass had this property merely as glass, or whether the form contributed any thing to it; we took a pane of sash-glass, and laying it on the hand, placed a plate of lead on its upper surface; then electrified that plate, and bringing a finger to it, there was a spark and shock. We then took two plates of lead of equal dimensions, but less than the glass by two inches every way, and electrified the glass between them, by electrifying the uppermost lead; then separated the glass from the lead, in doing which, what little fire might be in the lead was taken out, and the glass being touched in the electrified parts with a finger, afforded only very

 small

ſmall pricking ſparks, but a great number of them might be taken from different places. Then dexterouſly placing it again between the leaden plates, and compleating a circle between the two ſurfaces; a violent ſhock enſued.——Which demonſtrated the power to reſide in glaſs as glaſs, and that the non-electrics in contact ſerved only, like the armature of a loadſtone, to unite the force of the ſeveral parts, and bring them at once to any point deſired: it being the property of a non-electric, that the whole body inſtantly receives or gives what electrical fire is given to or taken from any one of its parts.

18. Upon this we made what we called an *electrical-battery*, conſiſting of eleven panes of large ſaſh-glaſs, arm'd with thin leaden plates, paſted on each ſide, placed vertically, and ſupported at two inches diſtance on ſilk cords, with thick hooks of leaden wire, one from each ſide, ſtanding upright, diſtant from each other, and convenient communications of wire and chain, from the giving ſide of one pane, to the receiving ſide of the other; that ſo the whole might be charged together, and with the ſame labour as one ſingle pane; and another contrivance to bring the giving ſides, after charging, in contact with one long wire, and the receivers with another, which two long wires would give the force of all the plates of glaſs at once through the body of any animal forming the circle with them. The plates may alſo be diſcharged ſeparately, or any number together that is required. But this machine is not much uſed, as not perfectly anſwering our intention

with

with regard to the eaſe of charging, for the reaſon given, *Sec.* 10. We made alſo of large glaſs panes, magical pictures, and ſelf-moving animated wheels, preſently to be deſcribed.

19. I perceive by the ingenious Mr *Watſon*'s laſt book, lately received, that Dr *Bevis* had uſed, before we had, panes of glaſs to give a ſhock *; though, till that book came to hand, I thought to have communicated it to you as a novelty. The excuſe for mentioning it here is, that we tried the experiment differently, drew different conſequences from it (for Mr *Watſon* ſtill ſeems to think the fire *accumulated on the non-electric* that is in contact with the glaſs, page 72) and, as far as we hitherto know, have carried it farther.

20. The magical picture † is made thus. Having a large metzotinto with a frame and glaſs, ſuppoſe of the KING, (God preſerve him) take out the print, and cut a pannel out of it, near two inches diſtant from the frame all round. If the cut is through the picture it is not the worſe. With thin paſte, or gum-water, fix the border that is cut off on the inſide the glaſs, preſſing it ſmooth and cloſe; then fill up the vacancy by gilding the glaſs well with leaf gold, or braſs. Gild likewiſe the inner edge of the back of the frame all round, except the top part, and form a communication between that gilding and the gilding behind the

* I have ſince heard that Mr *Smeaton* was the firſt who made uſe of panes of glaſs for that purpoſe.

† Contrived by Mr *Kinnerſley*.

the glaſs: then put in the board, and that ſide is finiſhed. Turn up the glaſs, and gild the fore ſide exactly over the back gilding, and when it is dry, cover it, by paſting on the pannel of the picture that hath been cut out, obſerving to bring the correſpondent parts of the border and picture together, by which the picture will appear of a piece, as at firſt, only part is behind the glaſs, and part before.—Hold the picture horizontally by the top, and place a little moveable gilt crown on the king's head. If now the picture be moderately electrified, and another perſon take hold of the frame with one hand, ſo that his fingers touch its inſide gilding, and with the other hand endeavour to take off the crown, he will receive a terrible blow, and fail in the attempt. If the picture were highly charged, the conſequence might perhaps be as fatal * as that of high treaſon, for when the ſpark is taken through a quire of paper laid on the picture, by means of a wire communication, it makes a fair hole through every ſheet, that is, through forty-eight leaves, (though a quire of paper is thought good armour againſt the puſh of a ſword, or even againſt a piſtol bullet, and the crack is exceeding loud. The operator, who holds the picture by the upper end, where the inſide of the frame is not gilt, to prevent its falling, feels nothing of the ſhock, and may touch the face of the picture without danger, which he pretends is a teſt of his loyalty.—If a

* We have ſince found it fatal to ſmall animals, though not to large ones. The biggeſt we have yet killed is a hen. 1750.

ring of persons take the shock among them, the experiment is called, *The Conspirators.*

21. On the principle, in *Sec.* 7, that hooks of bottles, differently charged, will attract and repel differently, is made an electrical wheel, that turns with considerable strength. A small upright shaft of wood passes at right angles through a thin round board, of about twelve inches diameter, and turns on a sharp point of iron, fixed in the lower end, while a strong wire in the upper end, passing through a small hole in a thin brass plate, keeps the shaft truly vertical. About thirty *radii* of equal length, made of sash-glass, cut in narrow strips, issue horizontally from the circumference of the board, the ends most distant from the center being about four inches apart. On the end of every one, a brass thimble is fixed. If now the wire of a bottle electrified in the common way, be brought near the circumference of this wheel, it will attract the nearest thimble, and so put the wheel in motion; that thimble, in passing by, receives a spark, and thereby being electrified is repelled, and so driven forwards; while a second being attracted, approaches the wire, receives a spark, and is driven after the first, and so on till the wheel has gone once round, when the thimbles before electrified approaching the wire, instead of being attracted as they were at first, are repelled, and the motion presently ceases.—But if another bottle, which had been charged through the coating, be placed near the same wheel, its wire will attract the thimble repelled by the first, and thereby double the force that carries the wheel

round;

round; and not only taking out the fire that had been communicated to the thimbles by the firſt bottle, but even robbing them of their natural quantity, inſtead of being repelled when they come again towards the firſt bottle, they are more ſtrongly attracted, ſo that the wheel mends its pace, till it goes with great rapidity twelve or fifteen rounds in a minute, and with ſuch ſtrength, as that the weight of one hundred *Spaniſh* dollars with which we once loaded it, did not ſeem in the leaſt to retard its motion.—This is called an electrical jack; and if a large fowl were ſpitted on the upright ſhaft, it would be carried round before a fire with a motion fit for roaſting.

22. But this wheel, like thoſe driven by wind, water, or weights, moves by a foreign force, to wit, that of the bottles. The ſelf-moving wheel, though conſtructed on the ſame principles, appears more ſurpriſing. 'Tis made of a thin round plate of window-glaſs, ſeventeen inches diameter, well gilt on both ſides, all but two inches next the edge. Two ſmall hemiſpheres of wood are then fixed with cement to the middle of the upper and under ſides, centrally oppoſite, and in each of them a thick ſtrong wire eight or ten inches long, which together make the axis of the wheel. It turns horizontally on a point at the lower end of its axis, which reſts on a bit of braſs cemented within a glaſs ſalt-cellar. The upper end of its axis paſſes through a hole in a thin braſs plate cemented to a long ſtrong piece of glaſs, which keeps it ſix or eight inches diſtant from any non-electric, and has a ſmall ball of wax or metal

metal on its top to keep in the fire. In a circle on the table which ſupports the wheel, are fixed twelve ſmall pillars of glaſs, at about four inches diſtance, with a thimble on the top of each. On the edge of the wheel is a ſmall leaden bullet, communicating by a wire with the gilding of the *upper* ſurface of the wheel; and about ſix inches from it is another bullet communicating in like manner with the *under* ſurface. When the wheel is to be charged by the upper ſurface, a communication muſt be made from the under ſurface to the table. When it is well charged it begins to move; the bullet neareſt to a pillar moves towards the thimble on that pillar, and paſſing by, electrifies it, and then puſhes itſelf from it; the ſucceeding bullet, which communicates with the other ſurface of the glaſs, more ſtrongly attracts that thimble, on account of its being before electrified by the other bullet; and thus the wheel encreaſes its motion till it comes to ſuch a height as that the reſiſtance of the air regulates it. It will go half an hour, and make one minute with another twenty turns in a minute, which is ſix hundred turns in the whole; the bullet of the upper ſurface giving in each turn twelve ſparks, to the thimbles, which makes ſeven thouſand two hundred ſparks; and the bullet of the under ſurface receiving as many from the thimbles; thoſe bullets moving in the time near two thouſand five hundred feet.—The thimbles are well fixed, and in ſo exact a circle, that the bullets may paſs within a very ſmall diſtance of each of them.—If inſtead of two bullets you put eight, four communicating with the upper ſurface, and four with the under ſurface, placed alternately; which eight, at about ſix inches diſtance, completes the circumference, the force and ſwiftneſs will be greatly increaſed, the wheel making fifty turns in a minute; but then it will not continue moving ſo long.——Theſe wheels may be applied, perhaps, to the ringing of chimes *, and moving of light-made orreries.

23. A ſmall wire bent circularly, with a loop at each end; let one end reſt againſt the under ſurface of the wheel, and bring the other end near the upper ſurface, it will give a terrible crack, and the force will be diſcharged.

24. Every ſpark in that manner drawn from the ſurface of the wheel, makes a round hole in the gilding, tearing off a part of it in coming out; which ſhews that the fire is not accumulated on the gilding, but is in the glaſs itſelf.

25. The gilding being varniſhed over with turpentine varniſh, the varniſh, though dry and hard, is burnt by the ſpark drawn through it, and gives a ſtrong ſmell and viſible ſmoke. And when the ſpark is drawn through paper, all round the hole made by it, the paper will be blacked by the ſmoke, which ſometimes penetrates ſeveral of the leaves. Part of the gilding torn off, is alſo found forcibly driven into the hole made in the paper by the ſtroke.

* This was afterwards done with ſucceſs by Mr *Kinnerſley*.

26. It

26. It is amazing to observe in how small a portion of glass a great electrical force may lie. A thin glass bubble about an inch diameter, weighing only six grains, being half filled with water, partly gilt on the outside, and furnish'd with a wire hook, gives, when electrified, as great a shock as a man can well bear. As the glass is thickest near the orifice, I suppose the lower half, which being gilt was electrified and gave the shock, did not exceed two grains; for it appeared, when broke, much thinner than the upper half.—If one of these thin bottles be electrified by the coating, and the spark taken out through the gilding, it will break the glass inwards, at the same time that it breaks the gilding outwards.

27. And allowing (for the reasons before given, §. 8, 9, 10.) that there is no more electrical fire in a bottle after charging, than before, how great must be the quantity in this small portion of glass! It seems as if it were of its very substance and essence. Perhaps if that due quantity of electrical fire so obstinately retained by glass, could be separated from it, it would no longer be glass; it might lose its transparency, or its brittleness, or its elasticity.—Experiments may possibly be invented hereafter, to discover this.

27. We were surprised at the account given in Mr *Watson*'s book, of a shock communicated through a great space of dry ground, and suspect there must be some metalline quality in the gravel of that ground; having found that

ſimple dry earth, rammed in a glaſs tube, open at both ends, and a wire hook inſerted in the earth at each end, the earth and wires making part of a circle, would not conduct the leaſt perceptible ſhock, and indeed when one wire was electrified, the other hardly ſhowed any ſigns of its being in connection with it *. Even a thoroughly wet pack-thread ſometimes fails of conducting a ſhock, though it otherwiſe conducts Electricity very well. A dry cake of ice, or an icicle held between two in a circle, likewiſe prevents the ſhock, which one would not expect, as water conducts it ſo perfectly well.—Gilding on a new book, though at firſt it conducts the ſhock extremely well, yet fails after ten or a dozen experiments, though it appears otherwiſe in all reſpects the ſame, which we cannot account for †.

28. There is one experiment more which ſurprizes us, and is not hitherto ſatisfactorily accounted for; it is this: Place an iron ſhot on a glaſs ſtand, and let a ball of damp cork, ſuſpended by a ſilk thread, hang in contact with the ſhot. Take a bottle in each hand, one that is electrified through the hook, the other through the coating: Apply the giving wire to the ſhot, which will electrify it *poſitive-*

* Probably the ground is never ſo dry.

† We afterwards found that it failed after one ſtroke with a large bottle; and the continuity of the gold appearing broken, and many of its parts diſſipated, the Electricity could not paſs the remaining parts without leaping from part to part through the air, which always reſiſts the motion of this fluid, and was probably the cauſe of the gold's not conducting ſo well as before.

ly,

ly, and the cork ſhall be repelled: then apply the requiring wire, which will take out the ſpark given by the other; when the cork will return to the ſhot: Apply the ſame again, and take out another ſpark, ſo will the ſhot be electrified *negatively*, and the cork in that caſe ſhall be repelled equally as before. Then apply the giving wire to the ſhot, and give the ſpark it wanted, ſo will the cork return: Give it another, which will be an addition to its natural quantity, ſo will the cork be repelled again: And ſo may the experiment be repeated as long as there is any charge in the bottles. Which ſhews that bodies having leſs than the common quantity of Electricity, repel each other, as well as thoſe that have more.

Chagrined a little that we have been hitherto able to produce nothing in this way of uſe to mankind; and the hot weather coming on, when electrical experiments are not ſo agreeable, it is propoſed to put an end to them for this ſeaſon, ſomewhat humorouſly, in a party of pleaſure, on the banks of *Skuylkil* *. Spirits, at the ſame time, are to be fired by a ſpark ſent from ſide to ſide through the river, without any other conductor than the water; an experiment which we ſome time ſince performed, to the amazement of many †. A turkey is to be killed for our dinner

* The river that waſhes one ſide of *Philadelphia*, as the *Delaware* does the other; both are ornamented with the ſummer habitations of the citizens, and the agreeable manſions of the principal people of this colony.

† As the poſſibility of this experiment has not been eaſily conceived, I ſhall

dinner by the *electrical shock*, and roasted by the *electrical jack*, before a fire kindled by the *electrified bottle*: when the healths of all the famous electricians in *England*, *Holland*, *France*, and *Germany*, are to be drank in * *electrified bumpers*, under the discharge of guns from the *electrical battery*.

shall here describe it.—Two iron rods, about three feet long, were planted just within the margin of the river, on the opposite sides. A thick piece of wire, with a small round knob at its end, was fixed to the top of one of the rods, bending downwards, so as to deliver commodiously the spark upon the surface of the spirit. A small wire fastened by one end to the handle of the spoon, containing the spirit, was carried a-cross the river, and supported in the air by the rope commonly used to hold by, in drawing the ferry-boats over. The other end of this wire was tied round the coating of the bottle; which being charged, the spark was delivered from the hook to the top of the rod standing in the water on that side. At the same instant the rod on the other side delivered a spark into the spoon, and fired the spirit. The electric fire returning to the coating of the bottle, through the handle of the spoon and the supported wire connected with them.

That the electric fire thus actually passes through the water, has since been satisfactorily demonstrated to many by an experiment of Mr *Kinnersley*'s, performed in a trough of water about ten feet long. The hand being placed under water in the direction of the spark (which always takes the strait or shortest course) is struck and penetrated by it as it passes.

* An *electrified bumper* is a small thin glass tumbler, near filled with wine, and electrified as the bottle. This when brought to the lips gives a shock, if the party be close shaved, and does not breathe on the liquor.

April 29,
1749.

LET-

LETTER V.

CONTAINING

OBSERVATIONS *and* SUPPOSITIONS, *towards forming a new* HYPOTHESIS, *for explaining the several* Phænomena *of* THUNDER-GUSTS *.

SIR,

§. 1. NON-ELECTRIC bodies, that have electric fire thrown into them, will retain it till other non-electrics, that have less, approach; and then it is communicated by a snap, and becomes equally divided.

2. Electrical fire loves water, is strongly attracted by it, and they can subsist together.

3. Air is an electric *per se*, and when dry will not conduct the electrical fire; it will neither receive it, nor give it to other bodies; otherwise no body surrounded by air, could be electrified positively and negatively: for should it be

* Thunder-gusts are sudden storms of thunder and lightning, which are frequently of short duration, but sometimes produce mischievous effects.

be attempted positively: the air would immediately take away the overplus; or negatively, the air would supply what was wanting.

4. Water being electrified, the vapours arising from it will be equally electrified; and floating in the air, in the form of clouds, or otherwise, will retain that quantity of electrical fire, till they meet with other clouds or bodies not so much electrified, and then will communicate as before mentioned.

5. Every particle of matter electrified is repelled by every other particle equally electrified. Thus the stream of a fountain, naturally dense and continual, when electrified, will separate and spread in the form of a brush, every drop endeavouring to recede from every other drop. But on taking out the electrical fire they close again.

6. Water being strongly electrified (as well as when heated by common fire) rises in vapours more copiously; the attraction of cohesion among its particles being greatly weakened, by the opposite power of repulsion introduced with the electrical fire; and when any particle is by any means disengaged, it is immediately repelled, and so flies into the air.

7. Particles happening to be situated as *A* and *B*, (FIG. VI. *representing the profile of a vessel of water*) are more easily disengaged than *C* and *D*, as each is held by contact with three only, whereas *C* and *D* are each in contact with nine. When the surface of the water has the least motion, parti-

particles are continually puſhed into the ſituation repreſented by *A* and *B*.

8. Friction between a non-electric and an electric *per ſe* will produce electrical fire; not by *creating*, but *collecting* it: for it is equally diffuſed in our walls, floors, earth, and the whole maſs of common matter. Thus the whirling glaſs globe, during its friction againſt the cuſhion, draws fire from the cuſhion, the cuſhion is ſupplied from the frame of the machine, that from the floor on which it ſtands. Cut off the communication by thick glaſs or wax, placed under the cuſhion, and no fire can be *produced*, becauſe it cannot be *collected*.

9. The ocean is a compound of water, a non-electric, and ſalt an electric *per ſe*.

10. When there is a friction among the parts near its ſurface, the electrical fire is collected from the parts below. It is then plainly viſible in the night; it appears at the ſtern and in the wake of every ſailing veſſel; every daſh of an oar ſhews it, and every ſurf and ſpray: In ſtorms the whole ſea ſeems on fire.—The detach'd particles of water then repelled from the electrified ſurface, continually carry off the fire as it is collected; they riſe and form clouds, and thoſe clouds are highly electrified, and retain the fire till they have an opportunity of communicating it.

11. The particles of water riſing in vapours, attach themſelves to particles of air.

12. The particles of air are ſaid to be hard, round, ſeparate and diſtant from each other; every particle ſtrongly

repelling every other particle, whereby they recede from each other, as far as common gravity will permit.

13. The ſpace between any three particles equally repelling each other, will be an equilateral triangle.

14. In air compreſſed, theſe triangles are ſmaller; in rarified air they are larger.

15. Common fire joined with air, increaſes the repulſion, enlarges the triangles, and thereby makes the air ſpecifically lighter. Such air, among denſer air, will riſe.

16. Common fire, as well as electrical fire, gives repulſion to the particles of water, and deſtroys their attraction of coheſion; hence common fire, as well as electrical fire, aſſiſts in raiſing vapours.

17. Particles of water, having no fire in them, mutually attract each other. Three particles of water then being attached to the three particles of a triangle of air, would by their mutual attraction operating againſt the air's repulſion, ſhorten the ſides and leſſen the triangle, whereby that portion of air being made denſer, would ſink to the earth with its water, and not riſe to contribute to the formation of a cloud.

18. But if every particle of water attaching itſelf to air, brings with it a particle of common fire, the repulſion of the air being aſſiſted and ſtrengthened by the fire, more than obſtructed by the mutual attraction of the particles of water, the triangle dilates, and that portion of air becoming rarer and ſpecifically lighter riſes.

19. If the particles of water bring electrical fire when they

they attach themſelves to air, the repulſion between the particles of water electrified, joins with the natural repulſion of the air, to force its particles to a greater diſtance, whereby the triangles are dilated, and the air riſes, carrying up with it the water.

20. If the particles of water bring with them portions of *both ſorts* of fire, the repulſion of the particles of air is ſtill more ſtrengthened and increaſed, and the triangles farther enlarged.

21. One particle of air may be ſurrounded by twelve particles of water of equal ſize with itſelf, all in contact with it; and by more added to thoſe.

22. Particles of air thus loaded would be drawn nearer together by the mutual attraction of the particles of water, did not the fire, common or electrical, aſſiſt their repulſion.

23. If air thus loaded be compreſſed by adverſe winds, or by being driven againſt mountains, *&c.* or condenſed by taking away the fire that aſſiſted it in expanding; the triangles contract, the air with its water will deſcend as a dew; or, if the water ſurrounding one particle of air comes in contact with the water ſurrounding another, they coaleſce and form a drop, and we have rain.

24. The ſun ſupplies (or ſeems to ſupply) common fire to all vapours, whether raiſed from earth or ſea.

25. Thoſe vapours which have both common and electrical fire in them, are better ſupported, than thoſe which have only common fire in them, For when vapours riſe into

into the coldeſt region above the earth, the cold will not diminiſh the electrical fire, if it doth the common.

26. Hence clouds formed by vapours raiſed from freſh waters within land, from growing vegetables, moiſt earth, *&c.* more ſpeedily and eaſily depoſite their water, having but little electrical fire to repel and keep the particles ſeparate. So that the greateſt part of the water raiſed from the land, is let fall on the land again; and winds blowing from the land to the ſea are dry; there being little uſe for rain on the ſea, and to rob the land of its moiſture, in order to rain on the ſea, would not appear reaſonable.

27. But clouds formed by vapours raiſed from the ſea, having both fires, and particularly a great quantity of the electrical, ſupport their water ſtrongly, raiſe it high, and being moved by winds, may bring it over the middle of the broadeſt continent from the middle of the wideſt ocean.

28. How theſe ocean clouds, ſo ſtrongly ſupporting their water, are made to depoſite it on the land where it is wanted, is next to be conſidered.

29. If they are driven by winds againſt mountains, thoſe mountains being leſs electrified attract them, and on contact take away their electrical fire (and being cold, the common fire alſo;) hence the particles cloſe towards the mountains and towards each other. If the air was not much loaded, it only falls in dews on the mountain tops and ſides, forms ſprings, and deſcends to the vales in rivulets, which united, make larger ſtreams and rivers. If much loaded, the electrical fire is at once taken from the whole

whole cloud; and, in leaving it, flaſhes brightly and cracks loudly; the particles inſtantly coaleſcing for want of that fire, and falling in a heavy ſhower.

30. When a ridge of mountains thus dams the clouds, and draws the electrical fire from the cloud firſt approaching it; that which next follows, when it comes near the firſt cloud, now deprived of its fire, flaſhes into it, and begins to depoſite its own water; the firſt cloud again flaſhing into the mountains; the third approaching cloud, and all the ſucceeding ones, acting in the ſame manner as far back as they extend, which may be over many hundred miles of country.

31. Hence the continual ſtorms of rain, thunder, and lightning on the eaſt ſide of the *Andes*, which running north and ſouth, and being vaſtly high, intercept all the clouds brought againſt them from the *Atlantic* ocean by the trade winds, and oblige them to depoſite their waters, by which the vaſt rivers *Amazons*, *La Plata*, and *Oroonoko* are formed, which return the water into the ſame ſea, after having fertilized a country of very great extent.

32. If a country be plain, having no mountains to intercept the electrified clouds, yet it is not without means to make them depoſite their water. For if an electrified cloud coming from the ſea, meets in the air a cloud raiſed from the land, and therefore not electrified; the firſt will flaſh its fire into the latter, and thereby both clouds ſhall be made ſuddenly to depoſite water.

33. The electrified particles of the firſt cloud cloſe when they loſe their fire; the particles of the other cloud

cloſe

close in receiving it: in both, they have thereby an opportunity of coalescing into drops.—The concussion or jerk given to the air, contributes also to shake down the water, not only from those two clouds, but from others near them. Hence the sudden fall of rain immediately after flashes of lightning.

34. To shew this by an easy experiment: Take two round pieces of pasteboard two inches diameter; from the center and circumference of each of them suspend by fine silk threads eighteen inches long, seven small balls of wood, or seven peas equal in bigness: so will the balls appending to each pasteboard, form equal equilateral triangles, one ball being in the center, and six at equal distances from that, and from each other; and thus they represent particles of air. Dip both sets in water, and some adhering to each ball, they will represent air loaded. Dexterously electrify one set, and its balls will repel each other to a greater distance, enlarging the triangles. Could the water supported by the seven balls come into contact, it would form a drop or drops so heavy as to break the cohesion it had with the balls, and so fall. Let the two sets then represent two clouds, the one a sea cloud electrified, the other a land cloud. Bring them within the sphere of attraction, and they will draw towards each other, and you will see the separated balls close thus; the first electrified ball that comes near an unelectrified ball by attraction joins it, and gives it fire; instantly they separate, and each flies to another ball of its own party, one to

give

give, the other to receive fire; and ſo it proceeds through both ſets, but ſo quick as to be in a manner inſtantaneous. In the colliſion they ſhake off and drop their water, which repreſents rain.

35. Thus when ſea and land clouds would paſs at too great a diſtance from the flaſh, they are attracted towards each other till within that diſtance; for the ſphere of electrical attraction is far beyond the diſtance of flaſhing.

36. When a great number of clouds from the ſea meet a number of clouds raiſed from the land, the electrical flaſhes appear to ſtrike in different parts; and as the clouds are joſtled and mixed by the winds, or brought near by the electrical attraction, they continue to give and receive flaſh after flaſh, till the electrical fire is equally diffuſed.

37. When the gun-barrel (in electrical experiments) has but little electrical fire in it, you muſt approach it very near with your knuckle, before you can draw a ſpark. Give it more fire, and it will give a ſpark at a greater diſtance. Two gun-barrels united, and as highly electrified, will give a ſpark at a ſtill greater diſtance. But if two gun-barrels electrified will ſtrike at two inches diſtance, and make a loud ſnap, to what a great diſtance may 10,000 acres of electrified cloud ſtrike and give its fire, and how loud muſt be that crack?

38. It is a common thing to ſee clouds at different heights paſſing different ways, which ſhews different currents of air, one under the other. As the air between the tropics

tropics is rarified by the ſun, it riſes, the denſer northern and ſouthern air preſſing into its place. The air ſo rarified and forced up, paſſes northward and ſouthward, and muſt deſcend in the polar regions, if it has no opportunity before, that the circulation may be carried on.

39. As currents of air, with the clouds therein, paſs different ways, 'tis eaſy to conceive how the clouds, paſſing over each other, may attract each other, and ſo come near enough for the electrical ſtroke. And alſo how electrical clouds may be carried within land very far from the ſea, before they have an opportunity to ſtrike.

40. When the air, with its vapours raiſed from the ocean between the tropics, comes to deſcend in the polar regions, and to be in contact with the vapours ariſing there, the electrical fire they brought begins to be communicated, and is ſeen in clear nights, being firſt viſible where 'tis firſt in motion, that is, where the contact begins, or in the moſt northern part; from thence the ſtreams of light ſeem to ſhoot ſoutherly, even up to the zenith of northern countries. But tho' the light ſeems to ſhoot from the north ſoutherly, the progreſs of the fire is really from the ſouth northerly, its motion beginning in the north being the reaſon that 'tis there firſt ſeen.

For the electrical fire is never viſible but when in motion, and leaping from body to body, or from particle to particle thro' the air. When it paſſes thro' denſe bodies 'tis unſeen. When a wire makes part of the circle, in the exploſion of the electrical phial, the fire, though in great quantity

quantity, paſſes in the wire inviſibly: but in paſſing along a chain, it becomes viſible as it leaps from link to link. In paſſing along leaf gilding 'tis viſible: for the leaf-gold is full of pores; hold a leaf to the light and it appears like a net, and the fire is ſeen in its leaping over the vacancies.—And as when a long canal filled with ſtill water is opened at one end, in order to be diſcharged, the motion of the water begins firſt near the opened end, and proceeds towards the cloſe end, tho' the water itſelf moves from the cloſe towards the opened end: ſo the electrical fire diſcharged into the polar regions, perhaps from a thouſand leagues length of vaporiſed air, appears firſt where 'tis firſt in motion, *i. e.* in the moſt northern part, and the appearance proceeds ſouthward, tho' the fire really moves northward. This is ſuppoſed to account for the *Aurora Borealis.*

41. When there is great heat on the land, in a particular region (the ſun having ſhone on it perhaps ſeveral days, while the ſurrounding countries have been ſcreen'd by clouds) the lower air is rarified and riſes, the cooler denſer air above deſcends; the clouds in that air meet from all ſides, and join over the heated place; and if ſome are electrified, others not, lightning and thunder ſucceed, and ſhowers fall. Hence thunder-guſts after heats, and cool air after guſts; the water and the clouds that bring it, coming from a higher and therefore a cooler region.

42. An electrical ſpark, drawn from an irregular body at ſome diſtance is ſcarce ever ſtrait, but ſhows crooked

and waving in the air. So do the flashes of lightning; the clouds being very irregular bodies.

43. As electrified clouds pass over a country, high hills and high trees, lofty towers, spires, masts of ships, chimneys, *&c.* as so many prominencies and points, draw the electrical fire, and the whole cloud discharges there.

44. Dangerous, therefore, is it to take shelter under a tree, during a thunder-gust. It has been fatal to many, both men and beasts.

45. It is safer to be in the open field for another reason. When the cloaths are wet, if a flash in its way to the ground should strike your head, it may run in the water over the surface of your body; whereas, if your cloaths were dry, it would go through the body.

Hence a wet rat cannot be killed by the exploding electrical bottle, when a dry rat may *.

46. Common fire is in all bodies, more or less, as well as electrical fire. Perhaps they may be different modifications of the same element; or they may be different elements. The latter is by some suspected.

47. If they are different things, yet they may and do subsist together in the same body.

48. When electrical fire strikes through a body, it acts upon the common fire contained in it, and puts that fire in motion; and if there be a sufficient quantity of each kind of fire, the body will be inflamed.

* This was tried with a bottle, containing about a quart. It is since thought that one of the large glass jars, mentioned in these papers, might have killed him, though wet.

49. When

49. When the quantity of common fire in the body is ſmall, the quantity of the electrical fire (or the electrical ſtroke) ſhould be greater: if the quantity of common fire be great, leſs electrical fire ſuffices to produce the effect.

50. Thus ſpirits muſt be heated before we can fire them by the electrical ſpark*. If they are much heated, a ſmall ſpark will do; if not, the ſpark muſt be greater.

51. 'Till lately we could only fire warm vapours; but now we can burn hard dry roſin. And when we can procure greater electrical ſparks, we may be able to fire not only unwarm'd ſpirits, as lightning does, but even wood, by giving ſufficient agitation to the common fire contained in it, as friction we know will do.

52. Sulphureous and inflammable vapours ariſing from the earth, are eaſily kindled by lightning. Beſides what ariſe from the earth, ſuch vapours are ſent out by ſtacks of moiſt hay, corn, or other vegetables, which heat and reek. Wood rotting in old trees or buildings does the ſame. Such are therefore eaſily and often fired.

53. Metals are often melted by lightning, tho' perhaps not from heat in the lightning, nor altogether from agitated fire in the metals.—For as whatever body can inſinuate itſelf between the particles of metal, and overcome the attraction by which they cohere (as ſundry menſtrua

* We have ſince fired ſpirits without heating them, when the weather is warm. A little poured into the palm of the hand, will be warmed ſufficiently by the hand, if the ſpirit be well rectified. Æther takes fire moſt readily.

can) will make the ſolid become a fluid, as well as fire, yet without heating it: ſo the electrical fire, or lightning, creating a violent repulſion between the particles of the metal it paſſes through, the metal is fuſed.

54. If you would, by a violent fire, melt off the end of a nail, which is half driven into a door, the heat given the whole nail before a part would melt, muſt burn the board it ſticks in. And the melted part would burn the floor it dropp'd on. But if a ſword can be melted in the ſcabbard, and money in a man's pocket, by lightning, without burning either, it muſt be a cold fuſion *.

55. Lightning rends ſome bodies. The electrical ſpark will ſtrike a hole through a quire of ſtrong paper.

56. If the ſource of lightning, aſſigned in this paper, be the true one, there ſhould be little thunder heard at ſea far from land. And accordingly ſome old ſea-captains, of whom enquiry has been made, do affirm, that the fact agrees perfectly with the hypotheſis; for that in croſſing the great ocean, they ſeldom meet with thunder till they come into ſoundings; and that the iſlands far from the continent have very little of it. And a curious obſerver, who lived 13 years at *Bermudas*, ſays, there was leſs thunder there in that whole time than he has ſometimes heard in a month at *Carolina*.

* Theſe facts, though related in ſeveral accounts, are now doubted; ſince it has been obſerved that the parts of a bell-wire which fell on the floor being broken and partly melted by lightning, did actually burn into the boards. (See *Philoſ. Tranſ.* Vol. LI. Part I. and Mr *Kinnerſley* has found that a fine iron wire, melted by Electricity, has had the ſame effect.)

A D-

ADDITIONAL PAPERS

TO

PETER COLLINSON, *Esq*; F.R.S. *London*.

SIR, *Philadelphia*, *July* 29, 1750.

AS you firſt put us on electrical experiments, by ſending to our library company a tube, with directions how to uſe it; and as our honourable proprietary enabled us to carry thoſe experiments to a greater height, by his generous preſent of a compleat electrical apparatus; 'tis fit that both ſhould know, from time to time, what progreſs we make. It was in this view I wrote and ſent you my former papers on this ſubject, deſiring, that as I had not the honour of a direct correſpondence with that bountiful benefactor to our library, they might be communicated to him through your hands. In the ſame view I write and ſend you this additional paper. If it happens to bring you nothing new (which may well be, conſidering the number of ingenious men in *Europe*, continually engaged in the ſame reſearches) at leaſt it will ſhow, that the inſtruments put into our hands are not neglected; and, that if no valuable diſcoveries are made by us, whatever the cauſe may be, it is not want of induſtry and application.

I am, Sir,

Your much obliged

Humble Servant,

B. FRANKLIN.

OPINIONS *and* CONJECTURES, *concerning the Properties and Effects of the electrical Matter, arising from Experiments and Observations, made at* Philadelphia, 1749.

§. 1. THE electrical matter consists of particles extremely subtile, since it can permeate common matter, even the densest metals, with such ease and freedom as not to receive any perceptible resistance.

2. If any one should doubt whether the electrical matter passes thro' the substance of bodies, or only over and along their surfaces, a shock from an electrified large glass jar, taken through his own body, will probably convince him.

3. Electrical matter differs from common matter in this, that the parts of the latter mutually attract, those of the former mutually repel, each other. Hence the appearing divergency in a stream of electrified effluvia.

4. But though the particles of electrical matter do repel each other, they are strongly attracted by all other matter *.

* See the ingenious essays on Electricity, in the *Transactions*, by Mr *Ellicot*.

5. From

5. From these three things, the extreme subtilty of the electrical matter, the mutual repulsion of its parts, and the strong attraction between them and other matter, arise this effect, that, when a quantity of electrical matter is applied to a mass of common matter, of any bigness or length, within our observation, (which hath not already got its quantity) it is immediately and equally diffused through the whole.

6. Thus common matter is a kind of spunge to the electrical fluid. And as a spunge would receive no water if the parts of water were not smaller than the pores of the spunge; and even then but slowly, if there were not a mutual attraction between those parts and the parts of the spunge; and would still imbibe it faster, if the mutual attraction among the parts of the water did not impede, some force being required to separate them; and fastest, if, instead of attraction, there were a mutual repulsion among those parts, which would act in conjunction with the attraction of the spunge. So is the case between the electrical and common matter.

7. But in common matter there is (generally) as much of the electrical as it will contain within its substance. If more is added, it lies without upon the surface, and forms what we call an electrical atmosphere; and then the body is said to be electrified.

8. 'Tis supposed, that all kinds of common matter do not attract and retain the electrical, with equal strength and force, for reasons to be given hereafter. And that those called

called electrics *per ſe*, as glaſs, &c. attract and retain it ſtrongeſt, and contain the greateſt quantity.

9. We know that the electrical fluid is *in* common matter, becauſe we can pump it *out* by the globe or tube. We know that common matter has near as much as it can contain, becauſe, when we add a little more to any portion of it, the additional quantity does not enter, but forms an electrical atmoſphere. And we know that common matter has not (generally) more than it can contain, otherwiſe all looſe portions of it would repel each other, as they conſtantly do when they have electric atmoſpheres.

10. The beneficial uſes of this electric fluid in the creation, we are not yet well acquainted with, though doubtleſs ſuch there are, and thoſe very conſiderable; but we may ſee ſome pernicious conſequences that would attend a much greater proportion of it. For had this globe we live on, as much of it in proportion as we can give to a globe of iron, wood, or the like, the particles of duſt and other light matters that get looſe from it, would, by virtue of their ſeparate electrical atmoſpheres, not only repel each other, but be repelled from the earth, and not eaſily be brought to unite with it again; whence our air would continually be more and more clogged with foreign matter, and grow unfit for reſpiration. This affords another occaſion of adoring that wiſdom which has made all things by weight and meaſure!

11. If a piece of common matter be ſuppoſed entirely free from electrical matter, and a ſingle particle of the latter

latter be brought nigh, it will be attracted, and enter the body, and take place in the center, or where the attraction is every way equal. If more particles enter, they take their places where the balance is equal between the attraction of the common matter, and their own mutual repulsion. 'Tis supposed they form triangles, whose sides shorten as their number increases; 'till the common matter has drawn in so many, that its whole power of compressing those triangles by attraction, is equal to their whole power of expanding themselves by repulsion; and then will such piece of matter receive no more.

12. When part of this natural proportion of electrical fluid is taken out of a piece of common matter, the triangles formed by the remainder, are supposed to widen by the mutual repulsion of the parts, until they occupy the whole piece.

13. When the quantity of electrical fluid, taken from a piece of common matter, is restored again, it enters, the expanded triangles being again compressed till there is room for the whole.

14. To explain this: take two apples, or two balls of wood or other matter, each having its own natural quantity of the electrical fluid. Suspend them by silk lines from the cieling. Apply the wire of a well-charged vial, held in your hand, to one of them (A) *Fig.* 7, and it will receive from the wire a quantity of the electrical fluid; but will not imbibe it, being already full. The fluid therefore will flow round its surface, and form an electrical atmosphere. Bring

Bring A into contact with B, and half the electrical fluid is communicated, so that each has now an electrical atmosphere, and therefore they repel each other. Take away these atmospheres by touching the balls, and leave them in their natural state: then, having fixed a stick of sealing-wax to the middle of the vial to hold it by, apply the wire to A, at the same time the coating touches B. Thus will a quantity of the electrical fluid be drawn out of B, and thrown on A. So that A will have a redundance of this fluid, which forms an atmosphere round it, and B an exactly equal deficiency. Now, bring these balls again into contact, and the electrical atmosphere will not be divided between A and B, into two smaller atmospheres as before; for B will drink up the whole atmosphere of A, and both will be found again in their natural state.

15. The form of the electrical atmosphere is that of the body it surrounds. This shape may be rendered visible in a still air, by raising a smoke from dry rosin, dropt into a hot tea-spoon under the electrised body, which will be attracted, and spread itself equally on all sides, covering and concealing the body*. And this form it takes, because it is attracted by all parts of the surface of the body, though it cannot enter the substance already replete. Without this attraction, it would not remain round the body, but dissipate in the air.

* See page 6.

16. The atmoſphere of electrical particles ſurrounding an electrified ſphere, is not more diſpoſed to leave it, or more eaſily drawn off from any one part of the ſphere than from another, becauſe it is equally attracted by every part. But that is not the caſe with bodies of any other figure. From a cube it is more eaſily drawn at the corners than at the plane ſides, and ſo from the angles of a body of any other form, and ſtill moſt eaſily from the angle that is moſt acute. Thus if a body ſhaped as A,B,C,D,E, in Fig. 8. be electrified, or have an electrical atmoſphere communicated to it, and we conſider every ſide as a baſe on which the particles reſt, and by which they are attracted, one may ſee, by imagining a line from A to F, and another from E to G, that the portion of the atmoſphere included in F,A,E,G, has the line A E for its baſis. So the portion of atmoſphere included in H, A, B, I, has the line A, B, for its baſis. And likewiſe the portion included in K, B, C, L, has B, C, to reſt on; and ſo on the other ſide of the figure. Now if you would draw off this atmoſphere with any blunt ſmooth body, and approach the middle of the ſide A, B, you muſt come very near, before the force of your attracter exceeds the force or power with which that ſide holds its atmoſphere. But there is a ſmall portion between I, B, K, that has leſs of the ſurface to reſt on, and to be attracted by, than the neighbouring portions, while at the ſame time there is a mutual repulſion between its particles, and the particles of thoſe portions, therefore here you can get it with more eaſe, or at

a greater diſtance. Between F,A,H, there is a larger portion that has yet a leſs ſurface to reſt on, and to attract it; here therefore you can get it away ſtill more eaſily. But eaſieſt of all between L,C,M, where the quantity is largeſt, and the ſurface to attract and keep it back the leaſt. When you have drawn away one of theſe angular portions of the fluid, another ſucceeds in its place, from the nature of fluidity and the mutual repulſion before-mentioned; and ſo the atmoſphere continues flowing off at ſuch angle, like a ſtream, till no more is remaining. The extremities of the portions of atmoſphere over theſe angular parts, are likewiſe at a greater diſtance from the electrified body, as may be ſeen by the inſpection of the above figure; the point of the atmoſphere of the angle C, being much farther from C, than any other part of the atmoſphere over the lines C, B, or B, A: And, beſides the diſtance ariſing from the nature of the figure, where the attraction is leſs, the particles will naturally expand to a greater diſtance by their mutual repulſion. On theſe accounts we ſuppoſe electrified bodies diſcharge their atmoſpheres upon unelectrified bodies more eaſily, and at a greater diſtance from their angles and points than from their ſmooth ſides.— Thoſe points will alſo diſcharge into the air, when the body has too great an electrical atmoſphere, without bringing any non-electric near, to receive what is thrown off: For the air, though an electric *per ſe*, yet has always more or leſs water and other non-electric matters mixed with it: and theſe attract and receive what is ſo diſcharged.

17. But

17. But points have a property, by which they *draw on* as well as *throw off* the electrical fluid, at greater distances than blunt bodies can. That is, as the pointed part of an electrified body will discharge the atmosphere of that body, or communicate it farthest to another body, so the point of an unelectrified body will draw off the electrical atmosphere from an electrified body, farther than a blunter part of the same unelectrified body will do. Thus a pin held by the head, and the point presented to an electrified body, will draw off its atmosphere at a foot distance; where, if the head were presented instead of the point, no such effect would follow. To understand this, we may consider, that if a person standing on the floor would draw off the electrical atmosphere from an electrified body, an iron crow and a blunt knitting-needle held alternately in his hand, and presented for that purpose, do not draw with different forces in proportion to their different masses. For the man, and what he holds in his hand, be it large or small, are connected with the common mass of unelectrified matter; and the force with which he draws is the same in both cases, it consisting in the different proportion of electricity in the electrified body, and that common mass. But the force with which the electrified body retains its atmosphere by attracting it, is proportioned to the surface over which the particles are placed; *i. e.* four square inches of that surface retain their atmosphere with four times the force that one square inch retains its atmosphere. And as in plucking the hairs from the horse's

tail,

tail, a degree of ſtrength not ſufficient to pull away a handful at once, could yet eaſily ſtrip it hair by hair ; ſo a blunt body preſented cannot draw off a number of particles at once, but a pointed one, with no greater force, takes them away eaſily, particle by particle.

18. Theſe explanations of the power and operation of points, when they firſt occurr'd to me, and while they firſt floated in my mind, appeared perfectly ſatisfactory ; but now I have wrote them, and conſidered them more cloſely in black and white, I muſt own I have ſome doubts about them ; yet, as I have at preſent nothing better to offer in their ſtead, I do not croſs them out : for even a bad ſolution read, and its faults diſcovered, has often given riſe to a good one, in the mind of an ingenious reader.

19. Nor is it of much importance to us, to know the manner in which nature executes her laws ; 'tis enough if we know the laws themſelves. 'Tis of real uſe to know that china left in the air unſupported will fall and break ; but *how* it comes to fall, and *why* it breaks, are matters of ſpeculation. 'Tis a pleaſure indeed to know them, but we can preſerve our china without it.

20. Thus in the preſent caſe, to know this power of points, may poſſibly be of ſome uſe to mankind, though we ſhould never be able to explain it. The following experiments, as well as thoſe in my firſt paper, ſhew this power. I have a large prime conductor, made of ſeveral thin ſheets of clothier's paſteboard, form'd into a tube, near

ten

ten feet long and a foot diameter. It is cover'd with *Dutch* emboſs'd paper, almoſt totally gilt. This large metallic ſurface ſupports a much greater electrical atmoſphere than a rod of iron of 50 times the weight would do. It is ſuſpended by ſilk lines, and when charged will ſtrike at near two inches diſtance, a pretty hard ſtroke, ſo as to make ones knuckle ach. Let a perſon ſtanding on the floor preſent the point of a needle at 12 or more inches diſtance from it, and while the needle is ſo preſented, the conductor cannot be charged, the point drawing off the fire as faſt as it is thrown on by the electrical globe. Let it be charged, and then preſent the point at the ſame diſtance, and it will ſuddenly be diſcharged. In the dark you may ſee a light on the point, when the experiment is made. And if the perſon holding the point ſtands upon wax, he will be electrified by receiving the fire at that diſtance. Attempt to draw off the electricity with a blunt body, as a bolt of iron round at the end, and ſmooth (a ſilverſmith's iron punch, inch thick, is what I uſe) and you muſt bring it within the diſtance of three inches before you can do it, and then it is done with a ſtroke and crack. As the paſteboard tube hangs looſe on ſilk lines, when you approach it with the punch iron, it likewiſe will move towards the punch, being attracted while it is charged; but if, at the ſame inſtant, a point be preſented as before, it retires again, for the point diſcharges it. Take a pair of large braſs ſcales, of two or more feet beam, the cords of the ſcales being ſilk. Suſpend

pend the beam by a pack-thread from the cieling, ſo that the bottom of the ſcales may be about a foot from the floor: The ſcales will move round in a circle by the untwiſting of the packthread. Set the iron punch on the end upon the floor, in ſuch a place as that the ſcales may paſs over it in making their circle: Then electrify one ſcale, by applying the wire of a charged phial to it. As they move round, you ſee that ſcale draw nigher to the floor, and dip more when it comes over the punch; and if that be placed at a proper diſtance, the ſcale will ſnap and diſcharge its fire into it. But if a needle be ſtuck on the end of the punch, its point upwards, the ſcale, inſtead of drawing nigh to the punch, and ſnapping, diſcharges its fire ſilently through the point, and riſes higher from the punch. Nay, even if the needle be placed upon the floor near the punch, its point upwards, the end of the punch, tho' ſo much higher than the needle, will not attract the ſcale and receive its fire, for the needle will get it and convey it away, before it comes nigh enough for the punch to act. And this is conſtantly obſervable in theſe experiments, that the greater quantity of electricity on the paſteboard tube, the farther it ſtrikes or diſcharges its fire, and the point likewiſe will draw it off at a ſtill greater diſtance.

Now if the fire of electricity and that of lightning be the ſame, as I have endeavoured to ſhew at large, in a former paper, this paſteboard tube and theſe ſcales may repreſent electrified clouds. If a tube of only ten feet long

long will ſtrike and diſcharge its fire on the punch at two or three inches diſtance, an electrified cloud of perhaps 10,000 acres may ſtrike and diſcharge on the earth at a proportionably greater diſtance. The horizontal motion of the ſcales over the floor, may repreſent the motion of the clouds over the earth; and the erect iron punch, a hill or high building; and then we ſee how electrified clouds paſſing over hills or high buildings at too great a height to ſtrike, may be attracted lower till within their ſtriking diſtance. And laſtly, if a needle fixed on the punch with its point upright, or even on the floor below the punch, will draw the fire from the ſcale ſilently at a much greater than the ſtriking diſtance, and ſo prevent its deſcending towards the punch; or if in its courſe it would have come nigh enough to ſtrike, yet being firſt deprived of its fire it cannot, and the punch is thereby ſecured from the ſtroke. I ſay, if theſe things are ſo, may not the knowledge of this power of points be of uſe to mankind, in preſerving houſes, churches, ſhips, *&c.* from the ſtroke of lightning, by directing us to fix on the higheſt parts of thoſe edifices, upright rods of iron made ſharp as a needle, and gilt to prevent ruſting, and from the foot of thoſe rods a wire down the outſide of the building into the ground, or down round one of the ſhrouds of a ſhip, and down her ſide till it reaches the water? Would not theſe pointed rods probably draw the electrical fire ſilently out of a cloud before it came nigh enough to ſtrike, and thereby ſecure us from that moſt ſudden and terrible miſchief?

21. To determine the queſtion, whether the clouds that contain lightning are electrified or not, I would propoſe an experiment to be try'd where it may be done conveniently. On the top of ſome high tower or ſteeple, place a kind of centry-box (as in Fig. 9.) big enough to contain a man and an electrical ſtand. From the middle of the ſtand let an iron rod riſe and paſs bending out of the door, and then upright 20 or 30 feet, pointed very ſharp at the end. If the electrical ſtand be kept clean and dry, a man ſtanding on it when ſuch clouds are paſſing low, might be electrified and afford ſparks, the rod drawing fire to him from a cloud. If any danger to the man ſhould be apprehended (though I think there would be none) let him ſtand on the floor of his box, and now and then bring near to the rod the loop of a wire that has one end faſtened to the leads, he holding it by a wax handle; ſo the ſparks, if the rod is electrified, will ſtrike from the rod to the wire, and not affect him.

22. Before I leave this ſubject of lightning, I may mention ſome other ſimilarities between the effects of that, and thoſe of electricity. Lightning has often been known to ſtrike people blind. A pigeon that we ſtruck dead to appearance by the electrical ſhock, recovering life, drooped about the yard ſeveral days, eat nothing, though crumbs were thrown to it, but declined and died. We did not think of its being deprived of ſight; but afterwards a pullet

pullet ſtruck dead in like manner, being recovered by repeatedly blowing into its lungs, when ſet down on the floor, ran headlong againſt the wall, and on examination appeared perfectly blind. Hence we concluded that the pigeon alſo had been abſolutely blinded by the ſhock. The biggeſt animal we have yet killed, or tried to kill, with the electrical ſtroke, was a well-grown pullet.

23. Reading in the ingenious Dr *Miles*'s account of the thunder ſtorm at *Stretham*, the effect of the lightning in ſtripping off all the paint that had covered a gilt moulding of a pannel of wainſcot, without hurting the reſt of the paint, I had a mind to lay a coat of paint over the filletting of gold on the cover of a book, and try the effect of a ſtrong electrical flaſh ſent through that gold from a charged ſheet of glaſs. But having no paint at hand, I paſted a narrow ſtrip of paper over it; and when dry, ſent the flaſh through the gilding, by which the paper was torn off from end to end, with ſuch force, that it was broke in ſeveral places, and in others brought away part of the grain of the Turky-leather in which it was bound; and convinced me, that had it been painted, the paint would have been ſtript off in the ſame manner with that on the wainſcot at *Stretham*.

24. Lightning melts metals, and I hinted in my paper on that ſubject, that I ſuſpected it to be a cold fuſion; I do not mean a fuſion by force of cold, but a fuſion without heat *. We have alſo melted gold, ſilver, and

* See note in page 49.

copper,

copper, in ſmall quantities, by the electrical flaſh. The manner is this: Take leaf gold, leaf ſilver, or leaf gilt copper, commonly called leaf braſs, or *Dutch* gold; cut off from the leaf long narrow ſtrips, the breadth of a ſtraw. Place one of theſe ſtrips between two ſtrips of ſmooth glaſs that are about the width of your finger. If one ſtrip of gold, the length of the leaf, be not long enough for the glaſs, add another to the end of it, ſo that you may have a little part hanging out looſe at each end of the glaſs. Bind the pieces of glaſs together from end to end with ſtrong ſilk thread; then place it ſo as to be part of an electrical circuit, (the ends of gold hanging out being of uſe to join with the other parts of the circuit) and ſend the flaſh through it, from a large electrified jar or ſheet of glaſs. Then if your ſtrips of glaſs remain whole, you will ſee that the gold is miſſing in ſeveral places, and inſtead of it a metallic ſtain on both the glaſſes; the ſtains on the upper and under glaſs exactly ſimilar in the minuteſt ſtroke, as may be ſeen by holding them to the light; the metal appeared to have been not only melted, but even vitrified, or otherwiſe ſo driven into the pores of the glaſs, as to be protected by it from the action of the ſtrongeſt *Aqua Fortis*, or *Aqua Regia*. I ſend you encloſed two little pieces of glaſs with theſe metallic ſtains upon them, which cannot be removed without taking part of the glaſs with them. Sometimes the ſtain ſpreads a little wider than the breadth of the leaf, and looks brighter at the edge, as by inſpecting cloſely

you

you may obſerve in theſe. Sometimes the glaſs breaks to pieces; once the upper glaſs broke into a thouſand pieces, looking like coarſe ſalt. Theſe pieces I ſend you were ſtain'd with *Dutch* gold. True gold makes a darker ſtain, ſomewhat reddiſh; ſilver, a greeniſh ſtain. We once took two pieces of thick looking-glaſs, as broad as a *Gunter's* ſcale, and ſix inches long; and placing leaf-gold between them, put them between two ſmoothly plain'd pieces of wood, and fix'd them tight in a book-binder's ſmall preſs; yet though they were ſo cloſely confined, the force of the electrical ſhock ſhivered the glaſs into many pieces. The gold was melted, and ſtain'd into the glaſs, as uſual. The circumſtances of the breaking of the glaſs differ much in making the experiment, and ſometimes it does not break at all: but this is conſtant, that the ſtains in the upper and under pieces are exact counterparts of each other. And though I have taken up the pieces of glaſs between my fingers immediately after this melting, I never could perceive the leaſt warmth in them.

25. In one of my former papers, I mentioned, that, gilding on a book, though at firſt it communicated the ſhock perfectly well, yet failed after a few experiments, which we could not account for. We have ſince found that one ſtrong ſhock breaks the continuity of the gold in the filletting, and makes it look rather like duſt of gold, abundance of its parts being broken and driven off; and it will ſeldom conduct above one ſtrong ſhock. Perhaps this may be the reaſon: When there is not a perfect continuity

continuity in the circuit, the fire muſt leap over the vacancies: There is a certain diſtance which it is able to leap over according to its ſtrength; if a number of ſmall vacancies, though each be very minute, taken together exceed that diſtance, it cannot leap over them, and ſo the ſhock is prevented.

26. From the before-mentioned law of electricity, that points as they are more or leſs acute, draw on and throw off the electrical fluid with more or leſs power, and at greater or leſs diſtances, and in larger or ſmaller quantities in the ſame time, we may ſee how to account for the ſituation of the leaf of gold ſuſpended between two plates, the upper one continually electrified, the under one in a perſon's hand ſtanding on the floor. When the upper plate is electrified, the leaf is attracted, and raiſed towards it, and would fly to that plate, were it not for its own points. The corner that happens to be uppermoſt when the leaf is riſing, being a ſharp point, from the extream thinneſs of the gold, draws and receives at a diſtance a ſufficient quantity of the electric fluid to give itſelf an electric atmoſphere, by which its progreſs to the upper plate is ſtopt, and it begins to be repelled from that plate, and would be driven back to the under plate, but that its loweſt corner is likewiſe a point, and throws off or diſcharges the overplus of the leaf's atmoſphere, as faſt as the upper corner draws it on. Were theſe two points perfectly equal in acuteneſs, the leaf would take place exactly in the middle ſpace, for

for its weight is a trifle, compared to the power acting on it: But it is generally neareſt the unelectrified plate, becauſe, when the leaf is offered to the electrified plate, at a diſtance, the ſharpeſt point is commonly firſt affected and raiſed towards it; ſo *that* point, from its greater acuteneſs, receiving the fluid faſter than its oppoſite can diſcharge it at equal diſtances, it retires from the electrified plate, and draws nearer to the unelectrified plate, till it comes to a diſtance where the diſcharge can be exactly equal to the receipt, the latter being leſſened, and the former encreaſed; and there it remains as long as the globe continues to ſupply freſh electrical matter. This will appear plain, when the difference of acuteneſs in the corners is made very great. Cut a piece of *Dutch* gold (which is fitteſt for theſe experiments on account of its greater ſtrength) into the form of FIG. 10. the upper corner a right angle, the two next obtuſe angles, and the loweſt a very acute one; and bring this on your plate under the electrified plate, in ſuch a manner as that the right-angled part may be firſt raiſed (which is done by covering the acute part with the hollow of your hand) and you will ſee this leaf take place much nearer to the upper than the under plate; becauſe without being nearer, it cannot receive ſo faſt at its right-angled point, as it can diſcharge at its acute one. Turn this leaf with the acute part uppermoſt, and then it takes place neareſt the unelectrified plate; becauſe, otherwiſe, it receives faſter at its acute point than it can diſcharge

at

at its right-angled one. Thus the difference of diſtance is always proportioned to the difference of acuteneſs. Take care in cutting your leaf, to leave no little ragged particles on the edges, which ſometimes form points where you would not have them. You may make this figure ſo acute below, and blunt above, as to need no under plate, it diſcharging faſt enough into the air. When it is made narrower, as the figure between the pricked lines, we call it the *Golden Fiſh*, from its manner of acting. For if you take it by the tail, and hold it at a foot or greater horizontal diſtance from the prime conductor, it will, when let go, fly to it with a briſk but wavering motion, like that of an eel through the water; it will then take place under the prime conductor, at perhaps a quarter or half an inch diſtance, and keep a continual ſhaking of its tail like a fiſh, ſo that it ſeems animated. Turn its tail towards the prime conductor, and then it flies to your finger, and ſeems to nibble it. And if you hold a plate under it at ſix or eight inches diſtance, and ceaſe turning the globe, when the electrical atmoſphere of the conductor grows ſmall, it will deſcend to the plate and ſwim back again ſeveral times with the ſame fiſh-like motion, greatly to the entertainment of ſpectators. By a little practice in blunting or ſharpening the heads or tails of theſe figures, you may make them take place as deſired, nearer or farther from the electrified plate.

27. It is ſaid in Section 8, of this paper, that all kinds of common matter are ſuppoſed not to attract the electrical

fluid

fluid with equal ſtrength; and that thoſe called electrics *per ſe*, as glaſs, *&c.* attract and retain it ſtrongeſt, and contain the greateſt quantity. This latter poſition may ſeem a paradox to ſome, being contrary to the hitherto received opinion; and therefore I ſhall now endeavour to explain it.

28. In order to this, let it firſt be conſider'd, *that we cannot by any means we are yet acquainted with, force the electrical fluid thro' glaſs.* I know it is commonly thought that it eaſily prevades glaſs; and the experiment of a feather ſuſpended by a thread, in a bottle hermetically ſealed, yet moved by bringing a rubbed tube near the outſide of the bottle, is alledged to prove it. But, if the electrical fluid ſo eaſily pervades glaſs, how does the vial become *charged* (as we term it) when we hold it in our hands? Would not the fire thrown in by the wire, paſs through to our hands, and ſo eſcape into the floor? Would not the bottle in that caſe be left juſt as we found it, uncharged, as we know a metal bottle ſo attempted to be charged would be? Indeed, if there be the leaſt crack, the minuteſt ſolution of continuity in the glaſs, though it remains ſo tight that nothing elſe we know of will paſs, yet the extremely ſubtile electric fluid flies through ſuch a crack with the greateſt freedom, and ſuch a bottle we know can never be charged: What then makes the difference between ſuch a bottle and one that is ſound, but this, that the fluid can paſs through the one, and not through the other*?

* See the firſt ſixteen Sections of the former paper, called *Farther Experiments, &c.*

29. It is true, there is an experiment that at firft fight would be apt to fatisfy a flight obferver, that the fire thrown into the bottle by the wire, does really pafs thro' the glafs. It is this: place the bottle on a glafs ftand, under the prime conductor; fufpend a bullet by a chain from the prime conductor, till it comes within a quarter of an inch right over the wire of the bottle; place your knuckle on the glafs ftand, at juft the fame diftance from the coating of the bottle, as the bullet is from its wire. Now let the globe be turned, and you fee a fpark ftrike from the bullet to the wire of the bottle, and the fame inftant you fee and feel an exactly equal fpark ftriking from the coating on your knuckle, and fo on, fpark for fpark. This looks as if the whole received by the bottle was again difcharged from it. And yet the bottle by this means is charged!* And therefore the fire that thus leaves the bottle, though the fame in quantity, cannot be the very fame fire that entered at the wire, for if it were, the bottle would remain uncharged.

30. If the fire that fo leaves the bottle be not the fame that is thrown in through the wire, it muft be fire that fubfifted in the bottle, (that is, in the glafs of the bottle) before the operation began.

31. If fo, there muft be a great quantity in glafs, becaufe a great quantity is thus difcharged, even from very thin glafs.

* See Sect. 10, of *Farther Experiments, &c.*

32. That

32. That this electrical fluid or fire is ſtrongly attracted by glaſs, we know from the quickneſs and violence with which it is reſumed by the part that had been deprived of it, when there is an opportunity. And by this, that we cannot from a maſs of glaſs, draw a quantity of electric fire, or electrify the whole maſs *minus*, as we can a maſs of metal. We cannot leſſen or increaſe its whole quantity, for the quantity it has it holds; and it has as much as it can hold. Its pores are filled with it as full as the mutual repellency of the particles will admit; and what is already in, refuſes, or ſtrongly repels, any additional quantity. Nor have we any way of moving the electrical fluid in glaſs, but one; that is, by covering part of the two ſurfaces of thin glaſs with non-electrics, and then throwing an additional quantity of this fluid on one ſurface, which ſpreading in the non-electric, and being bound by it to that ſurface, acts by its repelling force on the particles of the electrical fluid contained in the other ſurface, and drives them out of the glaſs into the non-electric on that ſide, from whence they are diſcharged, and then thoſe added on the charged ſide can enter. But when this is done, there is no more in the glaſs, nor leſs than before, juſt as much having left it on one ſide as it received on the other.

33. I feel a want of terms here, and doubt much whether I ſhall be able to make this part intelligible. By the word *ſurface*, in this caſe, I do not mean mere length and breadth without thickneſs; but when I ſpeak of the upper or under ſurface of a piece of glaſs, the outer or in-

ner ſurface of the vial, I mean length, breadth, and half the thickneſs, and beg the favour of being ſo underſtood. Now, I ſuppoſe, that glaſs in its firſt principles, and in the furnace, has no more of this electrical fluid than other common matter: That when it is blown, as it cools, and the particles of common fire leave it, its pores become a vacuum: That the component parts of glaſs are extremely ſmall and fine, I gueſs from its never ſhowing a rough face when it breaks, but always a poliſh; and from the ſmallneſs of its particles I ſuppoſe the pores between them muſt be exceeding ſmall, which is the reaſon that aqua-fortis, nor any other menſtruum we have, can enter to ſeparate them and diſſolve the ſubſtance; nor is any fluid we know of, fine enough to enter, except common fire, and the electric fluid. Now the departing fire leaving a vacuum, as aforeſaid, between theſe pores, which air nor water are fine enough to enter and fill, the electric fluid, (which is every where ready in what we call the non-electrics, and in the non-electric mixtures that are in the air) is attracted in; yet does not become fixed with the ſubſtance of the glaſs, but ſubſiſts there as water in a porous ſtone, retained only by the attraction of the fixed parts, itſelf ſtill looſe and a fluid. But I ſuppoſe farther, that in the cooling of the glaſs, its texture becomes cloſeſt in the middle, and forms a kind of partition, in which the pores are ſo narrow, that the particles of the electrical fluid, which enter both ſurfaces at the ſame time, cannot go through, or paſs and repaſs from one ſurface to the

other,

other, and ſo mix together; yet, though the particles of electric fluid, imbibed by each ſurface, cannot themſelves paſs through to thoſe of the other, their repellency can, and by this means they act on one another. The particles of the electric fluid have a mutual repellency, but by the power of attraction in the glaſs they are condenſed or forced nearer to each other. When the glaſs has received, and, by its attraction, forced cloſer together ſo much of this electric fluid, as that the power of attracting and condenſing in the one, is equal to the power of expanſion in the other, it can imbibe no more, and that remains its conſtant whole quantity; but each ſurface would receive more, if the repellency of what is in the oppoſite ſurface did not reſiſt its entrance. The quantities of this fluid in each ſurface being equal, their repelling action on each other is equal; and therefore thoſe of one ſurface cannot drive out thoſe of the other; but, if a greater quantity is forced into one ſurface than the glaſs would naturally draw in, this increaſes the repelling power on that ſide, and overpowering the attraction on the other, drives out part of the fluid that had been imbibed by that ſurface, if there be any non-electric ready to receive it: ſuch there is in all caſes where glaſs is electrified to give a ſhock. The ſurface that has been thus emptied by having its electrical fluid driven out, reſumes again an equal quantity with violence, as ſoon as the glaſs has an opportunity to diſcharge that over quantity more than it could retain by attraction in its other ſurface, by the additional repellency of which

the

the vacuum had been occaſioned. For experiments favouring (if I may not ſay confirming) this hypotheſis, I muſt, to avoid repetition, beg leave to refer you back to what is ſaid of the electrical phial in my former papers.

34. Let us now ſee how it will account for ſeveral other appearances.—Glaſs, a body extremely elaſtic (and perhaps its elaſticity may be owing in ſome degree to the ſubſiſting of ſo great a quantity of this repelling fluid in its pores) muſt, when rubbed, have its rubbed ſurface ſomewhat ſtretched, or its ſolid parts drawn a little farther aſunder, ſo that the vacancies in which the electrical fluid reſides, become larger, affording room for more of that fluid, which is immediately attracted into it from the cuſhion or hand rubbing, they being ſupplied from the common ſtock. But the inſtant the parts of the glaſs, ſo opened and filled, have paſſed the friction, they cloſe again, and force the additional quantity out upon the ſurface, where it muſt reſt till that part comes round to the cuſhion again, unleſs ſome non-electric (as the prime-conductor) firſt preſents to receive it *. But if the inſide of the globe be lined with a non-electric, the additional repellency of the electrical fluid, thus collected by

* In the dark the electric fluid may be ſeen on the cuſhion in two ſemi-circles or half-moons, one on the fore part, the other on the back part of the cuſhion, juſt where the globe and cuſhion ſeparate. In the fore creſcent the fire is paſſing out of the cuſhion into the glaſs; in the other it is leaving the glaſs, and returning into the back part of the cuſhion. When the prime conductor is apply'd to take it off the glaſs, the back creſcent diſappears.

by friction on the rubb'd part of the globe's outer surface, drives an equal quantity out of the inner surface into that non-electric lining, which receiving it, and carrying it away from the rubb'd part into the common mass, through the axis of the globe, and frame of the machine, the new collected electrical fluid can enter and remain in the outer surface, and none of it (or a very little) will be received by the prime conductor. As this charg'd part of the globe comes round to the cushion again, the outer surface delivers its overplus fire into the cushion, the opposite inner surface receiving at the same time an equal quantity from the floor. Every electrician knows that a globe wet within will afford little or no fire, but the reason has not before been attempted to be given, that I know of.

34. So if a tube lined with a * non-electric, be rubb'd, little or no fire is obtained from it. What is collected from the hand in the downward rubbing stroke, entering the pores of the glass, and driving an equal quantity out of the inner surface into the non-electric lining: and the hand in passing up to take a second stroke, takes out again what had been thrown into the outer surface, and then the inner surface receives back again what it had given to the non-electric lining. Thus the particles of electrical fluid belonging to the inside surface go in and out of their pores every stroke given to the tube. Put a

* Gilt Paper, with the gilt face next the glass, does well.

wire

wire into the tube, the inward end in contact with the non-electric lining, so it will represent the *Leyden* bottle. Let a second person touch the wire while you rub, and the fire driven out of the inward surface when you give the stroke, will pass through him into the common mass, and return through him when the inner surface resumes its quantity, and therefore this new kind of *Leyden* bottle cannot be so charged. But thus it may: after every stroke, before you pass your hand up to make another, let the second person apply his finger to the wire, take the spark, and then withdraw his finger; and so on till he has drawn a number of sparks; thus will the inner surface be exhausted, and the outer surface charged; then wrap a sheet of gilt paper close round the outer surface, and grasping it in your hand you may receive a shock by applying the finger of the other hand to the wire: for now the vacant pores in the inner surface resume their quantity, and the overcharg'd pores in the outer surface discharge that overplus; the equilibrium being restored through your body, which could not be restored through the glass *. If the tube be exhausted of air, a non-electric lining, in contact with the wire, is not necessary; for *in vacuo*, the electrical fire will fly freely from the inner surface, without a non-electric conductor: but air resists in motion; for being itself an electric *per*

* See *Farther Experiments*, Sect. 15.

se,

ſe, it does not attract it, having already its quantity. So the air never draws off an electric atmoſphere from any body, but in proportion to the non-electrics mix'd with it: it rather keeps ſuch an atmoſphere confin'd, which from the mutual repulſion of its particles, tends to diſſipation, and would immediately diſſipate *in vacuo.*—And thus the experiment of the feather incloſed in a glaſs veſſel hermetically ſealed, but moving on the approach of the rubbed tube, is explained: When an additional quantity of the electrical fluid is applied to the ſide of the veſſel by the atmoſphere of the tube, a quantity is repelled and driven out of the inner ſurface of that ſide into the veſſel, and there affects the feather, returning again into its pores, when the tube with its atmoſphere is withdrawn; not that the particles of that atmoſphere did themſelves paſs through the glaſs to the feather.—And every other appearance I have yet ſeen, in which glaſs and electricity are concerned, are, I think, explained with equal eaſe by the ſame hypotheſis. Yet, perhaps, it may not be a true one, and I ſhall be obliged to him that affords me a better.

35. Thus I take the difference between non electrics, and glaſs, an electric *per ſe*, to conſiſt in theſe two particulars. 1ſt, That a non-electric eaſily ſuffers a change in the quantity of the electric fluid it contains. You may leſſen its whole quantity, by drawing out a part, which the whole body will again reſume; but of glaſs you can only leſſen the quantity contained in one of its

ſurfaces; and not that, but by ſupplying an equal quantity at the ſame time to the other ſurface; ſo that the whole glaſs may always have the ſame quantity in the two ſurfaces, their two different quantities being added together. And this can only be done in glaſs that is thin; beyond a certain thickneſs we have yet no power that can make this change. And, 2dly, that the electric fire freely removes from place to place, in and through the ſubſtance of a non-electric, but not ſo through the ſubſtance of glaſs. If you offer a quantity to one end of a long rod of metal, it receives it, and when it enters, every particle that was before in the rod, puſhes its neighbour quite to the further end, where the overplus is diſcharged; and this inſtantaneouſly where the rod is part of the circle in the experiment of the ſhock. But glaſs, from the ſmallneſs of its pores, or ſtronger attraction of what it contains, refuſes to admit ſo free a motion; a glaſs rod will not conduct a ſhock, nor will the thinneſt glaſs ſuffer any particle entering one of its ſurfaces to paſs through to the other.

36. Hence we ſee the impoſſibility of ſucceſs in the experiments propoſed, to draw out the effluvial virtues of a non-electric, as cinnamon for inſtance, and mixing them with the electric fluid, to convey them with that into the body, by including it in the globe, and then applying friction, *&c.* For though the effluvia of cinnamon, and the electric fluid ſhould mix within the globe, they would never come out together through the pores of the glaſs,

glass, and so go to the prime conductor; for the electric fluid itself cannot come through; and the prime conductor is always supply'd from the cushion, and that from the floor. And besides, when the globe is filled with cinnamon, or other non-electric, no electric fluid can be obtained from its outer surface, for the reason before-mentioned. I have tried another way, which I thought more likely to obtain a mixture of the electric and other effluvia together, if such a mixture had been possible. I placed a glass plate under my cushion, to cut off the communication between the cushion and floor; then brought a small chain from the cushion into a glass of oil of turpentine, and carried another chain from the oil of turpentine to the floor, taking care that the chain from the cushion to the glass, touch'd no part of the frame of the machine. Another chain was fixed to the prime conductor, and held in the hand of a person to be electrised. The ends of the two chains in the glass were near an inch distant from each other, the oil of turpentine between. Now the globe being turned, could draw no fire from the floor through the machine, the communication that way being cut off by the thick glass plate under the cushion: it must then draw it through the chains whose ends were dipped in the oil of turpentine. And as the oil of turpentine, being an electric *per se*, would not conduct, what came up from the floor was obliged to jump from the end of one chain to the end of the other, through the substance of that oil, which we could ſee in large ſparks, and ſo it had a fair opportunity of ſeizing ſome of the fineſt particles of the oil in its paſſage, and carrying them off with it: but no ſuch effect followed, nor could I perceive the leaſt difference in the ſmell of the electric effluvia thus collected, from what it has when collected otherwiſe, nor does it otherwiſe affect the body of a perſon electriſed. I likewiſe put into a phial, inſtead of water, a ſtrong purgative liquid, and then charged the phial, and took repeated ſhocks from it, in which caſe every particle of the electrical fluid muſt, before it went through my body, have firſt gone through the liquid when the phial is charging, and returned through it when diſcharging, yet no other effect followed than if it had been charged with water. I have alſo ſmelt the electric fire when drawn thro' gold, ſilver, copper, lead, iron, wood, and the human body, and could perceive no difference; the odour is always the ſame where the ſpark does not burn what it ſtrikes; and therefore I imagine it does not take that ſmell from any quality of the bodies it paſſes through. And indeed, as that ſmell ſo readily leaves the electric matter, and adheres to the knuckle receiving the ſparks, and to other things; I ſuſpect that it never was connected with it, but ariſes inſtantaneouſly from ſomething in the air acted upon by it. For if it was fine enough to come with the electric fluid through the body of one perſon, why ſhould it ſtop on the ſkin of another?

But

But I ſhall never have done, if I tell you all my conjectures, thoughts, and imaginations on the nature and operations of this electric fluid, and relate the variety of little experiments we have tried. I have already made this paper too long, for which I muſt crave pardon, not having now time to make it ſhorter. I ſhall only add, that as it has been obſerved here that ſpirits will fire by the electric ſpark in the ſummer time, without heating them, when *Fahrenheit*'s thermometer is above 70; ſo when colder, if the operator puts a ſmall flat bottle of ſpirits in his boſom, or a cloſe pocket, with the ſpoon, ſome little time before he uſes them, the heat of his body will communicate warmth more than ſufficient for the purpoſe.

ADDITIONAL EXPERIMENT:

Proving that the Leyden Bottle *has no more electrical Fire in it when charged, than before; nor less when discharged: That, in discharging, the Fire does not issue from the Wire and the Coating at the same Time, as some have thought, but that the Coating always receives what is discharged by the Wire, or an equal Quantity; the outer Surface being always in a negative State of Electricity, when the inner Surface is in a positive State.*

PLACE a thick plate of glass under the rubbing cushion, to cut off the communication of electrical fire from the floor to the cushion; then, if there be no fine points or hairy threads sticking out from the cushion, or from the parts of the machine opposite to the cushion, (of which you must be careful) you can get but a few sparks from the prime conductor, which are all the cushion will part with.

Hang a phial then on the prime conductor, and it will not charge though you hold it by the coating.—But

Form a communication by a chain from the coating to the cushion, and the phial will charge.

For the globe then draws the electric fire out of the outside surface of the phial, and forces it through the prime conductor and wire of the phial, into the inside surface,

Thus

Thus the bottle is charged with its own fire, no other being to be had while the glaſs plate is under the cuſhion.

Hang two cork balls by flaxen threads to the prime conductor; then touch the coating of the bottle, and they will be electrified and recede from each other.

For juſt as much fire as you give the coating, ſo much is diſcharged through the wire upon the prime conductor, whence the cork balls receive an electrical atmoſphere. —But,

Take a wire bent in the form of a C, with a ſtick of wax fixed to the outſide of the curve, to hold it by; and apply one end of this wire to the coating, and the other at the ſame time to the prime conductor, the phial will be diſcharged; and if the balls are not electrified before the diſcharge, neither will they appear to be ſo after the diſcharge, for they will not repel each other.

Now if the fire diſcharged from the inſide ſurface of the bottle through its wire, remained on the prime conductor, the balls would be electrified, and recede from each other.

If the phial really exploded at both ends, and diſcharged fire from both coating and wire, the balls would be *more* electrified, and recede *farther*; for none of the fire can eſcape, the wax handle preventing.

But if the fire, with which the inſide ſurface is ſurcharged, be ſo much preciſely as is wanted by the outſide ſurface, it will paſs round through the wire fixed to the wax handle,

handle, reſtore the equilibrium in the glaſs, and make no alteration in the ſtate of the prime conductor.

Accordingly we find, that if the prime conductor be electrified, and the cork balls in a ſtate of repellency before the bottle is diſcharged, they continue ſo afterwards. If not, they are not electrified by that diſcharge.

LETTER VI.

FROM

BENJ. FRANKLIN, *Esq*; of *Philadelphia*,

TO

PETER COLLINSON, *Esq*; F. R. S. at *London*.

SIR, *July* 27, 1750.

MR *W-ts-n*, I believe, wrote his Observations on my last paper in haste, without having first well considered the Experiments related §. 17. *. which still appear to me decisive in the question,—*Whether the accumulation of the electrical fire be in the electrified glass, or in the non-electric matter connected with the glass?* and to demonstrate that 'tis really in the glass.

As to the experiment that ingenious Gentleman mentions, and which he thinks conclusive on the other side, I persuade myself he will change his opinion of it, when he considers, that as one person applying the wire of the charged bottle to warm spirits, in a spoon held by another per-

* See the Paper entitled, *Farther Experiments*, &c.

person, both ſtanding on the floor, will fire the ſpirits, and yet ſuch firing will not determine whether the accumulation was in the glaſs or the non-electric; ſo the placing another perſon between them, ſtanding on wax, with a baſon in his hand, into which the water from the phial is pour'd, *while he at the inſtant of pouring* preſents a finger of his other hand to the ſpirits, does not at all alter the caſe; the ſtream from the phial, the ſide of the baſon, with the arms and body of the perſon on the wax, being all together but as one long wire, reaching from the internal ſurface of the phial to the ſpirits.

June 29, 1751. In Capt. *Waddell's* account of the effects of lightning on his ſhip, I could not but take notice of the large comazants (as he calls them) that ſettled on the ſpintles at the top-maſt heads, and burnt like very large torches (before the ſtroke). According to my opinion, the electrical fire was then drawing off, as by points, from the cloud; the largeneſs of the flame betokening the great quantity of electricity in the cloud: and had there been a good wire communication from the ſpintle heads to the ſea, that could have conducted more freely than tarred ropes, or maſts of turpentine wood, I imagine there would either have been no ſtroke; or, if a ſtroke, the wire would have conducted it all into the ſea without damage to the ſhip.

His compaſſes loſt the virtue of the load-ſtone, or the poles were reverſed; the North point turning to the South. —By Electricity we have (*here* at *Philadelphia*) frequently given

given polarity to needles, and reverſed it at pleaſure. Mr *Wilſon*, at *London*, tried it on too large maſſes, and with too ſmall force.

A ſhock from four large glaſs jars, ſent through a fine ſewing needle, gives it polarity, and it will traverſe when laid on water.—If the needle when ſtruck lies Eaſt and Weſt, the end entered by the electric blaſt points North. —If it lies North and South, the end that lay towards the North will continue to point North when placed on water, whether the fire entered at that end, or at the contrary end.

The Polarity given is ſtrongeſt when the Needle is ſtruck lying North and South, weakeſt when lying Eaſt and Weſt; perhaps if the force was ſtill greater, the South end, enter'd by the fire, (when the needle lies North and South) might become the North, otherwiſe it puzzles us to account for the inverting of compaſſes by lightning; ſince their needles muſt always be found in that ſituation, and by our little Experiments, whether the blaſt entered the North and went out at the South end of the needle, or the contrary, ſtill the end that lay to the North ſhould continue to point North.

In theſe experiments the ends of the needles are ſometimes finely blued like a watch-ſpring by the electric flame. —This colour given by the flaſh from two jars only, will wipe off, but four jars fix it, and frequently melt the needles. I ſend you ſome that have had their heads and points melted off by our mimic lightning; and a pin that had its point melted off, and ſome part of its head and neck run. Sometimes the ſurface on the body of the needle is alſo run, and appears bliſter'd when examined by a magnifying glaſs: the jars I make uſe of hold 7 or 8 gallons, and are coated and lined with tin foil; each of them takes a thouſand turns* of a globe nine inches diameter to charge it.

I ſend you two ſpecimens of tin-foil melted between glaſs, by the force of two jars only.

I have not heard that any of your *European* electricians have ever been able to fire gunpowder by the electric flame.—We do it here in this manner.—A ſmall cartridge is filled with dry powder, hard rammed, ſo as to bruiſe ſome of the grains; two pointed wires are then thruſt in, one at each end, the points approaching each other in the middle of the cartridge till within the diſtance of half an inch; then, the cartridge being placed in the circle, when the four jars are diſcharged, the electric flame leaping from the point of one wire to the point of the other, within the cartridge amongſt the powder, *fires it*, and the exploſion of the powder is at the ſame inſtant with the crack of the diſcharge.

Yours, &c.

B. FRANKLIN.

* The cuſhion being afterwards covered with a long flap of buckſkin, which might cling to the globe; and care being taken to keep that flap of a due temperature; between too dry and too moiſt, we found ſo much more of the electric fluid was obtained, as that 150 turns were ſufficient. 1753.

LETTER VII.

FROM

BENJ. FRANKLIN, *Esq*; of *Philadelphia*,

TO

C. C. Esq; at *New-York*.

SIR, 1751.

I Inclose you answers, such as my present hurry of business will permit me to make, to the principal queries contained in yours of the 28th instant, and beg leave to refer you to the latter piece in the printed collection of my papers, for farther explanation of the difference between what is called *electrics per se*, and *non electrics*. When you have had time to read and consider these papers, I will endeavour to make any new experiments you shall propose, that you think may afford farther light or satisfaction to either of us; and shall be much obliged to you for such remarks, objections, &c. as may occur to you.—I forget whether I wrote you that I have melted brass pins and steel needles, inverted the poles of the magnetic needle, given a magnetism and polarity to needles

needles that had none, and fired dry gunpowder by the electric ſpark. I have five bottles that contain 8 or 9 gallons each, two of which charg'd, are ſufficient for thoſe purpoſes: but I can charge and diſcharge them altogether. There are no bounds (but what expence and labour give) to the force man may raiſe and uſe in the electrical way: For bottle may be added to bottle *in infinitum* and all united and diſcharged together as one, the force and effect proportioned to their number and ſize. The greateſt known effects of common lightning may, I think, without much difficulty, be exceeded in this way, which a few years ſince could not have been believed, and even now may ſeem to many a little extravagant to ſuppoſe.—So we are got beyond the ſkill of *Rabelais*'s devils of two years old, who, he humorouſly ſays, had only learnt to thunder and lighten a little round the head of a cabbage.

I am, with ſincere reſpect,

Your moſt obliged humble ſervant,

B. FRANKLIN.

Queries *and* Answers *referr'd to in the foregoing Letter.*

Query. Wherein consists the difference between an *electric* and a *non-electric* body?

Answer. The terms electric *per se*, and non-electric, were first used to distinguish bodies, on a mistaken supposition that those called electrics *per se*, alone contained electric matter in their substance, which was capable of being excited by friction, and of being produced or drawn from them, and communicated to those called non-electrics, supposed to be destitute of it: For the glass, &c. being rubbed, discover'd signs of having it, by snapping to the finger, attracting, repelling, &c. and could communicate those signs to metals and water.—Afterwards it was found, that rubbing of glass would not produce the electric matter, unless a communication was preserved between the rubber and the floor; and subsequent experiments proved that the electric matter was really drawn from those bodies that at first were thought to have none in them. Then it was doubted whether glass and other bodies called *electrics per se*, had really any electric matter in them, since they apparently afforded none but what they first extracted from those which had been called non-electrics. But some of my experiments shew that glass contains it in great quantity, and I now suspect it to be pretty equally diffused in all the matter of this terraqueous globe

globe. If so, the terms *electric per se*, and *non electric*, should be laid aside as improper: And (the only difference being this, that some bodies will conduct electric matter, and others will not) the terms *conductor* and *non-conductor* may supply their place. If any portion of electric matter is applied to a piece of conducting matter, it penetrates and flows through it, or spreads equally on its surface; if applied to a piece of non-conducting matter, it will do neither. Perfect conductors of electric matter are only metals and water. Other bodies conducting only as they contain a mixture of those; without more or less of which they will not conduct at all *. This (by the way) shews a new relation between metals and water heretofore unknown.

To illustrate this by a comparison, which, however, can only give a faint resemblance. Electric matter passes through conductors as water passes through a porous stone, or spreads on their surfaces as water spreads on a wet stone; but when applied to non-conductors, it is like water dropt on a greasy stone, it neither penetrates, passes through, nor spreads on the surface, but remains in drops where it falls. See farther on this head in my last printed piece.

Query. What are the effects of air in electrical experiments?

Answer. All I have hitherto observed, are these. Moist air

* This proposition is since found to be too general; Mr *Wilson* having discovered that melted wax and rosin will also conduct.

air receives and conducts the electrical matter in proportion to its moiſture, quite dry air not at all: air is therefore to be claſs'd with the non-conductors. Dry air aſſiſts in confining the electrical atmoſphere to the body it ſurrounds, and prevents its diſſipating: for in vacuo it quits eaſily, and points operate ſtronger, *i. e.* they throw off or attract the electrical matter more freely, and at greater diſtances; ſo that air intervening obſtructs its paſſing from body to body, in ſome degree. A clean electrical phial and wire, containing air inſtead of water, will not be charged nor give a ſhock, any more than if it was fill'd with powder of glaſs; but exhauſted of air it operates as well as if filled with water. Yet, an electric atmoſphere and air do not ſeem to exclude each other, for we breath freely in ſuch an atmoſphere, and dry air will blow through it without diſplacing or driving it away. I queſtion whether the ſtrongeſt dry N. Weſter would diſſipate it. I once electrified a large cork ball, at the end of a ſilk thread three feet long, the other end of which I held in my fingers, and whirl'd it round, like a ſling, 100 times in the air, with the ſwifteſt motion I could poſſibly give it, yet it retained its electric atmoſphere, though it muſt have paſſed through 800 yards of air, allowing my arm in giving the motion to add a foot to the ſemi-diameter of the circle.—By quite dry air, I mean the dryeſt we have: for perhaps we never have any perfectly free from moiſture. An electrical atmoſphere raiſed round a thick wire, inſerted in a phial of air, drives out none of the air, nor on withdrawing that atmoſphere will any air ruſh in, as I have found by a very curious experiment, accurately made, whence we concluded that the air's elaſticity was not affected thereby.

An Experiment towards diſcovering more of the Qualities of the Electric Fluid.

FROM the prime conductor, hang a bullet by a wire-hook; under the bullet at half an inch diſtance, place a bright piece of ſilver to receive the ſparks; then let the wheel be turned, and in a few minutes (if the repeated ſparks continually ſtrike in the ſame ſpot) the ſilver will receive a blue ſtain, near the colour of a watch ſpring.

A bright piece of iron will alſo be ſpotted, but not with that colour; it rather ſeems corroded.

On gold, braſs, or tin, I have not perceived that it makes any impreſſion. But the ſpots on the ſilver or iron will be the ſame, whether the bullet be lead, braſs, gold, or ſilver.

On a ſilver bullet there will alſo appear a ſmall ſpot, as well as on the plate below it.

LETTER VIII.

FROM

Mr E. KINNERSLEY at *Boston*,

TO

BENJAMIN FRANKLIN, Esq; at *Philadelphia*.

SIR, *Feb.* 3, 1752.

I Have the following Experiments to communicate: I held in one hand a wire, which was fastened at the other end to the handle of a pump, in order to try whether the stroke from the prime conductor, through my arms, would be any greater than when conveyed only to the surface of the earth, but could discover no difference.

I placed the needle of a compass on the point of a long pin, and holding it in the atmosphere of the prime conductor, at the distance of about three inches, found it to whirl round like the flyers of a jack, with great rapidity.

I suspended with silk a cork ball, about the bigness of a pea, and presented to it, rubbed amber, sealing-wax, and sulphur, by each of which it was strongly repelled;

then I tried rubbed glaſs and china, and found that each of theſe would attract it, until it became electrified again, and then it would be repelled as at firſt; and while thus repelled by the rubbed glaſs or china, either of the others when rubbed would attract it. Then I electrified the ball, with the wire of a charged phial, and preſented to it rubbed glaſs (the ſtopper of a decanter) and a china tea-cup, by which it was as ſtrongly repelled as by the wire; but when I preſented either of the other rubbed electrics, it would be ſtrongly attracted, and when I electrified it by either of theſe, till it became repelled, it would be attracted by the wire of the phial, but be repelled by its coating.

Theſe experiments ſurprized me very much, and have induced me to infer the following paradoxes.

1. If a glaſs globe be placed at one end of a prime-conductor, and a ſulphur one at the other end, both being equally in good order, and in equal motion, not a ſpark of fire can be obtained from the conductor; but one globe will draw out, as faſt as the other gives in.

2. If a phial be ſuſpended on the conductor, with a chain from its coating to the table, and only one of the globes be made uſe of at a time, 20 turns of the wheel, for inſtance, will charge it; after which, ſo many turns of the other wheel will diſcharge it; and as many more will charge it again.

3. The globes being both in motion, each having a ſeparate conductor, with a phial ſuſpended on one of them, and

and the chain of it faſtened to the other, the phial will become charged; one globe charging poſitively, the other negatively.

4. The phial being thus charged, hang it in like manner on the other conductor; ſet both wheels a going again, and the ſame number of turns that charged it before, will now diſcharge it; and the ſame number repeated, will charge it again.

5. When each globe communicates with the ſame prime conductor, having a chain hanging from it to the table, one of them, when in motion, (but which I can't ſay) will draw fire up through the cuſhion, and diſcharge it through the chain; the other will draw it up through the chain, and diſcharge it through the cuſhion.

I ſhould be glad if you would ſend to my houſe for my ſulphur globe, and the cuſhion belonging to it, and make the trial; but muſt caution you not to uſe chalk on the cuſhion, ſome fine powdered ſulphur will do better. If, as I expect, you ſhould find the globes to charge the prime conductor differently, I hope you will be able to diſcover ſome method of determining which it is that charges poſitively.

I am, &c.

E. KINNERSLEY.

LET-

LETTER IX.

FROM

BENJAMIN FRANKLIN, *Esq*; at *Philadelphia*,

TO

Mr E. KINNERSLEY, at *Boston*.

SIR, *March* 2, 1752.

I Thank you for the Experiments communicated. I sent immediately for your brimstone globe, in order to make the trials you desired, but found it wanted centers, which I have not time now to supply; but the first leisure I will get it fitted for use, try the experiments, and acquaint you with the result.

In the mean time I suspect, that the different attractions and repulsions you observed, proceeded rather from the greater or smaller quantities of the fire you obtained from different bodies, than from its being of a different *kind*, or having a different *direction*. In haste,

I am, &c.

B. FRANKLIN.

LET-

LETTER X.

FROM

BENJAMIN FRANKLIN, *Esq*; of *Philadelphia*,

TO

Mr E. KINNERSLEY, at *Boston*.

SIR, *March* 16, 1752.

HAving brought your brimstone globe to work, I tried one of the experiments you proposed, and was agreeably surprised to find that the glass globe being at one end of the conductor, and the sulphur globe at the other end, both globes in motion, no spark could be obtained from the conductor, unless when one globe turned slower, or was not in so good order as the other; and then the spark was only in proportion to the difference, so that turning equally, or turning that slowest which worked best, would again bring the conductor to afford no spark.

I found also, that the wire of a phial charg'd by the glass globe, attracted a cork ball that had touch'd the wire of a phial charged by the brimstone globe, and *vice versa*,

so

ſo that the cork continued to play between the two phials, juſt as when one phial was charged through the wire, the other through the coating, by the glaſs globe alone. And two phials charged, the one by the brimſtone globe, the other by the glaſs globe, would be both diſcharged by bringing their wires together, and ſhock the perſon holding the phials.

From theſe experiments one may be certain that your 2d, 3d, and 4th propoſed experiments, would ſucceed exactly as you ſuppoſe, though I have not tried them, wanting time.—I imagine it is the glaſs globe that charges poſitively, and the ſulphur negatively, for theſe reaſons, 1. Though the ſulphur globe ſeems to work equally well with the glaſs one, yet it can never occaſion ſo large and diſtant a ſpark between my knuckle and the conductor when the ſulphur one is working, as when the glaſs one is uſed; which, I ſuppoſe, is occaſioned by this, that bodies of a certain bigneſs cannot ſo eaſily part with a quantity of electrical fluid they have and hold attracted *within* their ſubſtance, as they can receive an additional quantity *upon* their ſurface by way of atmoſphere. Therefore ſo much cannot be drawn *out* of the conductor, as can be thrown *on* it. 2. I obſerve that the ſtream or bruſh of fire, appearing at the end of a wire, connected with the conductor, is long, large, and much diverging, when the glaſs globe is uſed, and makes a ſnapping (or rattling) noiſe; but when the ſulphur one is uſed, it is ſhort, ſmall, and makes a hiſſing noiſe; and juſt the re-

verſe

verse of both happens, when you hold the same wire in your hand, and the globes are worked alternately: the brush is large, long, diverging and snapping (or rattling) when the sulphur globe is turn'd; short, small, and hissing when the glass globe is turn'd.—When the brush is long, large, and much diverging, the body to which it joins, seems to me to be throwing the fire out; and when the contrary appears, it seems to be drinking in. 3. I observe, that when I hold my knuckle before the sulphur globe, while turning, the stream of fire between my knuckle and the globe, seems to spread on its surface, as if it flowed from the finger; on the glass globe it is otherwise. 4. The cool wind (or what was called so) that we used to feel as coming from an electrified point, is, I think, more sensible when the glass globe is used, than when the sulphur one.—But these are hasty thoughts. As to your fifth paradox, it must likewise be true, if the globes are alternately worked; but if worked together, the fire will neither come up nor go down by the chain, because one globe will drink it as fast as the other produces it.

I should be glad to know whether the effects would be contrary if the glass globe is solid, and the sulphur globe is hollow; but I have no means at present of trying.

In your journeys, your glass globes meet with accidents, and sulphur ones are heavy and inconvenient. *Query.* Would not a thin plane of brimstone, cast on a board, serve on occasion as a cushion, while a globe of leather ſtuffed (properly mounted) might receive the fire from the ſulphur, and charge the conductor poſitively? Such a globe would be in no danger of breaking*. I think I can conceive how it may be done; but have not time to add more than that I am,

Yours, &c.

B. FRANKLIN.

The preceding LETTERS *having been tranſlated into* French, *and printed at* Paris; *the* Abbe Mazeas, *in a Letter to Dr* Stephen Hales, *dated St* Germain, May 20, 1752, *gives the following Account (printed in the* Philoſophical Tranſactions) *of the Experiment made at* Marly, *in purſuance of that propoſed by Mr* Franklin, *Page* 66.

S I R,

THE *Philadelphian* experiments, that Mr *Collinſon*, a Member of the Royal Society, was ſo kind as to communicate to the public, having been univerſally admired in *France*, the King deſired to ſee them performed. Wherefore the Duke *D'Ayen* offered his Majeſty his country-houſe at St *Germain*, where M. *de Lor*, maſter of Experimental Philoſophy, ſhould put thoſe of *Philadelphia* in execution. His Majeſty ſaw them with great ſatisfaction, and greatly applauded Meſſieurs *Franklin* and *Collinſon*. Theſe applauſes of his Majeſty having excited in Meſ-

* The diſcoveries of the late ingenious Mr *Symmer*, on the poſitive and negative Electricity produced by the mutual friction of white and black ſilk, &c. afford hints for farther improvements to be made with this view.

Meſſieurs *de Buffon*, *D'Alibard*, and *De Lor*, a deſire of verifying the conjectures of Mr *Franklin*, upon the analogy of thunder and electricity, they prepar'd themſelves for making the experiment.

M. *D'Alibard* choſe, for this purpoſe, a garden ſituated at *Marly*, where he placed upon an electrical body a pointed bar of iron, of 40 feet high. On the tenth of *May*, 20 minutes paſt two in the afternoon, a ſtormy cloud having paſſed over the place where the bar ſtood, thoſe that were appointed to obſerve it, drew near, and attracted from it ſparks of fire, perceiving the ſame kind of commotions as in the common electrical Experiments.

M. *de Lor* ſenſible of the good ſucceſs of this experiment reſolved to repeat it at his houſe in the *Eſtrapade* at *Paris*. He raiſed a bar of iron 99 feet high, placed upon a cake of reſin, two feet ſquare, and three inches thick. On the 18th of *May*, between four and five in the afternoon, a ſtormy cloud having paſſed over the bar, where it remained half an hour, he drew ſparks from the bar, like thoſe from the gun barrel, when, in the electrical experiments the globe is only rubbed by the cuſhion, and they produced the ſame noiſe, the ſame fire, and the ſame crackling. They drew the ſtrongeſt ſparks at the diſtance of nine lines, while the rain, mingled with a little hail, fell from the cloud, without either thunder or lightning; this cloud being, according to all appearance, only the conſequence of a ſtorm, which happened elſewhere.

I am, with a profound reſpect,

Your moſt humble and obedient ſervant.

G. MAZEAS.

A LETTER *of Mr* W. WATSON, *F.R.S.* *to the* Royal Society, *concerning the electrical Experiments in* ENGLAND *upon Thunder-Clouds.* Read Dec. 1752. *Transf.* Vol. XLVII.

GENTLEMEN,

AFTER the communications, which we have received from ſeveral of our correſpondents in different parts of the continent, acquainting us with the ſucceſs of their experiments laſt ſummer, in endeavouring to extract the electricity from the atmoſphere during a thunder-ſtorm, in conſequence of Mr *Franklin*'s hypotheſis, it may be thought extraordinary, that no accounts have been yet laid before you, of our ſucceſs here from the ſame experiments. That no want of attention, therefore, may be attributed to thoſe here, who have been hitherto converſant in theſe enquiries, I thought proper to appriſe you, that, though ſeveral members of the Royal Society, as well as myſelf, did, upon the firſt advices from *France*, prepare and ſet up the neceſſary apparatus, for this purpoſe, we were defeated in our expectations, from the uncommon coolneſs and dampneſs of the air here, during the whole ſummer. We had only at *London* one thunder ſtorm; viz. on *July* 20; and then the thunder was accompanied with rain; ſo that, by wetting the apparatus, the electricity was diſſipated too ſoon to be perceived upon touching thoſe parts of the apparatus, which ſerved to

conduct

conduct it. This, I ſay, in general prevented our verifying Mr *Franklin*'s hypotheſis: But our worthy brother Mr *Canton* was more fortunate. I take the liberty, therefore, of laying before you an extract of a letter, which I received from that gentleman, dated from *Spital-ſquare*, *July* 21, 1752.

"I had yeſterday, about five in the afternoon, an op-" portunity of trying Mr. *Franklin*'s experiment of extract-" ing the electrical fire from the clouds; and ſucceeded, " by means of a tin tube, between three and four feet in " length, fixed to the top of a glaſs one, of about eighteen " inches. To the upper end of the tin tube, which was " not ſo high as a ſtack of chimnies on the ſame houſe, " I faſtened three needles with ſome wire; and to the " lower end was ſolder'd a tin cover to keep the rain from " the glaſs tube, which was ſet upright in a block of wood. " I attended this apparatus as ſoon after the thunder began " as poſſible, but did not find it in the leaſt electrified, till " between the third and fourth clap; when applying my " knuckle to the edge of the cover, I felt and heard an " electrical ſpark; and approaching it a ſecond time, I " received the ſpark at the diſtance of about half an inch, " and ſaw it diſtinctly. This I repeated four or five times " in the ſpace of a minute; but the ſparks grew weaker " and weaker; and in leſs than two minutes the tin tube " did not appear to be electriſed at all. The rain con-" tinued during the thunder, but was conſiderably abated " at the time of making the experiment." Thus far Mr *Canton*.

Mr

Mr *Wilson* likewise of the Society, to whom we are much obliged for the trouble he has taken in these pursuits, had an opportunity of verifying Mr *Franklin*'s hypothesis. He informed me, by a letter from near *Chelmsford* in *Essex*, dated *August* 12, 1752, that, on that day about noon, he perceived several electrical snaps, during, or rather at the end of a thunder storm, from no other apparatus than an iron curtain rod, one end of which he put into the neck of a glass phial, and held this phial in his hand. To the other end of the iron he fastened three needles with some silk. This phial, supporting the rod, he held in one hand, and drew snaps from the rod with a finger of his other. This experiment was not made upon any eminence, but in the garden of a gentleman, at whose house he then was.

Dr *Bevis* observed, at Mr *Cave*'s at St *John*'s *Gate*, nearly the same phænomena as Mr *Canton*, of which an account has been already laid before the public.

Trifling as the effects here mentioned are, when compared with those which we have received from *Paris* and *Berlin*, they are the only ones, that the last summer here has produced; and as they were made by persons worthy of credit, they tend to establish the authenticity of those transmitted from our correspondents.

I flatter myself, that this short account of these matters will not be disagreeable to you; and am,

with the most profound Respect,

Your most obedient humble Servant,

W. WATSON.

LETTER XI.

FROM

BENJ. FRANKLIN, *Eſq*; of *Philadelphia*.

Oct. 19, 1752.

AS frequent mention is made in public papers from *Europe* of the ſucceſs of the *Philadelphia* experiment for drawing the electric fire from clouds by means of pointed rods of iron erected on high buildings, &c. it may be agreeable to the curious to be informed that the ſame experiment has ſucceeded in *Philadelphia*, though made in a different and more eaſy manner, which is as follows:

Make a ſmall croſs of two light ſtrips of cedar, the arms ſo long as to reach to the four corners of a large thin ſilk handkerchief when extended; tie the corners of the handkerchief to the extremities of the croſs, ſo you have the body of a kite; which being properly accommodated with a tail, loop, and ſtring, will riſe in the air, like thoſe made of paper; but this being of ſilk, is fitter to bear the wet and wind of a thunder-guſt without tearing. To the top of the upright ſtick of the croſs is to be fixed a very ſharp pointed wire, riſing a foot or more above

above the wood. To the end of the twine, next the hand, is to be tied a ſilk ribbon, and where the ſilk and twine join, a key may be faſtened. This kite is to be raiſed when a thunder guſt appears to be coming on, and the perſon who holds the ſtring muſt ſtand within a door or window, or under ſome cover, ſo that the ſilk ribbon may not be wet; and care muſt be taken that the twine does not touch the frame of the door or window. As ſoon as any of the thunder clouds come over the kite, the pointed wire will draw the electric fire from them, and the kite, with all the twine, will be electrified, and the looſe filaments of the twine will ſtand out every way, and be attracted by an approaching finger. And when the rain has wet the kite and twine, ſo that it can conduct the electric fire freely, you will find it ſtream out plentifully from the key on the approach of your knuckle. At this key the phial may be charged; and from electric fire thus obtained, ſpirits may be kindled, and all the other electric experiments be performed, which are uſually done by the help of a rubbed glaſs globe or tube, and thereby the ſameneſs of the electric matter with that of lightening completely demonſtrated.

B. F.

LETTER XII.

FROM

BENJ. FRANKLIN, *Esq*; of *Philadelphia*,

TO

PETER COLLINSON, *Esq*; F.R.S. *London*.

SIR, *Philadelphia, September* 1753.

IN my former paper on this subject, wrote first in 1747, enlarged and sent to *England* in 1749. I considered the sea as the grand source of lightning, imagining its luminous appearance to be owing to electric fire, produc'd by friction between the particles of water and those of salt. Living far from the sea, I had then no opportunity of making experiments on the sea water, and so embraced this opinion too hastily.

For in 1750 and 1751, being occasionally on the sea coast, I found, by experiments, that sea water in a bottle, tho' at first it would by agitation appear luminous, yet in a few hours it lost that virtue; *hence, and from this*, that I could not by agitating a solution of sea salt in water produce any light, I firſt began to doubt of my former hypotheſis, and to ſuſpect that the luminous appearance in ſea water, muſt be owing to ſome other principles.

I then conſidered whether it were not poſſible, that the particles of air, being electrics *per ſe*, might, in hard gales of wind, by their friction againſt trees, hills, buildings, *&c.* as ſo many minute electric globes, rubbing againſt non-electric cuſhions, draw the electric fire from the earth, and that the riſing vapours might receive that fire from the air, and, by ſuch means, the clouds become electrified.

If this were ſo, I imagined that by forcing a conſtant violent ſtream of air againſt my prime conductor, by bellows, I ſhould electrify it *negatively*; the rubbing particles of air, drawing from it part of its natural quantity of the electric fluid. I accordingly made the experiment, but it did not ſucceed.

In *September* 1752, I erected an iron rod to draw the lightning down into my houſe, in order to make ſome experiments on it, with two bells to give notice when the rod ſhould be electrify'd: A contrivance obvious to every electrician.

I found the bells rang ſometimes when there was no lightning or thunder, but only a dark cloud over the rod; that ſometimes after a flaſh of lightning they would ſuddenly ſtop; and, at other times, when they had not rang before, they would, after a flaſh, ſuddenly begin to ring; that the electricity was ſometimes very faint, ſo that when a ſmall a small spark was obtain'd, another could not be got for some time after; at other times the sparks would follow extremely quick, and once I had a continual stream from bell to bell, the size of a crow-quill: Even during the same gust there were considerable variations.

In the winter following I conceived an experiment, to try whether the clouds were electrify'd *positively* or *negatively*; but my pointed rod, with its apparatus, becoming out of order, I did not refit it till towards the spring, when I expected the warm weather would bring on more frequent thunder-clouds.

The experiment was this: To take two phials; charge one of them with lightning from the iron rod, and give the other an equal charge by the electric glass globe, thro' the prime conductor: When charg'd, to place them on a table within three or four inches of each other, a small cork ball being suspended by a fine silk thread from the ceiling, so as it might play between the wires. If both bottles then were electrifyed *positively*, the ball being attracted and repelled by one, must be also repell'd by the other. If the one *positively*, and the other *negatively*; then the ball would be attracted and repell'd alternately by each, and continue to play between them as long as any considerable charge remained.

Being very intent on making this experiment, it was no small mortification to me, that I happened to be abroad during two of the greatest thunder-storms we had early in the spring, and tho' I had given orders in my family, that if the bells rang when I was from home, they ſhould catch ſome of the lightning for me in electrical phials, and they did ſo, yet it was moſtly diſſipated before my return, and in ſome of the other guſts, the quantity of lightning I was able to obtain was ſo ſmall, and the charge ſo weak, that I could not ſatisfy myſelf: Yet I ſometimes ſaw what heighten'd my ſuſpicions, and inflamed my curioſity.

At laſt, on the 12th of *April* 1753, there being a ſmart guſt of ſome continuance, I charged one phial pretty well with lightning, and the other equally, as near as I could judge, with electricity from my glaſs globe; and, having placed them properly, I beheld, with great ſurprize and pleaſure, the cork ball play briſkly between them; and was convinced that one bottle was electriſed *negatively*.

I repeated this experiment ſeveral times during the guſt, and in eight ſucceeding guſts, always with the ſame ſucceſs; and being of opinion (for reaſons I formerly gave in my letter to Mr *Kinnerſly*, ſince printed in *London*) that the glaſs globe electriſes *poſitively*, I concluded that the clouds are *always* electriſed *negatively*, or have always in them leſs than their natural quantity of the electric fluid.

Yet notwithſtanding ſo many experiments, it ſeems I concluded too ſoon; for at laſt, *June* the 6th, in a guſt which continued from five o'clock, P. M. to ſeven, I met with one cloud that was electriſed poſitively, tho' ſeveral that paſs'd over my rod before, during the ſame guſt, were in the negative ſtate. This was thus diſcovered:

I had

I had another concurring experiment, which I often repeated, to prove the negative ſtate of the clouds, *viz.* While the bells were ringing, I took the phial charged from the glaſs globe, and applied its wire to the erected rod, conſidering, that if the clouds were electriſed *poſitively*, the rod which received its electricity from them, muſt be ſo too; and then the additional *poſitive* electricity of the phial would make the bells ring faſter:—But, if the clouds were in a *negative* ſtate, they muſt exhauſt the electric fluid from my rod, and bring that into the ſame negative ſtate with themſelves, and then the wire of a poſitively charg'd phial, ſupplying the rod with what it wanted, (which it was obliged otherwiſe to draw from the earth by means of the pendulous braſs ball playing between the two bells) the ringing would ceaſe till the bottle was diſcharg'd.

In this manner I quite diſcharged into the rod ſeveral phials that were charged from the glaſs globe, the electric fluid ſtreaming from the wire to the rod, 'till the wire would receive no ſpark from the finger; and during this ſupply to the rod from the phial, the bells ſtopt ringing; but by continuing the application of the phial wire to the rod, I exhauſted the natural quantity from the inſide ſurface of the ſame phials, or, as I call it, charged them *negatively*.

At length, while I was charging a phial by my glaſs globe, to repeat this experiment, my bells, of themſelves, ſtopt ringing, and, after ſome pauſe, began to ring again.—But now, when I approached the wire of the charg'd phial to the rod, inftead of the ufual ftream that I expected from the wire to the rod, there was no fpark; not even when I brought the wire and the rod to touch; yet the bells continued ringing vigoroufly, which proved to me, that the rod was then *pofitively* electrify'd, as well as the wire of the phial, and equally fo; and, confequently, that the particular cloud then over the rod, was in the fame pofitive ftate. This was near the end of the guft.

But this was a fingle experiment, which, however, deftroys my firft too general conclufion, and reduces me to this: *That the clouds of a thunder-guft are moft commonly in a negative ftate of electricity, but fometimes in a pofitive ftate.*

The latter I believe is rare; for tho' I foon after the laft experiment, fet out on a journey to *Bofton*, and was from home moft part of the fummer, which prevented my making farther trials and obfervations; yet Mr *Kinnerfley* returning from the iflands juft as I left home, purfued the experiments during my abfence, and informs me that he always found the clouds in the *negative* ftate.

So that, for the moft part, in thunder-ftrokes, *'tis the earth that ftrikes into the clouds, and not the clouds that ftrike into the earth.*

Thofe who are vers'd in electric experiments, will eafily conceive, that the effects and appearances muft be nearly the fame in either cafe; the fame explofion, and the fame flafh between one cloud and another, and between the

clouds

clouds and mountains, &c. the ſame rending of trees, walls, &c. which the electric fluid meets with in its paſſage, and the ſame fatal ſhock to animal bodies; and that pointed rods fix'd on buildings, or maſts of ſhips, and communicating with the earth or ſea, muſt be of the ſame ſervice in reſtoring the equilibrium ſilently between the earth and clouds, or in conducting a flaſh or ſtroke, if one ſhould be, ſo as to ſave harmleſs the houſe or veſſel: For points have equal power to throw off, as to draw on the electric fire, and rods will conduct up as well as down.

But tho' the light gained from theſe experiments makes no alteration in the practice, it makes a conſiderable one in the theory. And now we as much need an hypotheſis to explain by what means the clouds become negatively, as before to ſhew how they became poſitively electrified.

I cannot forbear venturing ſome few conjectures on this occaſion: They are what occur to me at preſent, and tho' future diſcoveries ſhould prove them not wholly right, yet they may in the mean time be of ſome uſe, by ſtirring up the curious to make more experiments, and occaſion more exact diſquiſitions.

I conceive then, that this globe of earth and water, with its plants, animals, and buildings, have, diffus'd throughout their ſubſtance, a quantity of the electric fluid, juſt as much as they can contain, which I call the *natural quantity*.

That this natural quantity is not the ſame in all kinds of common matter under the ſame dimenſions, nor in the

ſame

ſame kind of common matter in all circumſtances; but a ſolid foot, for inſtance, of one kind of common matter, may contain more of the electric fluid than a ſolid foot of ſome other kind of common matter; and a pound weight of the ſame kind of common matter may, when in a rarer ſtate, contain more of the electric fluid than when in a denſer ſtate.

For the electric fluid, being attracted by any portion of common matter, the parts of that fluid (which have among themſelves a mutual repulſion) are brought ſo near to each other by the attraction of the common matter that abſorbs them, as that their repulſion is equal to the condenſing power of attraction in common matter; and then ſuch portion of common matter will abſorb no more.

Bodies of different kinds having thus attracted and abſorbed what I call their *natural quantity*, *i. e.* juſt as much of the electric fluid as is ſuited to their circumſtances of denſity, rarity, and power of attracting, do not then ſhow any ſigns of electricity among each other.

And if more electric fluid be added to one of theſe bodies, it does not enter, but ſpreads on the ſurface, forming an atmoſphere; and then ſuch body ſhews ſigns of electricity.

I have in a former paper compar'd common matter to a ſponge, and the electric fluid to water: I beg leave once more to make uſe of the ſame compariſon, to illuſtrate farther my meaning in this particular.

When

When a ſponge is ſomewhat condens'd by being ſqueez-ed between the fingers, it will not receive and retain ſo much water as when in its more looſe and open ſtate.

If *more* ſqueez'd and condens'd, ſome of the water will come out of its inner parts, and flow on the ſurface.

If the preſſure of the fingers be entirely removed, the ſponge will not only reſume what was lately forced out, but attract an additional quantity.

As the ſponge in its rarer ſtate will *naturally* attract and abſorb *more* water, and in its denſer ſtate will *naturally* attract and abſorb *leſs* water; we may call the quantity it attracts and abſorbs in either ſtate, its *natural quantity*, the ſtate being conſidered.

Now what the ſponge is to water, the ſame is water to the electric fluid.

When a portion of water is in its common denſe ſtate, it can hold no more electric fluid than it has; if any be added, it ſpreads on the ſurface.

When the ſame portion of water is rarefy'd into vapour, and forms a cloud, it is then capable of receiving and abſorbing a much greater quantity; there is room for each particle to have an electric atmoſphere.

Thus water, in its rarefy'd ſtate, or in the form of a cloud, will be in a negative ſtate of electricity; it will have leſs than its *natural quantity*; that is, leſs than it is naturally capable of attracting and abſorbing in that ſtate.

Such a cloud, then, coming ſo near the earth as to be within the ſtriking diſtance, will receive from the earth a flaſh of the electric fluid; which flaſh, to ſupply a great extent of cloud, muſt ſometimes contain a very great quantity of that fluid.

Or ſuch a cloud, paſſing over woods of tall trees, may from the points and ſharp edges of their moiſt top leaves, receive ſilently ſome ſupply.

A cloud being by any means ſupply'd from the earth, may ſtrike into other clouds that have not been ſupply'd, or not ſo much ſupply'd; and thoſe to others, till an equilibrium is produc'd among all the clouds that are within ſtriking diſtance of each other.

The cloud thus ſupply'd, having parted with much of what it firſt receiv'd, may require and receive a freſh ſupply from the earth, or from ſome other cloud, which, by the wind, is brought into ſuch a ſituation as to receive it more readily from the earth.

Hence repeated and continual ſtrokes and flaſhes till the clouds have all got nearly their natural quantity as clouds, or till they have deſcended in ſhowers, and are united again with this terraqueous globe, their original.

Thus thunder-clouds are generally in a negative ſtate of electricity compar'd with the earth, agreeable to moſt of our experiments; yet as by one experiment we found a cloud electris'd poſitively, I conjecture that, in that caſe, ſuch cloud, after having received what was, in its rare ſtate, only its *natural quantity*, became compreſs'd by the driving winds, or ſome other means, ſo that part of what it had abſorb'd was forc'd out, and form'd an electric atmoſphere

mosphere around it in its denser state. Hence it was capable of communicating positive electricity to my rod.

To show that a body in different circumstances of dilatation and contraction is capable of receiving and retaining more or less of the electric fluid on its surface, I would relate the following experiment. I placed a clean wine glass on the floor, and on it a small silver can. In the can I put about three yards of brass chain; to one end of which I fastened a silk thread, which went right up to the cieling, where it passed over a pulley, and came down again to my hand, that I might at pleasure draw the chain up out of the can, extending it till within a foot of the cieling, and let it gradually sink into the can again.——From the cieling, by another thread of fine raw silk, I suspended a small light lock of cotton, so as that when it hung perpendicularly, it came in contact with the side of the can.—Then approaching the wire of a charged vial to the can, I gave it a spark, which flow'd round in an electric atmosphere; and the lock of cotton was repelled from the side of the can to the distance of about nine or ten inches. The can would not then receive another spark from the wire of the vial; but as I gradually drew up the chain, the atmosphere of the can diminish'd by flowing over the rising chain, and the lock of cotton accordingly drew nearer and nearer to the can; and then, if I again brought the vial wire near the can, it would receive another spark, and the cotton fly off again to its first distance; and thus, as the chain was drawn higher, the can would receive more ſparks; becauſe the can and extended chain were capable of ſupporting a greater atmoſphere than the can with the chain gather'd up into its belly.——And that the atmoſphere round the can was diminiſhed by raiſing the chain, and increaſed again by lowering it, is not only agreeable to reaſon, ſince the atmoſphere of the chain muſt be drawn from that of the can, when it roſe, and returned to it again when it fell; but was alſo evident to the eye, the lock of cotton always approaching the can when the chain was drawn up, and receding when it was let down again.

Thus we ſee that increaſe of ſurface makes a body capable of receiving a greater electric atmoſphere: But this experiment does not, I own, fully demonſtrate my new hypotheſis; for the braſs and ſilver ſtill continue in their ſolid ſtate, and are not rarefied into vapour, as the water is in clouds. Perhaps ſome future experiments on vapourized water may ſet this matter in a clearer light.

One ſeemingly material objection ariſes to the new hypotheſis, and it is this. If water, in its rarefied ſtate, as a cloud, requires, and will abſord more of the electric fluid than when in its denſe ſtate as water, why does it not acquire from the earth all it wants at the inſtant of its leaving the ſurface, while it is yet near, and but juſt riſing in vapour? To this difficulty I own I cannot at preſent give a ſolution ſatisfactory to myſelf: I thought, how-

however, that I ought to ſtate it in its full force, as I have done, and ſubmit the whole to examination.

And I would beg leave to recommend it to the curious in this branch of natural philoſophy, to repeat with care and accurate obſervation, the experiments I have reported in this and former papers relating to *poſitive* and *negative* electricity, with ſuch other relative ones as ſhall occur to them, that it may be certainly known whether the electricity communicated by a glaſs globe, be *really poſitive*. And alſo I would requeſt all who may have an opportunity of obſerving the recent effects of lightning on buildings, trees, *&c.* that they would conſider them particularly with a view to diſcover the direction. But in theſe examinations, this one thing is always to be underſtood, *viz.* that a ſtream of the electric fluid paſſing thro' wood, brick, metal, *&c.* while ſuch fluid paſſes in *ſmall quantity*, the mutually repulſive power of its parts is confined and overcome by the cohesion of the parts of the body it paſſes thro', ſo as to prevent an exploſion; but when the fluid comes in a quantity too great to be confin'd by ſuch cohesion, it explodes, and rends or fuſes the body that endeavour'd to confine it. If it be wood, brick, ſtone, or the like, the ſplinters will flie off on that ſide where there is leaſt reſiſtance. And thus, when a hole is ſtruck thro' paſteboard by the electrify'd jar, if the ſurfaces of the paſte-board are not confin'd or compreſs'd, there will be a bur rais'd all round the hole on both ſides the paſte-board; but if one ſide be confin'd, ſo that the bur cannot be rais'd on that ſide,

ſide, it will be all rais'd on the other, which way ſoever the fluid was directed. For the bur round the outſide of the hole, is the effect of the exploſion every way from the center of the ſtream, and not an effect of the direction.

In every ſtroke of lightning, I am of opinion that the ſtream of the electric fluid, moving to reſtore the equilibrium between the cloud and the earth, does always previouſly find its paſſage, and mark out, as I may ſay, its own courſe, taking in its way all the conductors it can find, ſuch as metals, damp walls, moiſt wood, *&c.* and will go conſiderably out of a direct courſe, for the ſake of the aſſiſtance of good conductors; and that, in this courſe, it is actually moving, tho' ſilently and imperceptibly, before the exploſion, in and among the conductors; which exploſion happens only when the conductors cannot diſcharge it as faſt as they receive it, by reaſon of their being incompleat, diſunited, too ſmall, or not of the beſt materials for conducting. Metalline rods, therefore, of ſufficient thickneſs, and extending from the higheſt part of an edifice to the ground, being of the beſt materials and compleat conductors, will, I think, ſecure the building from damage, either by reſtoring the equilibrium ſo faſt as to prevent a ſtroke, or by conducting it in the ſubſtance of the rod as far as the rod goes, ſo that there ſhall be no exploſion but what is above its point, between that and the clouds.

If it be aſk'd, what thickneſs of a metalline rod may be ſuppoſed ſufficient? In anſwer, I would remark, that five large glaſs jars, ſuch as I have deſcribed in my former papers,

pers, diſcharge a very great quantity of electricity, which nevertheleſs will be all conducted round the corner of a book, by the fine filletting of gold on the cover, it following the gold the fartheſt way about, rather than take the ſhorter courſe through the cover, that not being ſo good a conductor. Now in this line of gold, the metal is ſo extremely thin as to be little more than the colour of gold, and on an octavo book is not in the whole an inch ſquare, and therefore not the 36th part of a grain according to M. *Reaumur*; yet 'tis ſufficient to conduct the charge of five large jars, and how many more I know not. Now, I ſuppoſe a wire of a quarter an inch diameter to contain about 5000 times as much metal as there is in that gold line, and if ſo, it will conduct the charge of 25,000 ſuch glaſs jarrs, which is a quantity, I imagine, far beyond what was ever contain'd in any one ſtroke of natural lightning. But a rod of half an inch diameter would conduct four times as much as one of a quarter.

And with regard to conducting, tho' a certain thickneſs of metal be required to conduct a great quantity of electricity, and, at the ſame time, keep its own ſubſtance firm and unſeparated; and a leſs quantity, as a very ſmall wire for inſtance, will be deſtroyed by the exploſion; yet ſuch ſmall wire will have anſwered the end of conducting that ſtroke, tho' it become incapable of conducting another. And conſidering the extream rapidity with which the electric fluid moves without exploding, when it has a free paſſage, or compleat metal communication, I ſhould think

think a vaſt quantity would be conducted in a ſhort time, either to or from a cloud, to reſtore its equilibrium with the earth, by means of a very ſmall wire; and therefore thick rods ſhould ſeem not ſo neceſſary.—However, as the quantity of lightning diſcharg'd in one ſtroke, cannot well be meaſured, and, in different ſtrokes, is certainly very various, in ſome much greater than others; and as iron (the beſt metal for the purpoſe, being leaſt apt to fuſe) is cheap, it may be well enough to provide a larger canal to guide that impetuous blaſt, than we imagine neceſſary: For, though one middling wire may be ſufficient, two or three can do no harm. And time, with careful obſervations well compar'd, will at length point out the proper ſize to greater certainty.

Pointed rods erected on edifices may likewiſe often prevent a ſtroke, in the following manner. An eye ſo ſituated as to view horizontally the under ſide of a thunder cloud, will ſee it very ragged, with a number of ſeparate fragments, or petty clouds, one under another, the loweſt ſometimes not far from the earth. Theſe, as ſo many ſtepping-ſtones, aſſiſt in conducting a ſtroke between the cloud and a building. To repreſent theſe by an experiment, take two or three locks of fine looſe cotton, connect one of them with the prime conductor by a fine thread of two inches, (which may be ſpun out of the ſame lock by the fingers) another to that, and the third to the ſecond, by like threads.—Turn the globe, and you will

will ſee theſe locks extend themſelves towards the table, (as the lower ſmall clouds do towards the earth) being attracted by it: But on preſenting a ſharp point erect under the loweſt, it will ſhrink up to the ſecond, the ſecond to the firſt, and all together to the prime conductor, where they will continue as long as the point continues under them. May not, in like manner, the ſmall electrifed clouds, whoſe equilibrium with the earth is ſoon reſtor'd by the point, riſe up to the main body, and by that means occaſion ſo large a vacancy, as that the grand cloud cannot ſtrike in that place?

Theſe thoughts, my dear friend, are many of them crude and haſty; and if I were merely ambitious of acquiring ſome reputation in philoſophy, I ought to keep them by me, till corrected and improved by time and farther experience. But ſince even ſhort hints and imperfect experiments in any new branch of ſcience, being communicated, have oftentimes a good effect, in exciting the attention of the ingenious to the ſubject, and ſo become the occaſion of more exact diſquiſition, and more compleat diſcoveries. You are at liberty to communicate this paper to whom you pleaſe; it being of more importance that knowledge ſhould increaſe, than that your friend ſhould be thought an accurate philoſopher.

LETTER XIII.

FROM

BENJ. FRANKLIN, *Esq*, at *Philadelphia*,

TO

PETER COLLINSON, *Esq*; F.R.S. at *London*.

SIR, *April* 18, 1754.

SINCE *September* laſt, having been abroad on two long journeys, and otherwiſe much engag'd, I have made but few obſervations on the *poſitive* and *negative* ſtate of electricity in the clouds. But Mr *Kinnerſley* kept his rod and bells in good order, and has made many.

Once this winter the bells rang a long time, during a fall of ſnow, tho' no thunder was heard, or lightning ſeen. Sometimes the flaſhes and cracks of the electric matter between bell and bell were ſo large and loud as to be heard all over the houſe: but by all his obſervations, the clouds were conſtantly in a negative ſtate, till about ſix weeks ago, when he found them once to change in a few minutes from the negative to the poſitive. About a fort-

night

night after that he made another obſervation of the ſame kind; and laſt *Monday* afternoon, the wind blowing hard at S. E. and veering round to N. E. with many thick driving clouds, there were five or ſix ſucceſſive changes from negative to poſitive, and from poſitive to negative, the bells ſtopping a minute or two between every change. Beſides the methods mentioned in my paper of *September* laſt, of diſcovering the electrical ſtate of the clouds, the following may be us'd. When your bells are ringing, paſs a rubb'd tube by the edge of the bell, connected with your pointed rod: if the cloud is then in a negative ſtate, the ringing will ſtop; if in a poſitive ſtate, it will continue, and perhaps be quicker. Or, ſuſpend a very ſmall cork-ball by a fine ſilk thread, ſo that it may hang cloſe to the edge of the rod-bell: then whenever the bell is electrified, whether poſitively or negatively, the little ball will be repell'd, and continue at ſome diſtance from the bell. Have ready a round-headed glaſs ſtopper of a decanter, rub it on your ſide till it is electrified, then preſent it to the cork-ball. If the electricity in the ball is poſitive, it will be repell'd from the glaſs ſtopper as well as from the bell. If negative, it will fly to the ſtopper.

REMARKS
On the Abbe NOLLET's
Letters on ELECTRICITY.
TO
BENJ. FRANKLIN, *Esq*; of *Philadelphia*,
BY
Mr DAVID COLDEN of *New-York*.

SIR, *Coldenham, in N. York, Dec.* 4, 1753.

IN considering the Abbe *Nollet*'s letters to Mr *Franklin*, I am obliged to pass by all the experiments which are made with, or in, bottles hermetically sealed, or exhausted of air; because, not being able to repeat the experiments, I could not second any thing which occurs to me thereon, by experimental proof. Wherefore, the first point wherein I can dare to give my opinion, is in the Abbe's 4th letter, *p.* 66, where he undertakes to prove, that the electric matter passes from one surface to another through the intire thickness of the glass: He takes Mr *Franklin*'s experiment of the magical picture, and writes thus of it. " When you electrise a pane of glass coated

" on

"on both ſides with metal, it is evident that whatever is "placed on the ſide oppoſite to that which receives the "electricity from the conductor, receives alſo an evident "electrical virtue." Which Mr *Franklin* ſays, is that equal quantity of electric matter, driven out of this ſide, by what is received from the conductor on the other ſide; and which will continue to give an electrical virtue, to any thing in contact with it, till it is entirely diſcharged of its electrical fire. To which the Abbe thus objects: "Tell me, ſays he, I pray you, how much time is ne- "ceſſary for this pretended diſcharge? I can aſſure you, "that after having maintain'd the electriſation for hours, "this ſurface, which ought, as it ſeems to me, to be en- "tirely diſcharged of its electrical matter, conſidering ei- "ther the vaſt number of ſparks that were drawn from it, "or the time that this matter had been expoſed to the action "of the expulſive cauſe; this ſurface, I ſay, appeared ra- "ther better electriſed thereby, and more proper to pro- "duce all the effects of an actual electric body. *p.* 68."

The Abbe does not tell us what thoſe effects were: all the effects I could never obſerve, and thoſe that are to be obſerved can eaſily be accounted for, by ſuppoſing that ſide to be entirely deſtitute of electric matter. The moſt ſenſible effect of a body charged with electricity is, that when you preſent your finger to it, a ſpark will iſſue from it to your finger: Now when a phial, prepared for the *Leyden* experiment, is hung to the gun-barrel or prime-

con-

conductor, and you turn the globe in order to charge it; as ſoon as the electric matter is excited, you can obſerve a ſpark to iſſue from the external ſurface of the phial to your finger, which, Mr *Franklin* ſays, is the natural electric matter of the glaſs driven out by that received by the inner ſurface from the conductor. If it be only drawn out by ſparks, a vaſt number of them may be drawn; but if you take hold of the external ſurface with your hand, the phial will ſoon receive all the electric matter it is capable of, and the outſide will then be entirely deſtitute of its electric matter, and no ſpark can be drawn from it by the finger: here then is a want of that effect which all bodies, charg'd with electricity, have. Some of the effects of an electric body, which I ſuppoſe the Abbe has obſerved in the exterior ſurface of a charged phial, are that all light bodies are attracted by it. This is an effect which I have conſtantly obſerved, but do not think that it proceeds from an attractive quality in the exterior ſurface of the phial, but in thoſe light bodies themſelves, which ſeem to be attracted by the phial. It is a conſtant obſervation, that when one body has a greater charge of electric matter in it than another (that is in proportion to the quantity they will hold) this body will attract that which has leſs: Now, I ſuppoſe, and it is a part of Mr *Franklin*'s ſyſtem, that all thoſe light bodies which appear to be attracted, have more electric matter in them than the external ſurface of the phial has, wherefore they endeavour to attract the phial

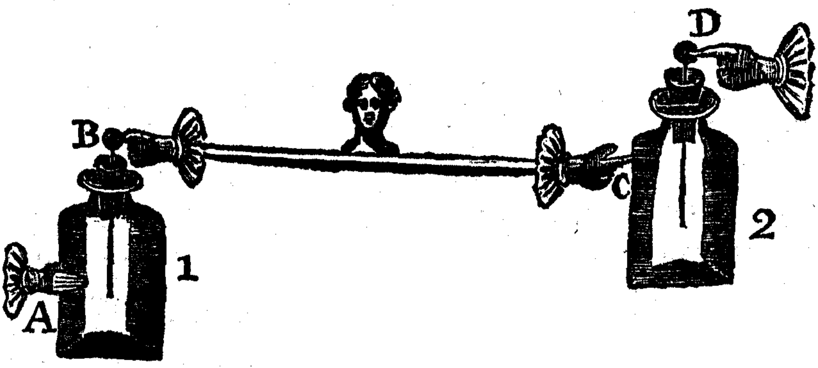
phial to them, which is too heavy to be moved by the ſmall degree of force they exert, and yet being greater than their own weight, moves them to the phial. The following experiment will help the imagination in conceiving this. Suſpend a cork ball, or a feather by a ſilk thread, and electriſe it; then bring this ball nigh to any fixed body, and it will appear to be attracted by that body, for it will fly to it: Now, by the conſent of electricians, the attractive cauſe is in the ball itſelf, and not in the fixed body to which it flies: This is a ſimilar caſe with the apparent attraction of light bodies, to the external ſurface of a charged phial.

The Abbe ſays, *p.* 69. "that he can electriſe a hundred men, ſtanding on wax, if they hold hands, and if one of them touch one of theſe ſurfaces (the exterior) with the end of his finger": This I know he can, while the phial is charging, but after the phial is charged I am as certain he cannot: That is, hang a phial, prepared for the *Leyden* experiment, to the conductor, and let a man, ſtanding on the floor, touch the coating with his finger, while the globe is turn'd, till the electric matter ſpews out of the hook of the phial, or ſome part of the conductor, which I take to be the certaineſt ſign that the phial has received all the electric matter it can: after this appears, let the man, who before ſtood on the floor, ſtep on a cake of wax, where he may ſtand for hours, and the globe all that time turned, and yet have no appearance of being electriſed. After

ter the electric matter was ſpewed out as above from the hook of a phial prepared for the *Lyden* experiment, I hung another phial, in like manner prepared, to a hook fixed in the coating of the firſt, and held this other phial in my hand; now if there was any electric matter tranſmitted thro' the glaſs of the firſt phial, the ſecond one would certainly receive and collect it; but having kept the phials in this ſituation for a conſiderable time, during which the globe was continually turned, I could not perceive that the ſecond phial was in the leaſt charged, for when I touched the hook with my finger, as in the *Leyden* experiment, I did not feel the leaſt commotion, nor perceive any ſpark to iſſue from the hook.

I likewiſe made the following experiment. Having charged two phials (prepared for the *Leyden* experiment) through their hooks; two perſons took each one of theſe phials in their hand; one held his phial by the coating, the other by the hook, which he could do by removing the communication from the bottom before he took hold of the hook. Theſe perſons placed themſelves one on each ſide of me, while I ſtood on a cake of wax, and took hold of the hook of that phial which was held by its coating (upon which a ſpark iſſued, but the phial was not diſcharged, as I ſtood on wax) keeping hold of the hook, I touched the coating of the phial that was held by its hook with my other hand, upon which there was a large ſpark to be ſeen between my finger and the coating, and both phials were inſtantly

inſtantly diſcharged. If the Abbe's opinion be right, that the exterior ſurface, communicating with the coating, is charged, as well as the interior, communicating with the hook; how can I, who ſtand on wax, diſcharge both theſe phials, when it is well known I could not diſcharge one of them ſingly? Nay, ſuppoſe I have drawn the electric matter from both of them, what becomes of it? For I appear to have no additional quantity in me when the experiment is over, and I have not ſtirr'd off the wax: Wherefore this experiment fully convinces me, that the exterior ſurface is not charged; and not only ſo, but that it wants as much electric matter as the inner has of exceſs: For by this ſuppoſition, which is a part of Mr *Franklin*'s ſyſtem, the above experiment is eaſily accounted for, as follows:



When I ſtand on wax, my body is not capable of receiving all the electric matter from the hook of one phial, which it is ready to give; neither can it give as much to the coating of the other phial as it is ready to take, when one is only applied to me: But when both are applied, the coating takes from one what the hook gives: Thus I receive the fire from the firſt phial at B, the exterior ſurface of which is ſupplied from the hand at A: I give the fire to the ſecond phial at C, whoſe interior ſurface is diſcharged by the hand at D. This diſcharge at D may be made evident by receiving that fire into the hook of a third phial, which is done thus: In place of taking the hook of the ſecond phial in your hand, run the wire of a third phial, prepared as for the *Leyden* experiment, through it, and hold this third phial in your hand, the ſecond one hanging to it, by the ends of the hooks run through each other: When the experiment is performed, this third phial receives the fire at D, and will be charged. When this experiment is conſidered, I think, it muſt fully prove that the exterior ſurface of a charged phial wants electric matter, while the inner ſurface has an exceſs of it. One thing more, worthy of notice in this experiment is, that I feel no commotion or ſhock in my arms, tho' ſo great a quantity of electric matter paſſes through them inſtantaneouſly: I only feel a prickling in the ends of my fingers. This makes me think the Abbe has miſtook, when he ſays, that there is no difference between the ſhock felt in performing the *Leyden* experiment, and the prickling felt on drawing ſimple ſparks, except that of greater to leſs. In the laſt experiment, as much electric matter went through my arms, as would have given me a very ſenſible ſhock, had there been an immediate commumunication, by my arms, from the hook to the coating of the ſame phial; becauſe when it was taken into a third phial, and that phial diſcharged ſingly thro' my arms, it gave me a ſenſible ſhock. If theſe experiments prove that the electric matter does not paſs through the intire thickneſs of the glaſs; it is a neceſſary conſequence that it muſt always come out where it enter'd.

The next thing I meet with is in the Abbe's fifth letter *p*. 88, where he differs from Mr *Franklin*, who thinks that the whole power of giving a ſhock is in the glaſs itſelf, and not in the non-electrics in contact with it. The experiments which Mr *Franklin* gave to prove this opinion, in his *Experiments and Obſervations on Electricity, Letter* III. *p*. 24. convinced me that he was in the right; and what the Abbe has aſſerted in contradiction thereto, has not made me think otherwiſe. The Abbe perceiving as I ſuppoſe, that the experiments, as Mr *Franklin* had perform'd them, muſt prove his aſſertion; alters them without giving any reaſon for it, and makes them in a manner that proves nothing. Why will he have the phial, into which the water is to be decanted from a charged phial, held in a man's hand? If the power of giving a ſhock is in the water contain'd in the phial, it ſhould remain there tho' decanted into another phial, ſince no non-electric body touch'd it to take that power off. The phial being placed on wax is no objection, for it cannot take the power from the water, if it had any, but it is a neceſſary means to try the fact; whereas, that phial's being charged when held in a man's hand, only proves that water will conduct the electric matter. The Abbe owns, *p.* 94, that he had heard this remarked, but says, Why is not a conductor of electricity an electric subject? This is not the question; Mr *Franklin* never said that water was not an electric subject; he said, that the power of giving a shock was in the glass, and not in the water; and this, his experiments, fully prove; so fully, that it may appear impertinent to offer any more: Yet as I do not know that the following has been taken notice of by any body before, my inserting of it in this place may be excused. It is this: Hang a phial, prepared for the *Leyden* experiment, to the conductor, by its hook, and charge it, which done, remove the communication from the bottom of the phial. Now the conductor shews evident signs of being electrised; for if a thread be tied round it, and its ends left about two inches long, they will extend themselves out like a pair of horns; but if you touch the conductor, a spark will issue from it, and the threads will fall, nor does the conductor shew the least sign of being electrised after this is done. I think that by this touch, I have taken out all the charge of electric matter that was in the conductor, the hook of the phial, and water or filings of iron contain'd in it; which is no more than we see all non-electric bodies will receive; yet the glass of the phial retains its power of giving a shock, as any one will

find

find that pleafes to try. This experiment fully evidences, that the water in the phial contains no more electric matter than it would do in an open bafon, and has not any of that great quantity which produces the fhock, and is only retain'd by the glafs. If after the fpark is drawn from the conductor, you touch the coating of the phial (which all this while is fuppofed to hang in the air, free from any non-electric body) the threads on the conductor will inftantly ftart up, and fhew that the conductor is electrifed. It receives this electrifation from the inner furface of the phial, which, when the outer furface can receive what it wants from the hand applied to it, will give as much as the bodies in contact with it can receive, or, if they be large enough, all that it has of excefs. It is diverting to fee how the threads will rife and fall by touching the coating and conductor of the phial alternately. May it not be that the difference between the charged fide of the glafs, and the outer or emptied fide, being leffen'd by touching the hook or the conductor; the outer fide can receive from the hand which touched it, and by its receiving the inner fide cannot retain fo much; and for that reafon fo much as it cannot contain electrifes the water, or filings and conductor: For it feems to be a rule, that the one fide muft be emptied in the fame proportion that the other is fill'd: Tho' this from experiment appears evident, yet it is ftill a myftery not to be accounted for.

I am.

I am, in many places of the Abbe's book, ſurpriſed to find that experiments have ſucceeded ſo differently at *Paris* from what they did with Mr *Franklin*, and as I have always obſerv'd them to do. The Abbe, in making experiments to find the difference between the two ſurfaces of a charged glaſs, will not have the phial placed on wax: For, ſays he, don't you know that being placed on a body originally electric, it quickly loſes its virtue? I cannot imagine what ſhould have made the Abbe think ſo; it certainly is contradictory to the notions commonly received of electrics per ſe; and by experiment I find it entirely otherwiſe: For having ſeveral times left a charged phial, for that purpoſe, ſtanding on wax for hours, I found it to retain as much of its charge as another that ſtood at the ſame time on a table. I left one ſtanding on wax from 10 o'clock at night till 8 next morning, when I found it to retain a ſufficient quantity of its charge, to give me a ſenſible commotion in my arms, though the room in which the phial ſtood had been ſwept in that time, which muſt have rais'd much duſt to facilitate the diſcharge of the phial.

I find that a cork ball ſuſpended between two bottles, the one fully and the other but little charged, will not play between them, but is driven into a ſituation that makes a triangle with the hooks of the phials; though the Abbe has aſſerted the contrary of this, *p.* 101, in order to account for the playing of a cork ball between the wire

wire thruſt into the phial, and one that riſes up from its coating. The phial which is leaſt charged muſt have more electric matter given to it, in proportion to its bulk, than the cork ball receives from the hook of the full phial.

The Abbe ſays, *p.* 103, "that a piece of metal leaf "hung to a ſilk thread and electriſed, will be repell'd by "the bottom of a charged phial held by its hook in the "air:" This I find conſtantly otherwiſe, it is with me always firſt attracted and then repelled: It is neceſſary in charging the leaf to be careful, that it does not fly off to ſome non-electric body, and ſo diſcharge itſelf when you think it is charged; it is difficult to keep it from flying to your own wriſt, or to ſome part of your body.

The Abbe, *p.* 108, ſays, "that it is not impoſſible, as "Mr *Franklin* ſays it is, to charge a phial while there is a "communication form'd between its coating and its hook." I have always found it impoſſible to charge ſuch a phial ſo as to give it a ſhock: Indeed if it hang on the conductor without a communication from it, you may draw a ſpark from it as you may from any body that hangs there, but this is very different from being charged in ſuch a manner as to give a ſhock. The Abbe, in order to account for the little quantity of electric matter that is to be found in the phial, ſays, "that it rather follows the metal than the "glaſs, and that it is ſpewed out into the air from the coating "of the phial". I wonder how it comes not to do ſo too, when

when it ſifts through the glaſs, and charges the exterior ſurface, according to the Abbe's ſyſtem!

The Abbe's objections againſt Mr *Franklin*'s two laſt experiments, I think, have little weight in them: He ſeems, indeed, much at a loſs what to ſay, wherefore he taxes Mr *Franklin* with having conceal'd a material part of the experiment; a thing too mean for any gentleman to be charged with, who has not ſhewn as great a partiality in relating experiments, as the Abbe has done.

ELEC-

ELECTRICAL EXPERIMENTS,

With an Attempt to account for their

SEVERAL PHÆNOMENA:

Together with

Some Obſervations on *Thunder-Clouds*,

In further Confirmation of Mr FRANKLIN's Obſervations on the poſitive and negative electrical State of the Clouds, by JOHN CANTON, M. A. and F. R. S.

Dec. 6, 1753.

EXPERIMEN I.

FROM the cieling, or any convenient part of a room, let two cork-balls, each about the bigneſs of a ſmall pea, be ſuſpended by linen threads of eight or nine inches in length, ſo as to be in contact with each other. Bring the excited glaſs tube under the balls, and they will be ſeparated by it, when held at the diſtance of three or four feet; let it be brought nearer, and they will ſtand farther apart; intirely withdraw it, and they will immediately come together. This experiment may be made with very ſmall braſs balls hung by ſilver wire; and will ſucceed as well with ſealing-wax made electrical, as with glaſs.

EXPERIMENT II.

If two cork-balls be ſuſpended by dry ſilk threads, the excited tube muſt be brought within eighteen inches before they will repel each other; which they will continue to do, for ſome time, after the tube is taken away.

As the balls in the firſt experiment are not inſulated, they cannot properly be ſaid to be electrified: but when they hang within the atmoſphere of the excited tube, they may attract and condenſe the electrical fluid round about them, and be ſeparated by the repulſion of its particles. It is conjectur'd alſo, that the balls at this time contain leſs than their common ſhare of the electrical fluid, on account of the repelling power of that which ſurrounds them; tho' ſome, perhaps, is continually entering and paſſing thro' the threads. And if that be the caſe, the reaſon is plain why the balls hung by ſilk, in the ſecond experiment, muſt be in a much more denſe part of the atmoſphere of the tube, before they will repel each other. At the approach of an excited ſtick of wax to the balls, in the firſt experiment, the electrical fire is ſuppoſed to come

come through the threads into the balls, and be condenſed there, in its paſſage towards the wax; for, according to Mr *Franklin*, excited glaſs *emits* the electrical fluid, but excited wax *receives* it.

EXPERIMENT III.

Let a tin tube, of four or five feet in length, and about two inches in diameter, be inſulated by ſilk; and from one end of it let the cork-balls be ſuſpended by linen threads. Electrify it, by bringing the excited glaſs tube near the other end, ſo as that the balls may ſtand an inch and an half, or two inches, a-part: Then, at the approach of the excited tube, they will, by degrees, loſe their repelling power, and come into contact; and as the tube is brought ſtill nearer, they will ſeparate again to as great a diſtance as before: In the return of the tube they will approach each other till they touch, and then repel as at firſt. If the tin tube be electrified by wax, or the wire of a charg'd phial, the balls will be affected in the ſame manner at the approach of excited wax, or the wire of the phial.

EXPERIMENT IV.

Electrify the cork-balls as in the laſt experiment by glaſs, and at the approach of an excited ſtick of wax their repulſion will be increaſed. The effect will be the ſame, if the excited glaſs be brought towards them, when they have been electrified by wax.

The bringing the excited glaſs to the end, or edge of the

the tin-tube, in the third experiment, is ſuppos'd to electrify it poſitively, or to add to the electrical fire it before contained; and therefore ſome will be running off through the balls, and they will repel each other. But at the approach of excited glaſs, which likewiſe *emits* the electrical fluid, the diſcharge of it from the balls will be diminiſh'd; or part will be driven back, by a force acting in a contrary direction; and they will come nearer together. If the tube be held at ſuch a diſtance from the balls, that the exceſs of the denſity of the fluid round about them, above the common quantity in air, be equal to the exceſs of the denſity of that within them, above the common quantity contain'd in cork; their repulſion will be quite deſtroy'd. But if the tube be brought nearer; the fluid wtthout, being more denſe than that within the balls, it will be attracted by them, and they will recede from each other again.

When the apparatus has loſt part of its natural ſhare of this fluid, by the approach of excited wax to one end of it, or is electrified *negatively*; the electrical fire is attracted and imbib'd by the balls to ſupply the deficiency; and that more plentifully at the approach of excited glaſs; or a body poſitively electrified, than before; whence the diſtance between the balls will be increaſed, as the fluid ſurrounding them is augmented. And in general, whether by the approach or receſs of any body; if the difference between the denſity of the internal and external fluid be be increaſed, or diminiſhed; the repulſion of the balls will be increaſed, or diminiſhed, accordingly.

EXPERIMENT V.

When the inſulated tin tube is not electrified, bring the excited glaſs tube towards the middle of it, ſo as to be nearly at right angles with it, and the balls at the end will repel each other; and the more ſo, as the excited tube is brought nearer. When it has been held a few ſeconds, at the diſtance of about ſix inches, withdraw it, and the balls will approach each other till they touch; and then ſeparating again, as the tube is moved farther off, will continue to repel when it is taken quite away. And this repulſion between the balls will be increaſed by the approach of excited glaſs, but diminiſhed by excited wax; juſt as if the apparatus had been electrified by wax, after the manner deſcribed in the third experiment.

EXPERIMENT VI.

Inſulate two tin tubes, diſtinguiſhed by *A* and *B*, ſo as to be in a line with each other, and about half an inch apart; and at the remote end of each, let a pair of cork balls be ſuſpended. Towards the middle of *A*, bring the excited glaſs tube, and holding it a ſhort time, at the diſtance of a few inches, each pair of balls will be obſerved to ſeparate: withdraw the tube, and the balls of *A* will come together, and then repel each other again; but thoſe of *B* will hardly be affected. By the approach of the excited

cited glaſs tube, held under the balls of *A*, their repulſion will be increaſed: but if the tube be brought, in the ſame manner, towards the balls of *B*, their repulſion will be diminiſhed.

In the fifth experiment, the common ſtock of electrical matter in the tin tube, is ſuppoſed to be attenuated about the middle, and to be condenſed at the ends, by the repelling power of the atmoſphere of the excited glaſs tube, when held near it. And perhaps the tin tube may loſe ſome of its natural quantity of the electrical fluid, before it receives any from the glaſs; as that fluid will more readily run off from the ends and edges of it, than enter at the middle: and accordingly, when the glaſs tube is withdrawn, and the fluid is again equally diffuſed through the apparatus, it is found to be electrified negatively: For excited glaſs brought under the balls will increaſe their repulſion.

In the ſixth experiment, part of the fluid driven out of one tin tube enters the other; which is found to be electrified poſitively, by the decreaſing of the repulſion of its balls, at the approach of excited glaſs.

EXPERIMENT VII.

Let the tin tube, with a pair of balls at one end, be placed three feet at leaſt from any part of the room, and the air render'd very dry by means of a fire; electrify the apparatus to a conſiderable degree; then touch the tin tube with a finger, or any other conductor, and the balls will

will, notwithstanding, continue to repel each other; tho' not at so great a great a distance as before.

The air surrounding the apparatus to the distance of two or three feet, is supposed to contain more or less of the electrical fire, than its common share, as the tin tube is electrified positively, or negatively; and when very dry, may not part with its overplus, or have its deficiency supplied so suddenly, as the tin; but may continue to be electrified, after that has been touch'd for a considerable time.

EXPERIMENT VIII.

Having made the Torricellian vacuum about five feet long, after the manner described in the *Philosophical Transactions*, Vol. xlvii. p. 370. if the excited tube be brought within a small distance of it, a light will be seen through more than half its length; which soon vanishes, if the tube be not brought nearer; but will appear again, as that is moved farther off. This may be repeated several times, without exciting the tube afresh.

This experiment may be consider'd as a kind of ocular demonstration of the truth of Mr *Franklin*'s hypothesis; that when the electrical fluid is condensed on one side of thin glass, it will be repelled from the other, if it meets with no resistance. According to which, at the approach of the excited tube, the fire is supposed to be repelled from the inside of the glass surrounding the vacuum, and to be

carried

carried off through the columns of mercury; but, as the tube is withdrawn, the fire is ſuppoſed to return.

EXPERIMENT IX.

Let an excited ſtick of wax, of two feet and an half in length, and about an inch in diameter, be held near its middle. Excite the glaſs tube, and draw it over one half of it; then, turning it a little about its axis, let the tube be excited again, and drawn over the ſame half; and let this operation be repeated ſeveral times: then will that half deſtroy the repelling power of balls electrified by glaſs, and the other half will increaſe it.

By this experiment it appears, that wax alſo may be electrified poſitively and negatively. And it is probable, that all bodies whatſoever may have the quantity they contain of the electrical fluid, increaſed, or diminiſhed. The clouds, I have obſerved, by a great number of experiments, to be ſome in a poſitive, and others in a negative ſtate of electricity. For the cork balls, electrified by them, will ſometimes cloſe at the approach of excited glaſs; and at other times be ſeparated to a greater diſtance. And this change I have known to happen five or ſix times in leſs than half an hour; the balls coming together each time and remaining in contact a few ſeconds, before they repel each other again. It may likewiſe eaſily be diſcover'd, by a charged phial, whether the electrical fire be drawn out of the apparatus by a negative cloud, or forced into it by a poſia positive one: and by which soever it be electrified, should that cloud either part with its overplus, or have its deficiency supplied suddenly, the apparatus will lose its electricity: which is frequently observed to be the case, immediately after a flash of lightning. Yet when the air is very dry, the apparatus will continue to be electrised for ten minutes, or a quarter of an hour, after the clouds have passed the zenith; and sometimes till they appear more than half-way towards the horizon. Rain, especially when the drops are large, generally brings down the electrical fire: and hail, in summer, I believe never fails. When the apparatus was last electrified, it was by the fall of thawing snow, which happened so lately, as on the 12th of *November*; that being the twenty-sixth day, and sixty-first time, it has been electrified, since it was first set up; which was about the middle of *May*. And as *Fahrenheit*'s thermometer was but seven degrees above freezing, it is supposed the winter will not intirely put a stop to observations of this sort. At *London*, no more than two thunder-storms have happened during the whole summer; and the apparatus was sometimes so strongly electrified in one of them, that the bells, which have been frequently rung by the clouds, so loud as to be heard in every room of the house (the doors being open) were silenced by the almost constant stream of dense electrical fire, between each bell and the brass ball, which would not suffer it to strike.

I shall conclude this paper, already too long, with the following queries:

1. May not air, suddenly rarefied, give electrical fire to, and air suddenly condensed, receive electrical fire from, clouds and vapours passing through it?

2. Is not the *aurora borealis*, the flashing of electrical fire from positive, towards negative clouds at a great distance, through the upper part of the atmosphere, where the resistance is least?

APPENDIX.

AS Mr *Franklin*, in a former letter to Mr *Collinſon*, mentioned his intending to try the power of a very ſtrong electrical ſhock upon a turkey, that gentleman accordingly has been ſo very obliging as to ſend an account of it, which is to the following purpoſe.

He made firſt ſeveral experiments on fowls, and found, that two large thin glaſs jars gilt, holding each about ſix gallons, were ſufficient, when fully charged, to kill common hens outright; but the turkeys, though thrown into violent convulſions, and then lying as dead for ſome minutes, would recover in leſs than a quarter of an hour. However, having added three other ſuch to the former two, though not fully charged, he killed a turkey of about ten pounds weight, and believes that they would have killed a much larger. He conceited, as himſelf ſays, that the birds kill'd in this manner eat uncommonly tender.

In making theſe experiments, he found, that a man could, without great detriment, bear a much greater ſhock than he had imagined: for he inadvertently received the ſtroke of two of theſe jars through his arms and body, when they were very near fully charged. It ſeemed to him an univerſal blow throughout the body from head to foot,

and

and was followed by a violent quick trembling in the trunk, which went off gradually in a few ſeconds. It was ſome minutes before he could recollect his thoughts, ſo as to know what was the matter; for he did not ſee the flaſh, tho' his eye was on the ſpot of the prime-conductor, from whence it ſtruck the back of his hand; nor did he hear the crack, though the by-ſtanders ſaid it was a loud one; nor did he particularly feel the ſtroke on his hand, tho' he afterwards found it had raiſed a ſwelling there, of the bigneſs of half a piſtol-bullet. His arms and the back of the neck felt ſomewhat numbed the remainder of the evening, and his breaſt was ſore for a week after, as if it had been bruiſed. From this experiment may be ſeen the danger, even under the greateſt caution, to the operator, when making theſe experiments with large jars; for it is not to be doubted, but ſeveral of theſe fully charged would as certainly, by increaſing them, in proportion to the ſize, kill a man, as they before did a turkey.

N.B. The original of this letter, which was read at the Royal Society, has been miſlaid.

ELECTRICAL *and other* PHILOSOPHICAL PAPERS *and* LETTERS.

ELECTRICAL EXPERIMENTS *made in Purſuance of thoſe made by Mr* Canton, *dated* December 6, 1753; *with Explanations, by Mr* Benjamin Franklin.

Philadelphia, March 14, 1755.

PRINCIPLES.

Read at the Royal Society, *Dec.* 18, 1755.

I. ELECTRIC atmoſpheres, that flow round non-electric bodies, being brought near each other, do not readily mix and unite into one atmoſphere, but remain ſeparate, and repel each other.

This is plainly ſeen in ſuſpended cork balls, and other bodies electrified.

II. An electric atmoſphere not only repels another electric atmoſphere, but will alſo repel the electric matter contained in the ſubſtance of a body approaching it; and without joining or mixing with it, force it to other parts of the body that contained it.

 This

This is ſhewn by ſome of the following experiments.

III. Bodies electrified negatively, or deprived of their natural quantity of Electricity, repel each other, (or at leaſt appear to do ſo, by a mutual receding) as well as thoſe electrified poſitively, or which have electric atmoſpheres.

This is ſhewn by applying the negatively charged wire of a phial to two cork balls, ſuſpended by ſilk threads, and by many other experiments.

PREPARATION.

Fix a taſſel of fifteen or twenty threads, three inches long at one end, of a tin prime conductor, (mine is about five feet long, and four inches diameter) ſupported by ſilk lines.

Let the threads be a little damp, but not wet.

EXPERIMENT I.

Paſs an excited glaſs Tube near the other end of the prime conductor, ſo as to give it ſome ſparks, and the threads will diverge.

Becauſe each thread, as well as the prime-conductor, has acquired an electric atmoſphere, which repels and is repelled by the atmoſpheres of the other threads: if thoſe ſeveral atmoſpheres would readily mix, the threads might unite, and hang in the middle of one atmoſphere, common to them all.

Rub

Rub the tube afreſh, and approach the prime-conductor therewith, croſsways, near that end, but not nigh enough to give ſparks; and the threads will diverge a little more.

Becauſe the atmoſphere of the prime-conductor is preſſed by the atmoſphere of the excited tube, and driven towards the end where the threads are, by which each thread acquires more atmoſphere.

Withdraw the tube, and they will cloſe as much.

They cloſe as much, and no more; becauſe the atmoſphere of the glaſs tube not having mixed with the atmoſphere of the prime conductor, is withdrawn intire, having made no addition to, or diminution from it.

Bring the excited tube under the tuft of threads, and they will cloſe a little.

They cloſe, becauſe the atmoſphere of the glaſs tube repels their atmoſpheres, and drives part of them back on the prime conductor.

Withdraw it, and they will diverge as much.

For the portion of atmoſphere which they had loſt, returns to them again.

EXPERIMENT II.

Excite the glaſs tube, and approach the prime conductor with it, holding it acroſs, near the end oppoſite to that on which the threads hang, at the diſtance of five or ſix inches. Keep it there a few ſeconds, and the threads of the taſſels will diverge. Withdraw it, and they will cloſe.

 They

They diverge, becauſe they have received electric atmoſpheres from the electric matter before contained in the ſubſtance of the prime conductor; but which is now repelled and driven away, by the atmoſphere of the glaſs tube, from the parts of the prime conductor oppoſite and neareſt to that atmoſphere, and forced out upon the ſurface of the prime conductor at its other end, and upon the threads hanging thereto. Were it any part of the atmoſphere of the glaſs tube that flowed over and along the prime conductor to the threads, and gave them atmoſpheres, (as is the caſe when a ſpark is given to the prime conductor from the glaſs tube) ſuch part of the tube's atmoſphere would have remained, and the threads continue to diverge; but they cloſe on withdrawing the tube, becauſe the tube takes with it *all its own atmoſphere*, and the electric matter, which had been driven out of the ſubſtance of the prime conductor, and formed atmoſpheres round the threads, is thereby permitted to return to its place.

Take a ſpark from the prime conductor near the threads, when they are diverged as before, and they will cloſe.

For by ſo doing they take away their atmoſpheres, compoſed of the electric matter driven out of the ſubſtance of the prime conductor, as aforeſaid, by the repellency of the atmoſphere of the glaſs tube. By taking this ſpark you rob the prime conductor of part of its natural quantity of the electric matter; which part ſo taken is not ſupplied by the glaſs tube, for when that is afterwards with-

drawn

drawn, it takes with it its whole atmoſphere, and leaves the prime conductor electriſed negatively, as appears by the next operation.

Then withdraw the tube, and they will open again.

For now the electric matter in the prime conductor, returning to its equilibrium, or equal diffuſion, in all parts of its ſubſtance, and the prime conductor having loſt ſome of its natural quantity, the threads connected with it loſe part of theirs, and ſo are electriſed negatively, and therefore repel each other, by *Pr.* III.

Approach the prime conductor with the tube near the ſame place as at firſt, and they will cloſe again.

Becauſe the part of their natural quantity of electric fluid, which they had loſt, is now reſtored to them again, by the repulſion of the glaſs tube forcing that fluid to them from other parts of the prime conductor; ſo they are now again in their natural ſtate.

Withdraw it, and they will open again.

For what had been reſtored to them, is now taken from them again, flowing back into the prime conductor, and leaving them once more electriſed negatively.

Bring the excited tube under the threads, and they will diverge more.

Becauſe more of their natural quantity is driven from them into the prime conductor, and thereby their negative Electricity increaſed.

EX-

EXPERIMENT III.

The prime conductor not being electrified, bring the excited tube under the tassel, and the threads will diverge.

Part of their natural quantity is thereby driven out of them into the prime conductor, and they become negatively electrised, and therefore repel each other.

Keeping the tube in the same place with one hand, attempt to touch the threads with the finger of the other hand, and they will recede from the finger.

Because the finger being plunged into the atmosphere of the glass tube, as well as the threads, part of its natural quantity is driven back through the hand and body, by that atmosphere, and the finger becomes, as well as the threads, negatively electrised, and so repels, and is repelled by them. To confirm this, hold a slender light lock of cotton, two or three inches long, near a prime conductor, that is electrified by a glass globe, or tube. You will see the cotton stretch itself out towards the prime conductor. Attempt to touch it with the finger of the other hand, and it will be repelled by the finger. Approach it with a positively charged wire of a bottle, and it will fly to the wire. Bring it near a negatively charged wire of a bottle, it will recede from that wire in the same manner that it did from the finger; which demonstrates the finger to be negatively electrised, as well as the lock of cotton so situated.

Extract

Extract of a Letter concerning Electricity, from Mr B. Franklin, *to Monf.* Dalibard, *at* Paris, *inclosed in a Letter to Mr* Peter Collinfon, F.R.S.

Philadelphia, June 29, 1755.

Read at the Royal Society, *Dec.* 18, 1755.

YOU defire my opinion of *Pere Beccaria*'s *Italian* book *. I have read it with much pleafure, and think it one of the beft pieces on the fubject that I have feen in any language. Yet as to the article of water-fpouts, I am not at prefent of his fentiments; though I muft own with you, that he has handled it very ingenioufly. Mr *Collinfon* has my opinion of whirlwinds and water-fpouts at large, written fome time fince. I know not whether they will be publifhed; if not, I will get them tranfcribed for your perufal. It does not appear to me that *Pere Beccaria* doubts of the *abfolute impermeability of glafs* in the fenfe I meant it; for the inftances he gives of holes made through glafs by the electric ftroke, are fuch as we have all experienced, and only fhew that the electric fluid could not pafs without making a hole. In the fame manner we fay, glafs is impermeable to water, and yet a ftream from a fire-engine will force through the ftrongeft panes of a window. As to the effect of points in

* This work is written conformable to Mr *Franklin*'s theory, upon artificial and natural Electricity, which compofe the two parts of it. It was printed in *Italian* at *Turin*, in 4to. 1753; between the two parts is a letter to the Abbe *Nollet*, in defence of Mr *Franklin*'s fyftem. J. B.

drawing

drawing the electric matter from clouds, and thereby securing buildings, *&c.* which, you say, he seems to doubt, I must own I think he only speaks modestly and judiciously. I find I have been but partly understood in that matter. I have mentioned it in several of my letters, and except once, always in the *alternative*, *viz.* that pointed rods erected on buildings, and communicating with the moist earth, would either *prevent* a stroke, *or*, if not prevented, would *conduct* it, so as that the building should suffer no damage. Yet whenever my opinion is examined in *Europe*, nothing is considered but the probability of those rods *preventing* a stroke or explosion, which is only a *part* of the use I proposed for them; and the other part, their conducting a stroke, which they may happen not to prevent, seems to be totally forgotten, though of equal importance and advantage.

I thank you for communicating M. *de Buffon*'s relation of the effect of lightning at *Dijon*, on the 7th of *June* last. In return, give me leave to relate an instance I lately saw of the same kind. Being in the town of *Newbury* in *New-England*, in *November* last, I was shewn the effect of lightning on their church, which had been struck a few months before. The steeple was a square tower of wood, reaching seventy feet up from the ground to the place where the bell hung, over which rose a taper spire, of wood likewise, reaching seventy feet higher, to the vane of the weather-cock. Near the bell was fixed an iron hammer to strike the hours; and from the tail of the hammer

mer a wire went down through a ſmall gimlet-hole in the floor that the bell ſtood upon, and through a ſecond floor in like manner; then horizontally under and near the plaiſtered cieling of that ſecond floor, till it came near a plaiſtered wall; then down by the ſide of that wall to a clock, which ſtood about twenty feet below the bell. The wire was not bigger than a common knitting needle. The ſpire was ſplit all to pieces by the lightning, and the parts flung in all directions over the ſquare in which the church ſtood, ſo that nothing remained above the bell.

The lightning paſſed between the hammer and the clock in the above-mentioned wire, without hurting either of the floors, or having any effect upon them, (except making the gimlet-holes, through which the wire paſſed, a little bigger,) and without hurting the plaiſtered wall, or any part of the building, ſo far as the aforeſaid wire and the pendulum wire of the clock extended; which latter wire was about the thickneſs of a gooſe-quill. From the end of the pendulum, down quite to the ground, the building was exceedingly rent and damaged, and ſome ſtones in the foundation-wall torn out, and thrown to the diſtance of twenty or thirty feet. No part of the aforementioned long ſmall wire, between the clock and the hammer, could be found, except about two inches that hung to the tail of the hammer, and about as much that was faſtened to the clock; the reſt being exploded, and its particles diſſipated in ſmoke and air, as gunpowder is by common fire, and had only left a black ſmutty track on the plaiſtering, three or four inches broad, darkeſt in the middle, and fainter toward the edges, all along the cieling, under which it paſſed, and down the wall. Theſe were the effects and appearances; on which I would only make the few following remarks, *viz.*

1. That lightning, in its paſſage through a building, will leave wood to paſs as far as it can in metal, and not enter the wood again till the conductor of metal ceaſes.

And the ſame I have obſerved in other inſtances, as to walls of brick or ſtone.

2. The quantity of lightning that paſſed through this ſteeple muſt have been very great, by its effects on the lofty ſpire above the bell, and on the ſquare tower all below the end of the clock pendulum.

3. Great as this quantity was, it was conducted by a ſmall wire and a clock pendulum, without the leaſt damage to the building ſo far as they extended.

4. The pendulum rod being of a ſufficient thickneſs, conducted the lightning without damage to itſelf; but the ſmall wire was utterly deſtroyed.

5. Though the ſmall wire was itſelf deſtroyed, yet it had conducted the lightning with ſafety to the building.

6. And from the whole it ſeems probable, that if even ſuch a ſmall wire had been extended from the ſpindle of the vane to the earth, before the ſtorm, no damage would have been done to the ſteeple by that ſtroke of lightning, though the wire itſelf had been deſtroyed.

LET-

LETTER XIII.

TO

PETER COLLINSON, *Eſq*; F. R. S. at *London*.

Dear Friend, *Philadelphia, Nov.* 23, 1753.

IN my laſt, *via Virginia*, I promiſed to ſend you per next ſhip, a ſmall philoſophical pacquet: But now having got the materials (old letters and rough drafts) before me, I fear you will find it a great one. Neverthe-leſs, as I am like to have a few days leiſure before this ſhip ſails, which I may not have again in a long time, I ſhall tranſcribe the whole, and ſend it; for you will be under no neceſſity of reading it all at once, but may take it a little at a time, now and then of a winter evening. When you happen to have nothing elſe to do (if that ever happens,) it may afford you ſome amuſement *.

B. F.

* Theſe Letters and Papers are a Philoſophical Correſpondence between Mr *Franklin* and ſome of his *American* Friends. Mr *Collinſon* communicated them to the Royal Society, where they were read at different meetings during the year 1756. But Mr *Franklin* having particularly requeſted that they might not be printed, none of them were inſerted in the Tranſactions. Mr *F.* had at that time an intention of reviſing them, and purſuing ſome of the enquiries farther; but finding that he is not like to have ſufficient leiſure, he has at length been induced, imperfect as they are, to permit their publication, as ſome of the hints they contain may poſſibly be uſeful to others in their philoſophical reſearches.

Extract of a Letter from a Gentleman in BOSTON, *to* BENJAMIN FRANKLIN, *Esq; concerning the* crooked Direction, *and the* Source *of* Lightning.

SIR, *Boston, Dec.* 21, 1751.

THE experiments Mr *K.* has exhibited here, have been greatly pleasing to all sorts of people that have seen them; and I hope, by the time he returns to *Philadelphia*, his tour this way will turn to good account. His experiments are very curious, and I think prove most effectually your doctrine of Electricity; that it is a real element, annexed to, and diffused among all bodies we are acquainted with; that it differs in nothing from lightning, the effects of both being similar, and their properties, so far as they are known, the same, &c.

The remarkable effect of lightning on iron, lately discovered, in giving it the magnetic virtue, and the same effect produced on small needles by the electrical fire, is a further and convincing proof that they are both the same element; but, which is very unaccountable, Mr *K.* tells me, it is necessary to produce this effect, that the direction of the needle and the electric fire should be North and South; from either to the other, and that just so far as they deviate therefrom, the magnetic power in the needle is less, till their direction being at right angles with the North and South, the effect entirely ceases. We made at *Faneuil* Hall,

Hall, where Mr *K*——'*s* apparatus is, several experiments to give some small needles the magnetic virtue; previously examining, by putting them in water, on which they will be supported, whether or not they had any of that virtue; and I think we found all of them to have some small degree of it, their points turning to the North: We had nothing to do then but to invert the poles, which accordingly was done, by sending through them the charge of two large glass jars; the eye of the needle turning to the North, as the point before had done; that end of the needle which the fire is thrown upon, Mr *K.* tells me always points to the North.

The electrical fire passing through air has the same crooked direction as lightning. * This appearance I endeavour to account for thus. Air is an electric *per se*, therefore there must be a mutual repulsion betwixt air and the electrical fire. A column or cylinder of air having the diameter of its base equal to the diameter of the electrical spark, intervenes that part of the body which the spark is taken from, and of the body it aims at. The spark acts upon this column, and is acted upon by it, more strongly than any other neighbouring portion of air.

The column being thus acted upon, becomes more dense, and being more dense, repels the spark more strongly; its repellency being in proportion to its density: Having acquired, by being condensed, a degree of repellency greater than its natural, it turns the spark out of its strait course; the neighbouring air which must be less dense, and therefore

* This is most easily observed in large strong sparks taken at some inches distance.

has

has a ſmaller degree of repellency, giving it a more ready paſſage.

The ſpark having taken a new direction, muſt now act on, or moſt ſtrongly repel the column of air which lies in that direction, and conſequently muſt condenſe that column in the ſame manner as the former, when the ſpark muſt again change its courſe, which courſe will be thus repeatedly changed, till the ſpark reaches the body that attracted it.

To this account one objection occurs; that as air is very fluid and elaſtic, and ſo endeavours to diffuſe itſelf equally, the ſuppoſed acccumulated air within the column aforeſaid, would be immediately diffuſed among the contiguous air, and circulate to fill the ſpace it was driven from; and conſequently that the ſaid column, on the greater denſity of which the phenomenon is ſuppoſed to depend, would not repel the ſpark more ſtrongly than the neighbouring air.

This might be an objection, if the electrical fire was as ſluggiſh and inactive as air. Air takes a ſenſible time to diffuſe itſelf equally, as is manifeſt from winds which often blow for a conſiderable time together from the ſame point, and with a velocity even in the greateſt ſtorms, not exceeding, as it is ſaid, ſixty miles an hour: But the electric fire ſeems propagated inſtantaneouſly, taking up no perceptible time in going very great diſtances. It muſt then be an inconceivably ſhort time in its progreſs from an electrified to an unelectrified body, which, in the preſent caſe, can be but a few inches apart: But this ſmall portion of time

time is not ſufficient for the elaſticity of the air to exert itſelf, and therefore the column aforeſaid muſt be in a denſer ſtate than its neighbouring air.

About the velocity of the electric fire more is ſaid below, which perhaps may more fully obviate this objection. But let us have recourſe to experiments. Experiments will obviate all objections, or confound the hypotheſis. The electric ſpark, if the foregoing be true, will paſs through a vacuum in a right line. To try this, let a wire be fixed perpendicularly on the plate of an air pump, having a leaden ball on its upper end; let another wire paſſing through the top of a receiver, have on each end a leaden ball; let the leaden balls within the receiver, when put on the air pump, be within two or three inches of each other: The receiver being exhauſted, the ſpark given from a charged vial to the upper wire, will paſs through rarified air, nearly approaching to a vacuum, to the lower wire, and I ſuppoſe in a right line, or nearly ſo; the ſmall portion of air remaining in the receiver, which cannot be entirely exhauſted, may poſſibly cauſe it to deviate a little, but perhaps not ſenſibly, from a right line. The ſpark alſo might be made to paſs through air greatly condenſed, which perhaps would give a ſtill more crooked direction. I have not had opportunity to make any experiments of this ſort, not knowing of an air-pump nearer than *Cambridge*, but you can eaſily make them. If theſe experiments anſwer, I think the crooked direction of lightning will be alſo accounted for.

With

With respect to your Letters on Electricity, * * * * * * * * * * * * * * * *. Your Hypothesis in particular for explaining the phænomena of lightning is very ingenious. That some clouds are highly charged with electrical fire, and that their communicating it to those that have less, to mountains and other eminencies, makes it visible and audible, when it is denominated lightning and thunder, is highly probable: But that the sea, which you suppose the grand source of it, can collect it, I think admits of a doubt: For though the sea be composed of salt and water, an electric *per se* and non-electric, and though the friction of electrics *per se* and non-electrics, will collect that fire, yet it is only under certain circumstances, which water will not admit. For it seems necessary, that the electrics *per se* and non-electrics rubbing one another, should be of such substances as will not adhere to, or incorporate with each other. Thus a glass or sulphur sphere turned in water, and so a friction between them, will not collect any fire; nor, I suppose, would a sphere of salt revolving in water; the water adhering to, or incorporating with those electrics *per se*. But granting that the friction between salt and water would collect the electrical fire, that fire, being so extreamly subtil and active, would be immediately communicated, either to those lower parts of the sea from which it was drawn, and so only perform quick revolutions; or be communicated to the adjacent islands or continent, and so be diffused instantaneously through the general mass of the earth. I say instantaneously,

neouſly, for the greateſt diſtances we can conceive within the limits of our globe, even that of the two moſt oppoſite points, it will take no ſenſible time in paſſing through: And therefore it ſeems a little difficult to conceive how there can be any accumulation of the electrical fire upon the ſurface of the ſea, or how the vapours ariſing from the ſea, ſhould have a greater ſhare of that fire than other vapours.

That the progreſs of the electrical fire is ſo amazingly ſwift, ſeems evident from an experiment you yourſelf (not out of choice) made, when two or three large glaſs jars were diſcharged through your body. You neither heard the crack, was ſenſible of the ſtroke, nor, which is more extraordinary, ſaw the light; which gave you juſt reaſon to conclude, that it was ſwifter than ſound, than animal ſenſation, and even light itſelf. Now light, (as aſtronomers have demonſtrated) is about ſix minutes paſſing from the ſun to the earth; a diſtance, they ſay, of more than eighty millions of miles. The greateſt rectilinear diſtance within the compaſs of the earth, is about eight thouſand miles, equal to its diameter. Suppoſing then, that the velocity of the electric fire be the ſame as that of light, it will go through a ſpace equal to the earth's diameter in about $\frac{2}{60}$ of one ſecond of a minute. It ſeems inconceivable then, that it ſhould be accumulated upon the ſea, in its preſent ſtate, which, as it is a non-electric, muſt give the fire an inſtantaneous paſſage to the neighbouring ſhores, and they convey it to the general maſs of the earth. But ſuch accumulation ſeems ſtill more inconceivable when the electrical fire has but a few feet depth of water to penetrate, to return to the place, from whence it is ſuppoſed to be collected.

Your thoughts upon theſe remarks I ſhall receive with a great deal of pleaſure. I take notice that in the printed copies of your letters ſeveral things are wanting which are in the manuſcript you ſent me. I underſtand by your ſon, that you had writ, or was writing, a paper on the effect of the electrical fire on loadſtones, needles, &c. which I would aſk the favour of a copy of, as well as of any other papers on Electricity, written ſince I had the manuſcript, for which I repeat my obligations to you.

I am, &c.

J. B.

LETTER XIV.

FROM

BENJ. FRANKLIN, *Esq*; of *Philadelphia*.

Philadelphia, Jan. 24, 1752.

SIR,

Read at the Royal Society *May* 27, 1756.

I AM glad to learn, by your favour of the 21st past, that Mr *Kinnersley*'s lectures have been acceptable to the Gentlemen of *Boston*, and are like to prove serviceable to himself.

I thank you for the countenance and encouragement you have so kindly afforded my fellow-citizen.

I send you enclosed an extract of a letter containing the substance of what I observed concerning the communication of magnetism to needles by Electricity. The minutes I took at the time of the experiments, are mislaid. I am very little acquainted with the nature of magnetism. Dr *Gawin Knight*, inventor of the steel magnets, has wrote largely on that subject, but I have not yet had leisure to peruse his writings with the attention necessary to become master of his doctrine.

 Your

Your explication of the crooked direction of lightning, appears to me both ingenious and ſolid. When we can account as ſatisfactorily for the electrification of clouds, I think that branch of Natural Philoſophy will be nearly compleat.

The air, undoubtedly, obſtructs the motion of the electric fluid. Dry air prevents the diſſipation of an electric atmoſphere, the denſer the more, as in cold weather. I queſtion whether ſuch an atmoſphere can be retained by a body *in vacuo*. A common electrical vial requires a non-electric communication from the wire to every part of the charged glaſs; otherwiſe, being dry and clean, and filled with air only, it charges ſlowly, and diſcharges gradually, by ſparks, without a ſhock: But, exhauſted of air, the communication is ſo open and free between the inſerted wire and ſurface of the glaſs, that it charges as readily, and ſhocks as ſmartly as if filled with water: And I doubt not, but that in the experiment you propoſe, the ſparks would not only be near ſtrait *in vacuo*, but ſtrike at a greater diſtance than in the open air, though perhaps there would not be a loud exploſion. As ſoon as I have a little leiſure, I will make the experiment, and ſend you the reſult.

My ſuppoſition that the ſea might poſſibly be the grand ſource of lightning, aroſe from the common obſervation of its luminous appearance in the night, on the leaſt motion; an appearance never obſerved in freſh water. Then I knew that the electric fluid may be pumped up out of the earth, by the friction of a glaſs globe, on a non-electric cu-

ſhion;

shion; and that, notwithstanding the surprizing activity and swiftness of that fluid, and the non-electric communication between all parts of the cushion and the earth, yet quantities would be snatch'd up by the revolving surface of the globe, thrown on the prime conductor, and dissipated in air. How this was done, and why that subtile active spirit did not immediately return again from the globe, into some part or other of the cushion, and so into the earth, was difficult to conceive; but whether from its being opposed by a current setting upwards to the cushion, or from whatever other cause, that it did not so return was an evident fact. Then I considered the separate particles of water as so many hard spherules, capable of touching the salt only in points, and imagined a particle of salt could therefore no more be wet by a particle of water, than a globe by a cushion; that there might therefore be such a friction between these originally constituent particles of salt and water, as in a sea of globes and cushions; that each particle of water on the surface might obtain from the common mass, some particles of the universally diffused, much finer, and more subtil electric fluid, and forming to itself an atmosphere of those particles, be repelled from the then generally electrified surface of the sea, and fly away with them into the air. I thought too, that possibly the great mixture of particles electric *per se*, in the ocean water, might, in some degree, impede the swift motion and dissipation of the electric fluid through it to the shores, &c.—But having since found, that salt in the water of an electric vial, does not lessen the shock; and

and having endeavoured in vain to produce that luminous appearance from a mixture of ſalt and water agitated; and obſerved, that even the ſea-water will not produce it after ſome hours ſtanding in a bottle; I ſuſpect it to proceed from ſome principle yet unknown to us (which I would gladly make ſome experiments to diſcover, if I lived near the ſea) and I grow more doubtful of my former ſuppoſition, and more ready to allow weight to that objection (drawn from the activity of the electric fluid, and the readineſs of water to conduct) which you have indeed ſtated with great ſtrength and clearneſs.

In the mean time, before we part with this hypotheſis, let us think what to ſubſtitute in its place. I have ſometimes queried whether the friction of the air, an electric *per ſe*, in violent winds, among trees, and againſt the ſurface of the earth, might not pump up, as ſo many glaſs globes, quantities of the electric fluid, which the riſing vapours might receive from the air, and retain in the clouds they form? on which I ſhould be glad to have your ſentiments. An ingenious friend of mine ſuppoſes the land-clouds more likely to be electrified than the ſea-clouds. I ſend his letter for your peruſal, which pleaſe to return me.

I have wrote nothing lately on Electricity, nor obſerved any thing new that is material, my time being much taken up with other affairs. Yeſterday I diſcharged four jars through a fine wire, tied up between two ſtrips of glaſs: The wire was in part melted, and the reſt broke into ſmall pieces, from half an inch long, to half a quarter of an inch.

My

My globe raiſes the electric fire with greater eaſe, in much greater quantities, by the means of a wire extended from the cuſhion, to the iron pin of a pump handle behind my houſe, which communicates by the pump ſpear with the water in the well.

By this poſt I ſend to * * * *, who is curious in that way, ſome meteorological obſervations and conjectures, and deſire him to communicate them to you, as they may afford you ſome amuſement, and I know you will look over them with a candid eye. By throwing our occaſional thoughts on paper, we more readily diſcover the defects of our opinions, or we digeſt them better, and find new arguments to ſupport them. This I ſometimes practice; but ſuch pieces are fit only to be ſeen by friends.

I am, &c.

B. F.

LET-

LETTER XV.

From J. B. Esq; of *Boston*,

TO

BENJAMIN FRANKLIN, Esq; at *Philadelphia*.

SIR, *Boston*, *March* 2, 1752.

Read at the Royal Society *June* 3, 1756.

I Have received your favour of the 24th of *January* past, inclosing an extract from your letter to Mr *Collinson*, and * * * 's letter to yourself, which I have read with a great deal of pleasure, and am much obliged to you for. Your extract confirms a correction Mr *Kinnersley* made a few days ago, of a mistake I was under respecting the polarity given to needles by the electrical fire, "that the end which "receives the fire always points North;" and, "that the "needle being situated East and West, will not have a po-"lar direction." You find, however, the polarity strongest when the needle is shocked lying North and South; weakest when lying East and West; which makes it probable that the communicated magnetism is less, as the nee-dle

dle varies from a North and South ſituation. As to the needle of Capt. *Waddel*'s compaſs, if its polarity was reverſed by the lightning, the effect of lightning and Electricity, in regard of that, ſeems diſſimilar; for a magnetic needle in a North and South ſituation (as the compaſs needle was) inſtead of having its power reverſed, or even diminiſhed, would have it confirmed or increaſed by the electric fire. But perhaps the lightning communicated to ſome nails in the binnacle (where the compaſs is placed) the magnetic virtue, which might diſturb the compaſs.

This I have heard was the caſe; if ſo, the ſeeming diſſimilarity vaniſhes: But this remarkable circumſtance (if it took place) I ſhould think would not be omitted in Capt. *Waddel*'s account.

I am very much pleaſed that the explication I ſent you, of the crooked direction of lightning, meets with your approbation.

As to your ſuppoſition about the ſource of lightning, the luminous appearance of the ſea in the night, and the ſimilitude between the friction of the particles of ſalt and water, as you conſidered them in their original ſeparate ſtate, and the friction of the globe and cuſhion, very naturally led you to the ocean, as the grand ſource of lightning: But the activity of lightning, or the electric element, and the fitneſs of water to conduct it, together with the experiments you mention of ſalt and water, ſeem to make againſt it, and to prepare the way for ſome other hypotheſis. Accordingly you propoſe a new one, which is very curious, and not ſo liable, I think, to objections as the former. But there is not as yet, I believe, a sufficient variety of experiments to establish any theory, though this seems the most hopeful of any I have heard of.

The effect which the discharge of your four glass jars had upon a fine wire, tied between two strips of glass, puts me in mind of a very similar one of lightning, that I observed at *New-York*, *October* 1750, a few days after I left *Philadelphia*. In company with a number of Gentlemen, I went to take a view of the city from the *Dutch* church steeple, in which is a clock about twenty or twenty-five feet below the bell. From the clock went a wire through two floors, to the clock-hammer near the bell, the holes in the floor for the wire being perhaps about a quarter of an inch diameter. We were told, that in the spring of 1750, the lightning struck the clock-hammer, and descended along the wire to the clock, melting in its way several spots of the wire, from three to nine inches long, through one-third of its substance, till coming within a few feet of the lower end, it melted the wire quite through, in several places, so that it fell down in several pieces; which spots and pieces we saw. When it got to the end of the wire, it flew off to the hinge of a door, shattered the door, and dissipated. In its passage through the holes of the floors it did not do the least damage, which evidences that wire is a good conductor of lightning (as it is of Electricity) provided it be substantial enough, and might, in this case, had

it

it been continued to the earth, have conducted it without damaging the building *.

Your information about your globe's raising the electric fire in greater quantities, by means of a wire extended from the cushion to the earth, will enable me, I hope, to remedy a great inconvenience I have been under, to collect the fire with the electrifying glass I use, which is fixed in a very dry room, three stories from the ground. When you send your meteorological observations to ****, I hope I shall have the pleasure of seeing them.

I am, &c.

J. B.

* The wire mentioned in this account was re-placed by a small brass chain. In the summer of 1763, the lightning again struck that steeple, and from the clock-hammer near the bell, it pursued the chain as it had before done the wire, went off to the same hinge, and again shattered the same door. In its passage through the same holes of the same floors, it did no damage to the floors, nor to the building during the whole extent of the chain. But the chain itself was destroyed, being partly scattered about in fragments of two or three links melted and stuck together, and partly blown up or reduced to smoke, and dissipated.—[See an account of the same effect of lightning on a wire at *Newbury*, p. 163.] The steeple, when repair'd, was guarded by an iron conductor, or rod, extending from the foot of the vane-spindle down the outside of the building, into the earth.—The newspapers have mentioned, that in 1765, the lightning fell a third time on the same steeple, and was safely conducted by the rod; but the particulars are not come to hand.

Physical and Meteorological Observations, Conjectures, and Suppositions; by B. F.

Read at the Royal Society, *June* 3, 1756.

THE particles of air are kept at a distance from each other by their mutual repulsion.

Every three particles, mutually and equally repelling each other, must form an equilateral triangle.

All the particles of air gravitate towards the earth, which gravitation compresses them, and shortens the sides of the triangles, otherwise their mutual repellency would force them to greater distances from each other.

Whatever particles of other matter, (not endued with that repellency) are supported in air, must adhere to the particles of air, and be supported by them; for in the vacancies there is nothing they can rest on.

Air and water mutually attract each other. Hence water will dissolve in air, as salt in water.

The specific gravity of matter is not altered by dividing the matter, though the superficies be increased. Sixteen leaden bullets, of an ounce each, weigh as much in water as one of a pound, whose superficies is less.

Therefore the supporting of salt in water is not owing to its superficies being increased.

A lump of salt, tho' laid at rest at the bottom of a vessel of water, will dissolve therein, and its parts move every way, till equally diffused in the water; therefore there is a mua mutual attraction between water and ſalt. Every particle of water aſſumes as many of ſalt as can adhere to it; when more is added, it precipitates, and will not remain ſuſpended.

Water, in the ſame manner, will diſſolve in air, every particle of air aſſuming one or more particles of water. When too much is added, it precipitates in rain.

But there not being the ſame contiguity between the particles of air as of water, the ſolution of water in air is not carried on without a motion of the air, ſo as to cauſe a freſh acceſſion of dry particles.

Part of a fluid, having more of what it diſſolves, will communicate to other parts that have leſs. Thus very ſalt water coming in contact with freſh, communicates its ſaltneſs till all is equal, and the ſooner if there is a little motion of the water.

Even earth will diſſolve, or mix with air. A ſtroke of a horſe's hoof on the ground, in a hot duſty road, will raiſe a cloud of duſt, that ſhall, if there be a light breeze, expand every way, till, perhaps, near as big as a common houſe. It is not by mechanical motion communicated to the particles of duſt by the hoof, that they fly ſo far, nor by the wind that they ſpread ſo wide: But the air near the ground, more heated by the hot duſt ſtruck into it, is rarified and riſes, and in riſing mixes with the cooler air, and communicates of its duſt to it, and it is at length ſo diffuſed as to become inviſible. Quantities of duſt are thus carried up in dry ſeaſons: Showers waſh it from the air,

and

and bring it down again. For water attracting it ſtronger, it quits the air, and adheres to the water.

Air ſuffering continual changes in the degrees of its heat, from various cauſes and circumſtances, and, conſequently, changes in its ſpecific gravity, muſt therefore be in continual motion.

A ſmall quantity of fire mixed with water (or degree of heat therein) ſo weakens the coheſion of its particles, that thoſe on the ſurface eaſily quit it, and adhere to the particles of air.

A greater degree of heat is required to break the coheſion between water and air.

Air moderately heated, will ſupport a greater quantity of water inviſibly than cold air; for its particles being by heat repelled to a greater diſtance from each other, thereby more eaſily keep the particles of water that are annexed to them from running into coheſions that would obſtruct, refract, or reflect the light.

Hence when we breathe in warm air, though the ſame quantity of moiſture may be taken up from the lungs, as when we breathe in cold air, yet that moiſture is not ſo viſible.

Water being extremely heated, *i. e.* to the degree of boiling, its particles in quitting it ſo repel each other, as to take up vaſtly more ſpace than before, and by that repellency ſupport themſelves, expelling the air from the ſpace they occupy. That degree of heat being leſſened, they again mutually attract, and having no air-particles mixed to

ad-

adhere to, by which they might be supported and kept at a distance, they instantly fall, coalesce, and become water again.

The water commonly diffus'd in our atmosphere, never receives such a degree of heat from the sun, or other cause, as water has when boiling; it is not, therefore, supported by such heat, but by adhering to air.

Water being dissolv'd in, and adhering to air, that air will not readily take up oil, because of the mutual repellency between water and oil.

Hence cold oils evaporate but slowly, the air having generally a quantity of dissolved water.

Oil being heated extremely, the air that approaches its surface will be also heated extremely; the water then quitting it, it will attract and carry off oil, which can now adhere to it. Hence the quick evaporation of oil heated to a great degree.

Oil being dissolved in air, the particles to which it adheres will not take up water.

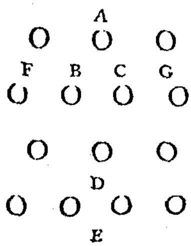
Hence the suffocating nature of air impregnated with burnt grease, as from snuffs of candles, and the like. A certain quantity of moisture should be every moment discharged and taken away from the lungs; air that has been frequently breath'd, is already overloaded, and, for that reason, can take no more, so will not answer the end. Greasy air refuses to touch it. In both cases suffocation for want of the discharge.

Air will attract and support many other substances.

A par-

A particle of air loaded with adhering water, or any any other matter, is heavier than before, and would deſcend.

The atmoſphere ſuppoſed at reſt, a loaded deſcending particle muſt act with a force on the particles it paſſes between, or meets with, ſufficient to overcome, in ſome degree, their mutual repellency, and puſh them nearer to each other.



Thus, ſuppoſing the particles A B C D, and the others near them, to be at the diſtance cauſed by their mutual repellency (confin'd by their common gravity) if A would deſcend to E, it muſt paſs between B and C; when it comes between B and C it will be nearer to them than before, and muſt either have puſh'd them nearer to F and G, contrary to their mutual repellency, or paſs through by a force exceeding its repellency with them. It then approaches D, and, to move it out of the way, muſt act on it with a force ſufficient to overcome its repellency with the two next lower particles, by which it is kept in its preſent ſituation.

Every particle of air, therefore, will bear any load inferior to the force of theſe repulſions.

Hence the ſupport of fogs, miſts, clouds.

Very warm air, clear, though ſupporting a very great quantity of moiſture, will grow turbid and cloudy on the mixture of a colder air, as foggy turbid air will grow clear by warming.

Thus

Thus the ſun ſhining on a morning fog, diſſipates it; clouds are ſeen to waſte in a ſun-ſhiny day.

But cold condenſes and renders viſible the vapour; a tankard or decanter filled with cold water, will condenſe the moiſture of warm clear air on its outſide, where it becomes viſible as dew, coaleſces into drops, deſcends in little ſtreams.

The ſun heats the air of our atmoſphere moſt near the ſurface of the earth; for there, beſides the direct rays, there are many reflections. Moreover, the earth itſelf being heated, communicates of its heat to the neighbouring air.

The higher regions having only the direct rays of the ſun paſſing through them, are comparatively very cold. Hence the cold air on the tops of mountains, and ſnow on ſome of them all the year, even in the Torrid zone. Hence hail in ſummer.

If the atmoſphere were, all of it (both above and below) always of the ſame temper as to cold or heat, then the upper air would always be *rarer* than the lower, becauſe the preſſure on it is leſs; conſequently lighter, and therefore would keep its place.

But the upper air may be more condenſed by cold than the lower air by preſſure; the lower more expanded by heat, than the upper for want of preſſure. In ſuch caſe the upper air will become the heavier, the lower the lighter.

The lower region of air being heated and expanded, heaves up, and ſupports for ſome time the colder heavier air above, and will continue to ſupport it while the equilibrium is kept. Thus water is ſupported in an inverted open glaſs, while the equilibrium is maintained by the equal preſſure upwards of the air below; but the equilibrium by any means breaking, the water deſcends on the heavier ſide, and the air riſes into its place.

The lifted heavy cold air over a heated country, becoming by any means unequally ſupported, or unequal in its weight, the heavieſt part deſcends firſt, and the reſt follows impetuouſly. Hence guſts after heats, and hurricanes in hot climates. Hence the air of guſts and hurricanes cold, though in hot climes and ſeaſons; it coming from above.

The cold air deſcending from above, as it penetrates our warm region full of watry particles, condenſes them, renders them viſible, forms a cloud thick and dark, overcaſting ſometimes, at once, large and extenſive; ſometimes, when ſeen at a diſtance, ſmall at firſt, gradually increaſing; the cold edge, or ſurface, of the cloud, condenſing the vapours next it, which form ſmaller clouds that join it, increaſe its bulk, it deſcends with the wind and its acquired weight, draws nearer the earth, grows denſer with continual additions of water, and diſcharges heavy ſhowers.

Small black clouds thus appearing in a clear ſky, in hot climates, portend ſtorms, and warn ſeamen to hand their ſails.

The earth turning on its axis in about twenty-fours, the equatorial parts muſt move about fifteen miles in each minute; in Northern and Southern latitudes this motion is gradually leſs to the Poles, and there nothing.

If

If there was a general calm over the face of the globe, it muſt be by the air's moving in every part as faſt as the earth or ſea it covers.

He that ſails, or rides, has inſenſibly the ſame degree of motion as the ſhip or coach with which he is connected. If the ſhip ſtrikes the ſhore, or the coach ſtops ſuddenly, the motion continuing in the man, he is thrown forward. If a man were to jump from the land into a ſwift ſailing ſhip, he would be thrown backward (or towards the ſtern) not having at firſt the motion of the ſhip.

He that travels, by ſea or land, towards the equinoctial, gradually acquires motion; from it, loſes.

But if a man were taken up from latitude 40 (where ſuppoſe the earth's ſurface to move twelve miles *per* minute) and immediately ſet down at the equinoctial, without changing the motion he had, his heels would be ſtruck up, he would fall weſtward. If taken up from the equinoctial, and ſet down in latitude 40, he would fall eaſtward.

The air under the equator, and between the tropics, being conſtantly heated and rarified by the ſun, riſes. Its place is ſupplied by air from Northern and Southern latitudes, which coming from parts where the earth and air had leſs motion, and not ſuddenly acquiring the quicker motion of the equatorial earth, appears an Eaſt wind blowing Weſtward; the earth moving from Weſt to Eaſt, and ſlipping under the air.

Thus, when we ride in a calm, it ſeems a wind againſt us: If we ride with the wind, and faſter, even that will ſeem a ſmall wind againſt us.

The air rarified between the Tropics, and rising, must flow in the higher region North and South. Before it rose, it had acquired the greatest motion the earth's rotation could give it. It retains some degree of this motion, and descending in higher latitudes, where the earth's motion is less, will appear a Westerly wind, yet tending towards the equatorial parts, to supply the vacancy occasioned by the air of the lower regions flowing thitherwards.

Hence our general cold winds are about North-West, our summer cold gusts the same.

The air in sultry weather, though not cloudy, has a kind of haziness in it, which makes objects at a distance appear dull and indistinct. This haziness is occasioned by the great quantity of moisture equally diffused in that air. When, by the cold wind blowing down among it, it is condensed into clouds, and falls in rain, the air becomes purer and clearer. Hence, after gusts, distant objects appear distinct, their figures sharply terminated.

Extream cold winds congeal the surface of the earth; by carrying off its fire. Warm winds afterwards blowing over that frozen surface, will be chilled by it. Could that frozen surface be turned under, and a warmer turned up from beneath it, those warm winds would not be chilled so much.

The surface of the earth is also sometimes much heated by the sun; and such heated surface not being changed, heats the air that moves over it.

Seas, lakes, and great bodies of water, agitated by the winds, continually change surfaces; the cold surface in win-

winter is turned under, by the rolling of the waves, and a warmer turned up; in ſummer, the warm is turned under, and colder turned up. Hence the more equal temper of ſea-water, and the air over it. Hence, in winter, winds from the ſea ſeem warm, winds from the land cold. In ſummer the contrary.

Therefore the lakes North-Weſt of us*, as they are not ſo much frozen, nor ſo apt to freeze as the earth, rather moderate than increaſe the coldneſs of our winter winds.

The air over the ſea being warmer, and therefore lighter in winter than the air over the frozen land, may be another cauſe of our general N. W. winds, which blow off to ſea at right angles from our *North-American* coaſt. The warm light ſea air riſing, the heavy cold land air preſſing into its place.

Heavy fluids deſcending, frequently form eddies, or whirlpools, as is ſeen in a funnel, where the water acquires a circular motion, receding every way from a center, and leaving a vacancy in the middle, greateſt above, and leſſening downwards, like a ſpeaking trumpet, its big end upwards.

Air deſcending, or aſcending, may form the ſame kind of eddies, or whirlings, the parts of air acquiring a circular motion, and receding from the middle of the circle by a centrifugal force, and leaving there a vacancy; if deſcending, greateſt above, and leſſening downwards; if aſcending, greateſt below, and leſſening upwards; like a ſpeaking trumpet ſtanding its big end on the ground.

When

* In *Penſylvania.*

When the air defcends with violence in fome places, it may rife with equal violence in others, and form both kinds of whirlwinds.

The air in its whirling motion receding every way from the center or axis of the trumpet, leaves there a vacuum; which cannot be filled through the fides, the whirling air, as an arch, preventing; it muft then prefs in at the open ends.

The greateft preffure inwards muft be at the lower end, the greateft weight of the furrounding atmofphere being there. The air entering, rifes within, and carries up duft, leaves, and even heavier bodies that happen in its way, as the eddy, or whirl, paffes over land.

If it paffes over water, the weight of the furrounding atmofphere forces up the water into the vacuity, part of which, by degrees, joins with the whirling air, and adding weight, and receiving accelerated motion, recedes ftill farther from the center or axis of the trump, as the preffure leffens; and at laft, as the trump widens, is broken into fmall particles, and fo united with air as to be fupported by it, and become black clouds at the top of the trump.

Thus thefe eddies may be whirlwinds at land, water-fpouts at fea. A body of water fo raifed may be fuddenly let fall, when the motion, *&c.* has not ftrength to fupport it, or the whirling arch is broken fo as to let in the air; falling in the fea, it is harmlefs, unlefs fhips happen under it. But if in the progreffive motion of the whirl, it has moved from the fea, over the land, and there breaks, fudden, violent, and mifchievous torrents are the confequences.

LET-

LETTER XVI.

From Dr *** of *Boston*,

TO

BENJ. FRANKLIN, *Esq*; of *Philadelphia*.

SIR, *Boston*, *Aug.* 3, 1752.

THIS comes to you on account of Dr *Douglass*: He desired me to write to you for what you know of the number that died of the inoculation in *Philadelphia*, telling me he designed to write something on the small-pox shortly. We shall both be obliged to you for a word on this affair.

The chief particulars of our visitation, you have in the public prints. But the less degree of mortality than usual in the common way of infection, seems chiefly owing to the purging method designed to prevent the secondary fever; a method first begun and carried on in this town, and with success beyond expectation. We lost one in 11 $\frac{1}{6}$, but had we been experienced in this way, at the first coming of the distemper, probably the proportion had been but one in in 13 or 14. In the year 1730 we loſt one in nine, which is more favourable than ever before with us. The diſtemper pretty much the ſame then as now, but ſome circumſtances not ſo kind this time.

If there be any particulars which you want to know, pleaſe to ſignify what they are, and I ſhall ſend them.

The number of our inhabitants decreaſes*. On a ſtrict inquiry, the overſeers of the poor find but 14,190 Whites, and 1,544 Blacks, including thoſe abſent, on account of the ſmall-pox, many of whom, it is probable, will never return.

I paſs this opportunity without any particulars of my old theme. One thing, however, I muſt mention, which is, that perhaps my laſt letters contained ſomething that ſeemed to militate with your doctrine of the *Origin, &c.* But my deſign was only to relate the phænomena as they appeared to me. I have received ſo much light and pleaſure from your writings, as to prejudice me in favour of every thing from your hand, and leave me only liberty to obſerve, and a power of diſſenting when ſome great probability might oblige me: And if at any time that be the caſe, you will certainly hear of it.

I am, Sir, &c.

* *Boſton* is an old town, and was formerly the ſeat of all the trade of the country, that was carried on by ſea. New towns, and ports, have, of late, divided the trade with it, and diminiſhed its inhabitants, though the inhabitants of the country, in general, have greatly increaſed.

LET-

LETTER XVII.

FROM

BENJ. FRANKLIN, *Esq*; of *Philadelphia*.

To Doctor —— of *Boston*.

SIR, *Philadelphia*, *Aug.* 13, 1752.

I Received your favour of the 3d instant. Some time last winter I procured from one of our physicians an account of the number of persons inoculated during the five visitations of the small-pox we have had in 22 years; which account I sent to Mr *W*—— *V*——, of your town, and have no copy. If I remember right, the number exceeded 800, and the deaths were but 4. I suppose Mr *V*—— will shew you the account, if he ever received it. Those four were all that our doctors allow to have died of the small-pox by inoculation, though I think there were two more of the inoculated who died of the distemper; but the eruptions appearing soon after the operation, it is supposed they had taken the infection before, in the common way.

I ſhall be glad to ſee what Dr *Douglaſs* may write on the ſubject. I have a *French* piece printed at *Paris* 1724, entitled, *Obſervations ſur la Saignée du Pied, et ſur la Purgation au commencement de la Petite Verole, & Raiſons de doubte contre l'Inoculation.*—A letter of the doctor's is mentioned in it. If he or you have it not, and deſire to ſee it, I'll ſend it.—Pleaſe to favour me with the particulars of your purging method, to prevent the ſecondary fever.

I am indebted for your preceding letter, but buſineſs ſometimes obliges one to poſtpone philoſophical amuſements. Whatever I have wrote of that kind, are really, as they are entitled, but *Conjectures* and *Suppoſitions*; which ought always to give place, when careful obſervation militates againſt them. I own I have too ſtrong a penchant to the building of hypotheſes; they indulge my natural indolence: I wiſh I had more of your patience and accuracy in making obſervations, on which, alone, true Philſophy can be founded. And, I aſſure you, nothing can be be more obliging to me, than your kind communication of thoſe you make, however they may diſagree with my pre-conceived notions.

I am ſorry to hear that the number of your inhabitants decreaſes. I ſome time ſince, wrote a ſmall paper of *Thoughts on the peopling of Countries*, which, if I can find, I will ſend you, to obtain your ſentiments. The favourable opinion you expreſs of my writings, may, you ſee, occaſion you more trouble than you expected from, Sir,

Yours, &c. B. F.

Ob-

OBSERVATIONS *concerning the Increase of Mankind, peopling of Countries,* &c. *Written in* Pensilvania, 1751.

1. TABLES of the proportion of marriages to births, of deaths to births, of marriages to the numbers of inhabitants, *&c.* formed on observations made upon the bills of mortality, christenings, *&c.* of populous cities, will not suit countries; nor will tables formed on observations made on full settled old countries, as *Europe*, suit new countries, as *America.*

2. For people increase in proportion to the number of marriages, and that is greater in proportion to the ease and convenience of supporting a family. When families can be easily supported, more persons marry, and earlier in life.

3. In cities, where all trades, occupations, and offices are full, many delay marrying, till they can see how to bear the charges of a family; which charges are greater in cities, as luxury is more common; many live single during life, and continue servants to families, journeymen to trades, *&c.* Hence cities do not, by natural generation, supply themselves with inhabitants; the deaths are more than the births.

4. In countries full settled, the case must be nearly the same; all lands being occupied and improved to the heighth; those who cannot get land, must labour for others that have it; when labourers are plenty, their wages will

will be low; by low wages a family is ſupported with difficulty; this difficulty deters many from marriage, who, therefore, long continue ſervants, and ſingle.—Only as the cities take ſupplies of people from the country, and thereby make a little more room in the country, marriage is a little more encouraged there, and the births exceed the deaths.

5. Great part of *Europe* is full ſettled with huſbandmen, manufacturers, *&c.* and therefore cannot now much encreaſe in people. *America* is chiefly occupied by *Indians*, who ſubſiſt moſtly by hunting—But as the hunter, of all men, requires the greateſt quantity of land from whence to draw his ſubſiſtence, (the huſbandman ſubſiſting on much leſs, the gardener on ſtill leſs, and the manufacturer requiring leaſt of all) the *Europeans* found *America* as fully ſettled as it well could be by hunters; yet theſe having large tracts, were eaſily prevailed on to part with portions of territory to the new comers, who did not much interfere with the natives in hunting, and furniſhed them with many things they wanted.

6. Land being thus plenty in *America*, and ſo cheap as that a labouring man that underſtands huſbandry, can, in a ſhort time, ſave money enough to purchaſe a piece of new land, ſufficient for a plantation, whereon he may ſubſiſt a family; ſuch are not afraid to marry; for if they even look far enough forward to conſider how their children, when grown up, are to be provided for, they ſee that

more

more land is to be had at rates equally easy, all circumstances considered.

7. Hence marriages in *America* are more general, and more generally early than in *Europe*. And if it is reckoned there, that there is but one marriage *per Annum* among 100 persons, perhaps we may here reckon two; and if in *Europe* they have but four births to a marriage, (many of their marriages being late) we may here reckon eight; of which, if one half grow up, and our marriages are made, reckoning one with another, at twenty years of age, our people must at least be doubled every twenty years.

8. But notwithstanding this increase, so vast is the territory of *North-America*, that it will require many ages to settle it fully; and till it is fully settled, labour will never be cheap here, where no man continues long a labourer for others, but gets a plantation of his own; no man continues long a journeyman to a trade, but goes among those new settlers, and sets up for himself, *&c.* Hence labour is no cheaper now, in *Pensilvania*, than it was thirty years ago, though so many thousand labouring people have been imported from *Germany* and *Ireland*.

9. The danger, therefore, of these colonies interfereing with their mother country in trades that depend on labour, manufactures, *&c.* is too remote to require the attention of *Great-Britain*.

10. But in proportion to the increase of the colonies, a vast demand is growing for *British* manufactures; a glorious market, wholly in the power of *Britain*, in which foreigners

foreigners cannot interfere, which will increaſe, in a ſhort time, even beyond her power of ſupplying, though her whole trade ſhould be to her colonies. * * * *

12. 'Tis an ill-grounded opinion, that by the labour of ſlaves, *America* may poſſibly vie in cheapneſs of manufactures with *Britain.* The labour of ſlaves can never be ſo cheap here, as the labour of working men is in *Britain.* Any one may compute it. Intereſt of money is in the colonies from 6 to 10 *per Cent.* Slaves, one with another, coſt 30*l.* ſterling *per* head. Reckon then the intereſt of the firſt purchaſe of a ſlave, the inſurance or riſque on his life, his cloathing and diet, expences in his ſickneſs, and loſs of time, loſs by his neglect of buſineſs, (neglect is natural to the man who is not to be benefited by his own care or diligence) expence of a driver to keep him at work, and his pilfering from time to time, almoſt every ſlave being, from the nature of ſlavery, a thief; and compare the whole amount with the wages of a manufacturer of iron or wool in *England*, you will ſee that labour is much cheaper there, than it ever can be by negroes here. Why then will *Americans* purchaſe ſlaves? Becauſe ſlaves may be kept as long as a man pleaſes, or has occaſion for their labour; while hired men are continually leaving their maſter (often in the midſt of his buſineſs) and ſetting up for themſelves. § 8.

13. As the increaſe of people depends on the encouragement of marriages, the following things muſt diminiſh a nation, *viz.* 1. The being conquered; for the conquerors

querors will engrofs as many offices, and exact as much tribute or profit on the labour of the conquered, as will maintain them in their new eftablifhment; and this diminifhing the fubfiftence of the natives, difcourages their marriages, and fo gradually diminifhes them, while the foreigners increafe. 2. Lofs of territory. Thus the *Britons* being driven into *Wales*, and crouded together in a barren country, infufficient to fupport fuch great numbers, diminifhed, till the people bore a proportion to the produce, while the *Saxons* increafed on their abandoned lands, 'till the ifland became full of *Englifh*. And, were the *Englifh* now driven into *Wales* by fome foreign nation, there would, in a few years, be no more *Englifhmen* in *Britain*, than there are now people in *Wales*. 3. Lofs of trade. Manufactures exported, draw fubfiftence from foreign countries for numbers; who are thereby enabled to marry and raife families. If the nation be deprived of any branch of trade, and no new employment is found for the people occupied in that branch, it will foon be deprived of fo many people. 4. Lofs of food. Suppofe a nation has a fifhery, which not only employs great numbers, but makes the food and fubfiftence of the people cheaper: if another nation becomes mafter of the feas, and prevents the fifhery, the people will diminifh in proportion as the lofs of employ, and dearnefs of provifion makes it more difficult to fubfift a family. 5. Bad government and infecure property. People not only leave fuch a country, and fettling abroad incorporate with other nations, lofe their

their native language, and become foreigners; but the induſtry of thoſe that remain being diſcouraged, the quantity of ſubſiſtence in the country is leſſened, and the ſupport of a family becomes more difficult. So heavy taxes tend to diminiſh a people. 6. The introduction of ſlaves. The negroes brought into the *Engliſh* ſugar iſlands, have greatly diminiſhed the Whites there; the poor are by this means deprived of employment, while a few families acquire vaſt eſtates, which they ſpend on foreign luxuries, and educating their children in the habit of thoſe luxuries; the ſame income is needed for the ſupport of one, that might have maintained one hundred. The whites, who have ſlaves not labouring, are enfeebled, and therefore not ſo generally prolific; the ſlaves being worked too hard, and ill fed, their conſtitutions are broken, and the deaths among them are more than the births; ſo that a continual ſupply is needed from *Africa*. The northern colonies having few ſlaves, increaſe in whites. Slaves alſo pejorate the families that uſe them; the white children become proud, diſguſted with labour, and being educated in idleneſs, are rendered unfit to get a living by induſtry.

14. Hence the prince that acquires new territory, if he finds it vacant, or removes the natives to give his own people room; the legiſlator that makes effectual laws for promoting of trade, increaſing employment, improving land by more or better tillage, providing more food by fiſheries, ſecuring property, *&c.* and the man that invents new

new trades, arts, or manufactures, or new improvements in husbandry, may be properly called *Fathers of their nation*, as they are the cause of the generation of multitudes, by the encouragement they afford to marriage.

15. As to privileges granted to the married, (such as the *jus trium liberorum* among the *Romans*) they may hasten the filling of a country that has been thinned by war or pestilence, or that has otherwise vacant territory, but cannot increase a people beyond the means provided for their subsistence.

16. Foreign luxuries, and needless manufactures, imported and used in a nation, do, by the same reasoning, increase the people of the nation that furnishes them, and diminish the people of the nation that uses them.—Laws, therefore, that prevent such importations, and, on the contrary, promote the exportation of manufactures to be consumed in foreign countries, may be called (with respect to the people that make them) *generative laws*, as by increasing subsistence they encourage marriage. Such laws likewise strengthen a country doubly, by increasing its own people, and diminishing its neighbours.

17. Some *European* nations prudently refuse to consume the manufactures of *East-India*:—They should likewise forbid them to their colonies; for the gain to the merchant is not to be compared with the loss, by this means, of people to the nation.

18. Home luxury in the great, increases the nation's manufacturers employed by it, who are many, and only

tends to diminifh the families that indulge in it, who are few. The greater the common fafhionable expence of any rank of people, the more cautious they are of marriage. Therefore luxury fhould never be fuffered to become common.

19. The great increafe of offspring in particular families, is not always owing to greater fecundity of nature, but fometimes to examples of induftry in the heads, and induftrious education; by which the children are enabled to provide better for themfelves, and their marrying early is encouraged from the profpect of good fubfiftence.

20. If there be a fect, therefore, in our nation, that regard frugality and induftry as religious duties, and educate their children therein, more than others commonly do; fuch fect muft confequently increafe more by natural generation, than any other fect in *Britain*.

21. The importation of foreigners into a country that has as many inhabitants as the prefent employments and provifions for fubfiftence will bear, will be in the end no increafe of people, unlefs the new-comers have more induftry and frugality than the natives, and then they will provide more fubfiftence, and increafe in the country; but they will gradually eat the natives out.—Nor is it neceffary to bring in foreigners to fill up any occafional vacancy in a country; for fuch vacancy (if the laws are good, § 14, 16) will foon be filled by natural generation. Who can now find the vacancy made in *Sweden*, *France*, or other warlike nations, by the plague of heroifm 40 years ago;

ago; in *France*, by the expulsion of the Protestants; in *England*, by the settlement of her colonies; or in *Guinea*, by a hundred years exportation of slaves, that has blackened half *America*?—The thinness of the inhabitants in *Spain*, is owing to national pride, and idleness, and other causes, rather than to the expulsion of the *Moors*, or to the making of new settlements.

22. There is, in short, no bound to the prolific nature of plants or animals, but what is made by their crowding and interfering with each other's means of subsistence. Was the face of the earth vacant of other plants, it might be gradually sowed and overspread with one kind only; as for instance, with Fennel; and were it empty of other inhabitants, it might, in a few ages, be replenished from one nation only, as for instance, with *Englishmen*. Thus there are supposed to be now upwards of one million *English* souls in *North-America*, (though it is thought scarce 80,000 have been brought over sea) and yet perhaps there is not one the fewer in *Britain*, but rather many more, on account of the employment the colonies afford to manufacturers at home. This million doubling, suppose but once in 25 years, will, in another century, be more than the people of *England*, and the greatest number of *Englishmen* will be on this side the water. What an accession of power to the *British* empire by sea as well as land! What increase of trade and navigation! What numbers of ships and seamen! We have been here but little more than a hundred years, and yet the force of our privateers in the

late war, united, was greater, both in men and guns, than that of the whole *Britiſh* navy in Queen *Elizabeth*'s time.——How important an affair, then, to *Britain*, is the preſent treaty * for ſettling the bounds between her colonies, and the *French!* and how careful ſhould ſhe be to ſecure room enough, ſince on the room depends ſo much the increaſe of her people?

23. In fine, a nation well regulated is like a polypus †; take away a limb, its place is ſoon ſupplied; cut it in two, and each deficient part ſhall ſpeedily grow out of the part remaining. Thus if you have room and ſubſiſtence enough, as you may, by dividing, make ten polypuſes out of one, you may, of one, make ten nations, equally populous and powerful; or, rather, increaſe a nation ten fold in numbers and ſtrength. * * * * *

* In 1751. † A water-inſect, well known to Naturaliſts.

LETTER XVIII.

From Doctor ——, of *Boſton*,

TO

BENJAMIN FRANKLIN, Eſq; at *Philadelphia*.

S I R, *Boſton, October* 16, 1752.

Read at the Royal Society *June* 3, 1756.

I Find, by a word or two in your laſt, that you are willing to be found fault with; which authoriſes me to let you know what I am at a loſs about in your papers, which is only in the article of the water-ſpout. I am in doubt, whether water in bulk, or even broken into drops, ever aſcends into the region of the clouds, *per verticem,* (*i. e.*) whether there be, in reality, what I call a direct water-ſpout. I make no doubt of direct and inverted whirl-winds; your deſcription of them, and the reaſon of the thing, are ſufficient. I am ſenſible, too, that they are very ſtrong, and often move conſiderable weights. But I have not met with any hiſtorical accounts that ſeem exact enough to remove my ſcruples concerning the aſcent above ſaid.

Deſcend-

Deſcending ſpouts (as I take them to be) are many times ſeen, as I take it, in the calms, between the ſea and land trade-winds, on the coaſt of *Africa.* Theſe contrary winds, or diverging, I can conceive may occaſion them, as it were by ſuction, making a breach in a large cloud. But I imagine they have, at the ſame time, a tendency to hinder any direct or riſing ſpout, by carrying off the lower part of the atmoſphere, as faſt as it begins to rarefy; and yet ſpouts are frequent here, which ſtrengthens my opinion, that all of them deſcend.

But however this be, I cannot conceive a force producible by the rarification and condenſation of our atmoſphere, in the circumſtances of our globe, capable of carrying water, in large portions, into the region of the clouds. Suppoſing it to be raiſed, it would be too heavy to continue the aſcent beyond a conſiderable height, unleſs parted into ſmall drops; and, even then, by its centrifugal force, from the manner of conveyance, it would be flung out of the circle, and fall ſcattered, like rain.

But I need not expatiate on theſe matters to you. I have mentioned my objections, and, as truth is my purſuit, ſhall be glad to be informed. I have ſeen few accounts of theſe whirl, or eddy winds, and as little of the ſpouts; and theſe, eſpecially, lame and poor things to obtain any certainty by. If you know any thing determinate that has been obſerved, I ſhall hope to hear from you; as alſo of any miſtake in my thoughts.

I have

I have nothing to object to any other part of your suppositions; and as to that of the trade-winds, I believe nobody can.

I am, &c.

P. S. The figures in the *Philosophical Transactions* shew, by several circumstances, that they all descended, though the relators seemed to think they took up water.

LETTER XIX.

From Doctor ——, of *Boston*,

TO

BENJAMIN FRANKLIN, *Esq*; of *Philadelphia*.

SIR, *Boston, October* 23, 1752.

Read at the Royal Society, *June* 24, 1754.

IN the inclosed you have all I have to say of that matter *. It proved longer than I expected, so that I was forced to add a cover to it. I confess it looks like a dispute; but that is quite contrary to my intentions. The sincerity of friendship and esteem were my motives; nor do I doubt

* Water-spouts.

your

your ſcrupling the goodneſs of the intention. However, I muſt confeſs I cannot tell exactly how far I was acted by hopes of better information, in diſcovering the whole foundation of my opinion, which, indeed, is but an opinion, as I am very much at a loſs about the validity of the reaſons. I have not been able to differ from you in ſentiment concerning any thing elſe in your *Suppoſitions.* In the preſent caſe I lie open to conviction, and ſhall be the gainer when informed. If I am right, you will know that, without my adding any more. Too much ſaid on a merely ſpeculative matter, is but a robbery committed on practical knowledge. Perhaps I am too much pleaſed with theſe dry notions: However, by this you will ſee that I think it unreaſonable to give you more trouble about them, than your leiſure and inclination may prompt you to.

I am, &c.

SINCE my laſt I conſidered, that, as I had begun with the reaſons of my diſſatisfaction about the aſcent of water in ſpouts, you would not be unwilling to hear the whole I have to ſay, and then you will know what I rely upon.

What occaſioned my thinking all ſpouts deſcend, is, that I found ſome did certainly do ſo. A difficulty appeared concerning the aſcent of ſo heavy a body as water, by any force I was appriſed of, as probably ſufficient.

And,

And, above all, a view of Mr *Stuart's* portraits of ſpouts, in the *Philoſophical Tranſactions.*

Some obſervations on theſe laſt, will include the chief part of my difficulties.

Mr *Stuart* has given us the figures of a number obſerved by him in the *Mediterranean*: All with ſome particulars which make for my opinion, if well drawn.

The great ſpattering which relators mention in the water where the ſpout deſcends, and which appears in all his draughts, I conceive to be occaſioned by drops deſcending very thick and large into the place.

On the place of this ſpattering ariſes the appearance of a buſh, into the center of which the ſpout comes down. This buſh I take to be formed by a ſpray, made by the force of theſe drops, which being uncommonly large, and deſcending with unuſual force, by a ſtream of wind deſcending from the cloud with them, increaſes the height of the ſpray; which wind being repulſed by the ſurface of the waters, rebounds and ſpreads; by the firſt raiſing the ſpray higher than otherwiſe it would go; and by the laſt making the top of the buſh appear to bend outwards (*i. e.*) the cloud of ſpray is forc'd off from the trunk of the ſpout, and falls backward.

The buſh does the ſame, where there is no appearance of a ſpout reaching it; and is depreſſed in the middle, where the ſpout is expected. This, I imagine, to be from numerous drops of the ſpout falling into it, together with the wind I mentioned, by their deſcent, which beat back the riſing ſpray in the center.

This circumſtance, of the buſh bending outwards at the top, ſeems not to agree with what I call a direct whirlwind, but conſiſtent with the revers'd; for a direct one would ſweep the buſh inwards; if, in that caſe, any thing of a buſh would appear.

The pillar of water, as they call it, from its likeneſs, I ſuppoſe to be only the end of the ſpout immers'd in the buſh, a little blacken'd by the additional cloud, and, perhaps, appears to the eye beyond its real bigneſs, by a refraction in the buſh, and which refraction may be the cauſe of the appearance of ſeparation, betwixt the part in the buſh, and that above it. The part in the buſh is cylindrical, as it is above (*i. e.*) the bigneſs the ſame from the top of the buſh to the water. Inſtead of this ſhape, in caſe of a whirlwind, it muſt have been pyramidical.

Another thing remarkable, is, the curve in ſome of them: This is eaſy to conceive, in caſe of deſcending parcels of drops through various winds, at leaſt till the cloud condenſes ſo faſt as to come down, as it were, *uno rivo.* But it is harder to me to conceive it in the aſcent of water, that it ſhould be conveyed along, ſecure of not leaking, or often dropping through the under ſide, in the prone part: And, ſhould the water be conveyed ſo ſwiftly, and with ſuch force, up into the cloud, as to prevent this; it would, by a natural diſpoſition to move on

in

in a present direction, presently straiten the curve, raising the shoulder very swiftly, till lost in the cloud.

Over every one of *Stuart's* figures, I see a cloud: I suppose his clouds were first, and then the spout; I do not know whether it be so with all spouts, but suppose it is. Now, if whirlwinds carried up the water, I should expect them in fair weather, but not under a cloud; as is observable of whirlwinds; they come in fair weather, not under the shade of a cloud, nor in the night; since shade cools the air: But, on the contrary, violent winds often descend from the clouds; strong gusts which occupy small spaces; and from the higher regions, extensive hurricanes, *&c.*

Another thing is, the appearance of the spout *coming from* the cloud. This I cannot account for on the notion of a direct spout, but, in the real descending one, it is easy. I take it, that the cloud begins first of all to pour out drops at that particular spot, or *foramen*; and, when that current of drops increases, so as to force down wind and vapour, the spout becomes, so far as that goes, opaque. I take it, that no clouds drop spouts, but such as make very fast, and happen to condense in a particular spot, which, perhaps, is coldest, and gives a determination downwards, so as to make a passage through the subjacent atmosphere.

If spouts ascend, it is to carry up the warm rarified air below, to let down all and any that is colder above; and, if so, they must carry it through the cloud they go

 into.

into, (for that is cold and denſe, I imagine) perhaps far into the higher region, making a wonderful appearance at a convenient diſtance to obſerve it, by the ſwift riſe of a body of vapour, above the region of the clouds. But, as this has never been obſerved in any age, if it be ſuppoſeable that is all.

I cannot learn, by mariners, that any wind blows towards a ſpout more than any other way; but it blows towards a whirlwind, for a large diſtance round.

I ſuppoſe there has been no inſtance of the water of a ſpout being ſalt, when coming a-croſs any veſſel at ſea. I ſuppoſe, too, that there have been no ſalt rains; theſe would make the caſe clear.

I ſuppoſe it is from ſome unhappy effects of theſe dangerous creatures of nature, that ſailors have an univerſal dread on them of breaking in their decks, ſhould they come acroſs them.

I imagine ſpouts, in cold ſeaſons, as *Gordon*'s in the *Downs*, prove the deſcent.

Query. Whether there is not always more or leſs cloud, firſt, where a ſpout appears?

Whether they are not, generally, on the borders of trade-winds; and whether this is for, or againſt me?

Whether there be any credible account of a whirlwind's carrying up all the water in a pool, or ſmall pond: As when ſhoal, and the banks low, a ſtrong guſt might be ſuppoſed to blow it all out?

Whether

Whether a violent tornado, of a ſmall extent, and other ſudden and ſtrong guſts, be not winds from above, deſcending nearly perpendicular; and, whether many that are called whirlwinds at ſea, are any other than theſe; and ſo might be called air-ſpouts, if they were objects of ſight?

I overlooked, in its proper place, *Stuart's* No. 11, which is curious for its inequalities, and, in particular, the approach to breaking, which, if it would not be too tedious, I would have obſerv'd a little upon, in my own way, as, I think, this would argue againſt the aſcent, *&c.* but I muſt paſs it, not only for the reaſon mentioned, but want of room beſides.

As to Mr *Stuart's* ocular demonſtration of the aſcent in his great perpendicular ſpout, the only one it appears in, I ſay, as to this, what I have written ſuppoſes him miſtaken, which, yet, I am far from aſſerting.

The force of an airy vortex, having leſs influence on the ſolid drops of water, than on the interſperſed cloudy vapours, makes the laſt whirl round ſwifter, though it deſcend ſlower: And this might eaſily deceive, without great care, the moſt unprejudiced perſon.

LET-

LETTER XX.

FROM

BENJ. FRANKLIN, *Esq*; of *Philadelphia*.

To Doctor *** of *Boston*.

SIR, *Philadelphia*, *Feb.* 4, 1753.

Read at the Royal Society *June* 24, 1756.

I Ought to have written to you, long ſince, in anſwer to yours of *October* 16, concerning the water-ſpout; but buſineſs partly, and partly a deſire of procuring further information, by enquiry among my ſea-faring acquaintance, induced me to poſtpone writing, from time to time, till I am now almoſt aſhamed to reſume the ſubject, not knowing but you may have forgot what has been ſaid upon it.

Nothing, certainly, can be more improving to a ſearcher into nature, than objections judiciouſly made to his opinion, taken up, perhaps, too haſtily: For ſuch objections oblige him to re-ſtudy the point, conſider every circumſtance carefully, compare facts, make experiments, weigh arguments, and be ſlow in drawing concluſions. And

And hence a ſure advantage reſults; for he either confirms a truth, before too ſlightly ſupported; or diſcovers an error, and receives inſtruction from the objector.

In this view I conſider the objections and remarks you ſent me, and thank you for them ſincerely: But, how much ſoever my inclinations lead me to Philoſophical inquiries, I am ſo engaged in buſineſs, public and private, that thoſe more pleaſing purſuits are frequently interrupted, and the chain of thought, neceſſary to be cloſely continued in ſuch diſquiſitions, ſo broken and disjointed, that it is with difficulty I ſatisfy myſelf in any of them: And I am now not much nearer a concluſion, in this matter of the ſpout, than when I firſt read your letter.

Yet, hoping we may, in time, ſift out the truth between us, I will ſend you my preſent thoughts, with ſome obſervations on your reaſons, on the accounts in the *Tranſactions*, and on other relations I have met with. Perhaps, while I am writing, ſome new light may ſtrike me, for I ſhall now be obliged to conſider the ſubject with a little more attention.

I agree with you, that, by means of a vacuum in a whirlwind, water cannot be ſuppoſed to riſe in large maſſes to the region of the clouds; for the preſſure of the ſurrounding atmoſphere could not force it up in a continued body, or column, to a much greater height than thirty feet. But, if there really is a vacuum in the center, or near the axis of whirlwinds, then, I think, water may

may rise in such vacuum to that height, or to less height, as the vacuum may be less perfect.

I had not read *Stuart*'s account, in the *Transactions*, for many years, before the receipt of your letter, and had quite forgot it; but now, on viewing his draughts, and considering his descriptions, I think they seem to favour *my hypothesis*; for he describes and draws columns of water, of various heights, terminating abruptly at the top, exactly as water would do, when forced up by the pressure of the atmosphere, into an exhausted tube.

I must, however, no longer call it *my hypothesis*, since I find *Stuart* had the same thought, though somewhat obscurely expressed, where he says "he imagines this phæ-"nomenon may be solv'd by suction (improperly so call-"ed) or rather pulsion, as in the application of a cup-"ping glass to the flesh, the air being first voided by the "kindled flax."

In my paper, I supposed a whirlwind and a spout to be the same thing, and to proceed from the same cause; the only difference between them being, that the one passes over land, the other over water. I find, also, in the *Transactions*, that M. *de la Pryme* was of the same opinion; for he there describes two spouts, as he calls them, which were seen at different times, at *Hatfield* in *Yorkshire*, whose appearances in the air were the same with those of the spouts at sea, and effects the same with those of real whirlwinds.

Whirl-

Whirlwinds have, generally, a progreſſive, as well as a circular motion; ſo had what is called the ſpout, at *Topſham—(See the account of it in the Tranſactions)*—which alſo appears, by its effects deſcribed, to have been a real whirlwind. Water-ſpouts have, alſo, a progreſſive motion; this is ſometimes greater, and ſometimes leſs; in ſome violent, in others barely perceivable. The whirlwind at *Warrington* continued long in *Acrement-Cloſe.*

Whirlwinds generally ariſe after calms and great heats: The ſame is obſerved of water-ſpouts, which are, therefore, moſt frequent in the warm latitudes. The ſpout that happened in cold weather, in the *Downs*, deſcribed by Mr *Gordon* in the *Tranſactions*, was, for that reaſon, thought extraordinary; but he remarks withal, that the weather, though cold when the ſpout appeared, was ſoon after much colder; as we find it, commonly, leſs warm after a whirlwind.

You agree, that the wind blows every way towards a whirlwind, from a large ſpace round. An intelligent whaleman of *Nantucket*, informed me, that three of their veſſels, which were out in ſearch of whales, happening to be becalmed, lay in ſight of each other, at about a league diſtance, if I remember right, nearly forming a triangle: After ſome time, a water-ſpout appeared near the middle of the triangle, when a briſk breeze of wind ſprung up, and every veſſel made ſail; and then it appeared to them all, by the ſetting of the ſails, and the courſe each veſſel ſtood, that the ſpout was to the leeward of every one of them; and they all declared it to have been ſo, when they happened afterwards in company, and came to confer about it. So that in this particular likewiſe, whirlwinds and water-ſpouts agree.

But, if that which appears a water-ſpout at ſea, does ſometimes, in its progreſſive motion, meet with and paſs over land, and there produce all the phænomena and effects of a whirlwind, it ſhould thence ſeem ſtill more evident, that a whirlwind and a ſpout are the ſame. I ſend you, herewith, a letter from an ingenious phyſician of my acquaintance, which gives one inſtance of this, that fell within his obſervation.

A fluid, moving from all points horizontally, towards a center, muſt, at that center, either aſcend or deſcend. Water being in a tub, if a hole be opened in the middle of the bottom, will flow from all ſides to the center, and there deſcend in a whirl. But, air flowing on and near the ſurface of land or water, from all ſides, towards a center, muſt, at that center, aſcend; the land or water hindering its deſcent.

If theſe concentring currents of air be in the upper region, they may, indeed, deſcend in the ſpout or whirlwind; but then, when the united current reached the earth or water, it would ſpread, and, probably, blow every way from the center. There may be whirlwinds of both kinds, but, from the commonly obſerved effects, I ſuſpect the riſing one to be the moſt common: When the upper air deſcends, it is, perhaps, in a greater body, extending

tending wider, as in our thunder-gusts, and without much whirling; and, when air descends in a spout, or whirlwind, I should rather expect it would press the roof of a house *inwards*, or force *in* the tiles, shingles, or thatch, force a boat down into the water, or a piece of timber into the earth, than that it would lift them up, and carry them away.

It has so happened, that I have not met with any accounts of spouts, that certainly descended; I suspect they are not frequent. Please to communicate those you mention. The apparent dropping of a pipe from the clouds towards the earth or sea, I will endeavour to explain hereafter.

The augmentation of the cloud, which, as I am informed, is generally, if not always the case, during a spout, seems to shew an ascent, rather than a descent of the matter of which such cloud is composed; for a descending spout, one would expect, should diminish a cloud. I own, however, that cold air descending, may, by condensing the vapours in a lower region, form and increase clouds; which, I think, is generally the case in our common thunder-gusts, and, therefore, do not lay great stress on this argument.

Whirlwinds, and spouts, are not always, though most commonly, in the day time. The terrible whirlwind which damaged a great part of *Rome*, *June* 11, 1749, happened in the night of that day. The same was supposed to have been first a spout, for it is said to be beyond

 doubt,

doubt, that it gathered in the neighbouring ſea, as it could be tracked from *Oſtia* to *Rome*. I find this in *Pere Boſchovich*'s account of it, as abridg'd in the *Monthly Review* for *December* 1750.

In that account, the whirlwind is ſaid to have appeared as a very black, long, and lofty cloud, diſcoverable, notwithſtanding the darkneſs of the night, by its continually lightning or emitting flaſhes on all ſides, puſhing along with a ſurprizing ſwiftneſs, and within three or four feet of the ground. Its general effects on houſes, were, ſtripping off the roofs, blowing away chimneys, breaking doors and windows, *forcing up the floors, and unpaving the rooms*, (ſome of theſe effects ſeem to agree well with a ſuppoſed vacuum in the center of the whirlwind) and the very rafters of the houſes were broke and diſperſed, and even hurled againſt houſes at a conſiderable diſtance, &c.

It ſeems, by an expreſſion of *Pere Boſchovich*'s, as if the wind blew from all ſides towards the whirlwind; for, having carefully obſerved its effects, he concludes of all whirlwinds, "that their motion is circular, and their action attractive."

He obſerves, on a number of hiſtories of whirlwinds, &c. "that a common effect of them is, to carry up into "the air tiles, ſtones, and animals themſelves, which happen to be in their courſe, and all kinds of bodies unexceptionably, throwing them to a conſiderable diſtance, "with

"with great impetuosity." Such effects seem to shew a rising current of air.

I will endeavour to explain my conceptions of this matter by figures, representing a plan and an elevation of a spout or whirlwind.

I would only first beg to be allowed two or three positions, mentioned in my former paper.

1. That the lower region of air is often more heated, and so more rarified, than the upper; consequently, specifically lighter. The coldness of the upper region is manifested by the hail which sometimes falls from it in a hot day.

2. That heated air may be very moist, and yet the moisture so equally diffus'd and rarified, as not to be visible, till colder air mixes with it, when it condenses, and becomes visible. Thus our breath, invisible in summer, becomes visible in winter.

Now, let us suppose a tract of land, or sea, of perhaps sixty miles square, unscreened by clouds, and unfanned by winds, during great part of a summer's day, or, it may be, for several days successively, till it is violently heated, together with the lower region of air in contact with it, so that the said lower air becomes specifically lighter than the superincumbent higher region of the atmosphere, in which the clouds commonly float: Let us suppose, also, that the air surrounding this tract has not been so much heated during those days, and, therefore, remains heavier. The consequence of this should be, as I conceive, that the heated

heated lighter air, being preſſed on all ſides, muſt aſcend, and the heavier deſcend; and, as this riſing cannot be in all parts, or the whole area of the tract at once, for that would leave too extenſive a vacuum, the riſing will begin preciſely in that column that happens to be the lighteſt, or moſt rarified; and the warm air will flow horizontally from all points to this column, where the ſeveral currents meeting, and joining to riſe, a whirl is naturally formed, in the ſame manner as a whirl is formed in the tub of water, by the deſcending fluid flowing from all ſides of the tub, to the hole in the center.

And, as the ſeveral currents arrive at this central riſing column, with a conſiderable degree of horizontal motion, they cannot ſuddenly change it to a vertical motion; therefore, as they gradually, in approaching the whirl, decline from right to curve or circular lines, ſo, having joined the whirl, they *aſcend* by a ſpiral motion; in the ſame manner as the water *deſcends* ſpirally through the hole in the tub before-mentioned.

Laſtly, as the lower air, and neareſt the ſurface, is moſt rarified by the heat of the ſun, that air is moſt acted on by the preſſure of the ſurrounding cold and heavy air, which is to take its place; conſequently, its motion towards the whirl is ſwifteſt, and ſo the force of the lower part of the whirl, or trump, ſtrongeſt, and the centrifugal force of its particles greateſt; and hence the vacuum round the axis of the whirl ſhould be greateſt near the earth or ſea, and be gradually diminiſhed as it ap-

proaches

proaches the region of the clouds, till it ends in a point, as at A in Fig. II. forming a long and ſharp cone.

In Fig. I. which is a plan or ground-plat of a whirlwind, the circle V. repreſents the central vacuum.

Between *aaaa* and *bbbb* I ſuppoſe a body of air condenſed ſtrongly by the preſſure of the currents moving towards it, from all ſides without, and by its centrifugal force from within; moving round with prodigious ſwiftneſs, (having, as it were, the momenta of all the currents

⟶⟶⟶⟶

united in itſelf) and with a power equal to its ſwiftneſs and denſity.

It is this whirling body of air between *aaaa* and *bbbb* that riſes ſpirally; by its force it tears buildings to pieces, twiſts up great trees by the roots, *&c.* and, by its ſpiral motion, raiſes the fragments ſo high, till the preſſure of the ſurrounding and approaching currents diminiſhing, can no longer confine them to the circle; or their own centrifugal force encreaſing, grows too ſtrong for ſuch preſſure, when they fly off in tangent lines, as ſtones out of a ſling, and fall on all ſides, and at great diſtances.

If it happens at ſea, the water under and between *aaaa* and *bbbb* will be violently agitated and driven about, and parts of it raiſed with the ſpiral current, and thrown about, ſo as to form a buſh like appearance.

This circle is of various diameters, ſometimes very large.

If the vacuum paſſes over water, the water may riſe in it in a body, or column, to near the height of thirty-two feet.

If

If it paſſes over houſes, it may burſt their windows or walls outwards, pluck off the roofs, and pluck up the floors, by the ſudden rarefaction of the air contained within ſuch buildings; the outward preſſure of the atmoſphere being ſuddenly taken off: So the ſtopp'd bottle of air burſts under the exhauſted receiver of the air-pump.

FIG. II. is to repreſent the elevation of a water-ſpout, wherein, I ſuppoſe P P P to be the cone, at firſt a vacuum, till W W, the riſing column of water, has filled ſo much of it. S S S S, the ſpiral whirl of air ſurrounding the vacuum, and continued higher in a cloſe column after the vacuum ends in the point P, till it reaches the cool region of the air. B B, the buſh deſcribed by *Stuart*, ſurrounding the foot of the column of water.

Now, I ſuppoſe this whirl of air will, at firſt, be as inviſible as the air itſelf, though reaching, in reality, from the water, to the region of cool air, in which our low ſummer thunder-clouds commonly float; but preſently it will become viſible at its extremities. *At its lower end*, by the agitation of the water, under the whirling part of the circle, between P and S, forming *Stuart*'s buſh, and by the ſwelling and riſing of the water, in the beginning vacuum, which is, at firſt, a ſmall, low, broad cone, whoſe top gradually riſes and ſharpens, as the force of the whirl encreaſes. *At its upper end* it becomes viſible, by the warm air brought up to the cooler region, where its moiſture begins to be condenſed into thick vapour, by the cold, and is ſeen firſt at A, the higheſt part, which being

being now cooled, condenses what rises next at B, which condenses that at C, and that condenses what is rising at D, the cold operating by the contact of the vapours faster in a right line downwards, than the vapours themselves can climb in a spiral line upwards; they climb, however, and as by continual addition they grow denser, and, consequently, their centrifugal force greater, and being risen above the concentrating currents that compose the whirl, they fly off, spread, and form a cloud.

It seems easy to conceive, how, by this successive condensation from above, the spout appears to drop or descend from the cloud, though the materials of which it is composed are all the while ascending.

The condensation of the moisture contained in so great a quantity of warm air as may be supposed to rise in a short time in this prodigiously rapid whirl, is, perhaps, sufficient to form a great extent of cloud, though the spout should be over land, as those at *Hatfield*; and if the land happens not to be very dusty, perhaps the lower part of the spout will scarce become visible at all; though the upper, or what is commonly called, the descending part, be very distinctly seen.

The same may happen at sea, in case the whirl is not violent enough to make a high vacuum, and raise the column, *&c.* In such case, the upper part A B C D only will be visible, and the bush, perhaps, below.

But if the whirl be strong, and there be much dust on the land, and the column W W be raised from the water, then the lower part becomes viſible, and ſometimes even united to the upper part. For the duſt may be carried up in the ſpiral whirl, till it reach the region where the vapour is condenſed, and riſe with that even to the clouds: And the friction of the whirling air, on the ſides of the column W W, may detach great quantities of its water, break it into drops, and carry them up in the ſpiral whirl mixed with the air; the heavier drops may, indeed, fly off, and fall, in a ſhower, round the ſpout; but much of it will be broken into vapour, yet viſible; and thus, in both caſes, by duſt at land, and, by water at ſea, the whole tube may be darkened and rendered viſible.

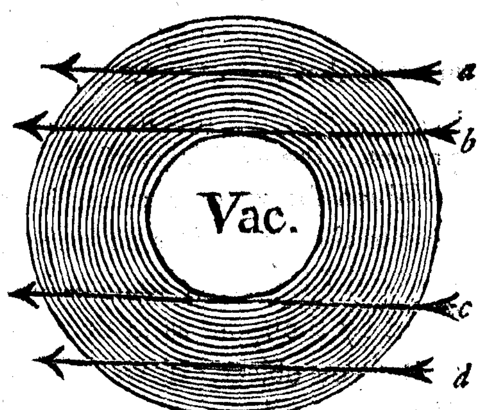
As the whirl weakens, the tube may (in appearance) ſeparate in the middle; the column of water ſubſiding, and the ſuperior condenſed part drawing up to the cloud. Yet ſtill the tube, or whirl of air, may remain entire, the middle only becoming inviſible, as not containing viſible matter.

Dr *Stuart* ſays, ‘ It was obſervable of all the ſpouts ‘ he ſaw, but more perceptible of the great one; that, ‘ towards the end, it began to appear like a hollow canal, ‘ only black in the borders, but white in the middle; ‘ and though at firſt it was altogether black and opaque, ‘ yet, now, one could very diſtinctly perceive the ſea-‘ water to fly up along the middle of this canal, as ſmoak ‘ up a chimney.’

And Dr *Mather*, deſcribing a whirlwind, ſays, ‘ a ‘ thick dark ſmall cloud aroſe, with a pillar of light in ‘ it,

'it, of about eight or ten feet diameter, and paſſed a-
'long the ground in a tract not wider than a ſtreet, hor-
'ribly tearing up trees by the roots, blowing them up in
'the air like feathers, and throwing up ſtones of great
'weight to a conſiderable height in the air, *&c.*'

Theſe accounts, the one of water-ſpouts, the other of a whirlwind, ſeem, in this particular, to agree; what one Gentleman deſcribes as a tube, black in the borders, and white in the middle, the other calls a black cloud, with a pillar of light in it; the latter expreſſion has only a little more of the *marvellous*, but the thing is the ſame; and it ſeems not very difficult to underſtand. When Dr *Stuart*'s ſpouts were full charged, that is, when the whirling pipe of air was filled between *a a a a* and *b b b b*, Fig. I. with quantities of drops, and vapour torn off from the column W W, Fig. II. the whole was rendered ſo dark, as that it could not be ſeen through, nor the ſpiral aſcending motion diſcovered; but when the quantity aſcending leſſened, the pipe became more tranſparent, and the aſcending motion viſible. For, by inſpection of this figure in the margin, repreſenting a ſection of our ſpout, with the vacuum in the middle, it is plain that if we look at ſuch a hollow pipe in



 the

the direction of the arrows, and ſuppoſe opaque particles to be equally mix'd in the ſpace between the two circular lines, both the part between the arrows *a* and *b*, and that between the arrows *c* and *d*, will appear much darker than that between *b* and *c*, as there muſt be many more of thoſe opaque particles in the line of viſion acroſs the ſides, than acroſs the middle. It is thus that a hair in a microſcope evidently appears to be a pipe, the ſides ſhewing darker than the middle. Dr *Mather*'s whirl was probably filled with duſt, the ſides were very dark, but the vacuum within rendering the middle more tranſparent, he calls it a pillar of light.

It was in this more tranſparent part, between *b* and *c*, that *Stuart* could ſee the ſpiral motion of the vapours, whoſe lines on the neareſt and fartheſt ſide of the tranſparent part croſſing each other, repreſented ſmoak aſcending in a chimney; for the quantity being ſtill too great in the line of ſight through the ſides of the tube, the motion could not be diſcovered there, and ſo they repreſented the ſolid ſides of the chimney.

When the vapours reach in the pipe from the clouds near to the earth, it is no wonder now to thoſe who underſtand Electricity, that flaſhes of lightning ſhould deſcend by the ſpout as in that at *Rome*.

But you object, If water may be thus carried into the clouds, why have we no ſalt rains? The objection is ſtrong and reaſonable, and I know not whether I can anſwer it to your ſatisfaction. I never heard but of one ſalt

rain,

rain, and that was where a spout passed pretty near a ship, so I suppose it to be only the drops thrown off from the spout, by the centrifugal force (as the birds were at *Hatfield*) when they had been carried so high as to be above, or to be too strongly centrifugal for, the pressure of the concurring winds surrounding it: And, indeed, I believe there can be no other kind of salt rain; for it has pleased the goodness of God so to order it, that the particles of air will not attract the particles of salt, though they strongly attract water.

Hence, though all metals, even gold, may be united with air, and rendered volatile, salt remains fixt in the fire, and no heat can force it up to any considerable height, or oblige the air to hold it. Hence, when salt rises, as it will a little way, into air with water, there is instantly a separation made; the particles of water adhere to the air, and the particles of salt fall down again, as if repelled and forced off from the water by some power in the air; or, as some metals dissolved in a proper menstruum, will quit the solvent when other matter approaches, and adhere to that, so the water quits the salt, and embraces the air; but air will not embrace the salt, and quit the water, otherwise our rains would indeed be salt, and every tree and plant on the face of the earth be destroyed, with all the animals that depend on them for subsistence. —— HE who hath proportioned and given proper qualities to all things, was not unmindful of this. Let us adore HIM with praise and thanksgiving!

By

By ſome accounts of ſeamen, it ſeems the column of water W W, ſometimes falls ſuddenly; and if it be, as ſome ſay, fifteen or twenty yards diameter, it muſt fall with great force, and they may well fear for their ſhips. By one account, in the *Tranſactions*, of a ſpout that fell at *Colne* in *Lancaſhire*, one would think the column is ſometimes lifted off from the water, and carried over land, and there let fall in a body; but this, I ſuppoſe, happens rarely.

Stuart deſcribes his ſpouts as appearing no bigger than a maſt, and ſometimes leſs; but they were ſeen at a league and a half diſtance.

I think I formerly read in *Dampier*, or ſome other voyager, that a ſpout, in its progreſſive motion, went over a ſhip becalm'd, on the coaſt of *Guinea*, and firſt threw her down on one ſide, carrying away her fore-maſt, then ſuddenly whipp'd her up, and threw her down on the other ſide, carrying away her mizen-maſt, and the whole was over in an inſtant. I ſuppoſe the firſt miſchief was done by the fore-ſide of the whirl, the latter by the hinder ſide, their motion being contrary.

I ſuppoſe a whirlwind, or ſpout, may be ſtationary, when the concurring winds are equal; but if unequal, the whirl acquires a progreſſive motion, in the direction of the ſtrongeſt preſſure.

When the wind that gives the progreſſive motion, becomes ſtronger below than above, or above than below, the ſpout will be bent, and the cauſe ceaſing, ſtraiten again.

Your

Your Queries, towards the end of your paper, appear judicious, and worth considering. At present I am not furnished with facts sufficient to make any pertinent answer to them; and this paper has already a sufficient quantity of conjecture.

Your manner of accommodating the accounts to your hypothesis of descending spouts, is, I own, ingenious; and perhaps that hypothesis may be true. I will consider it farther, but, as yet, I am not satisfied with it, though hereafter I may be.

Here you have my method of accounting for the principal phænomena, which I submit to your candid examination.

And as I now seem to have almost written a book, instead of a letter, you will think it high time I should conclude; which I beg leave to do, with assuring you that

I am, Sir, &c.

B. F.

LETTER XXI.

From Doctor *M———r*,

TO

BENJAMIN FRANKLIN, Esq; at *Philadelphia*.

SIR, *New-Brunswick, November* 11, 1752.

Read at the Royal Society, *June* 24, 1756.

I AM favoured with your letter of the 2d instant, and shall, with pleasure, comply with your request, in describing (as well as my memory serves me) the water-spout I saw at *Antigua*; and shall think this, or any other service I can do, well repaid, if it contributes to your satisfaction in so curious a disquisition.

I had often seen water-spouts at a distance, and heard many strange stories of them, but never knew any thing satisfactory of their nature or cause, until that which I saw at *Antigua*; which convinced me that a water-spout is a whirlwind, which becomes visible in all its dimensions by the water it carries up with it.

There appeared, not far from the mouth of the harbour of St *John*'s, two or three water-spouts, one of which took

took its courſe up the harbour. Its progreſſive motion was ſlow and unequal, not in a ſtrait line, but, as it were, by jerks or ſtarts. When juſt by the wharff I ſtood about 100 yards from it. There appeared in the water a circle of about twenty yards diameter, which, to me, had a dreadful, though pleaſing appearance. The water in this circle was violently agitated, being whiſked about, and carried up into the air with great rapidity and noiſe, and reflected a luſtre, as if the ſun ſhined bright on that ſpot, which was more conſpicuous, as there appeared a dark circle around it. When it made the ſhore, it carried up with the ſame violence ſhingles, ſtaves *, large pieces of the roofs of houſes, *&c.* and one ſmall wooden houſe it lifted entire from the foundation on which it ſtood, and carried it to the diſtance of fourteen feet, where it ſettled without breaking or overſetting; and, what is remarkable, though the whirlwind moved from Weſt to Eaſt, the houſe moved from Eaſt to Weſt. Two or three Negroes and a white woman, were killed by the fall of timber, which it carried up into the air, and dropt again. After paſſing through the town, I believe it was ſoon diſſipated; for, except tearing a large limb from a tree, and part of the cover of a ſugar-work near the town, I do not remember any farther damage done by it. I conclude, wiſhing you ſucceſs in your enquiry, and am, *&c.* W. M.

* I ſuppoſe ſhingles, ſtaves, timber, and other lumber, might be lying in quantities on the wharff, for ſale, as brought from the Northern colonies. B. F.

LETTER XXII.

From Doctor ——, of *Boston*,

TO

BENJAMIN FRANKLIN, *Esq*; of *Philadelphia*.

SIR, *Boston, May* 14, 1753.

Read at the Royal Society *July* 8, 1756.

I Received your letter of *April* last, and thank you for it. Several things in it make me at a loss which side the truth lies on, and determine me to wait for farther evidence.

As to shooting stars, as they are called, I know very little, and hardly know what to say. I imagine them to be passes of electric fire from place to place in the atmosphere, perhaps occasioned by accidental pressures of a non-electric circumambient fluid, and so by propulsion, or allicited by the circumstance of a distant quantity *minus* electrified, which it shoots to supply, and becomes apparent by its contracted passage through a non-electric medium. Electric fire in our globe is always in action, sometimes ascending, descending, or passing from region to region

gion. I suppose it avoids too dry air, and therefore we never see these shoots ascend. It always has freedom enough to pass down unobserved, but, I imagine, not always so, to pass to distant climes and meridians less stored with it.

The shoots are sometimes all one way, which, in the last case, they should be.

Possibly there may be collections of particles in our atmosphere, which gradually form, by attraction, either similar ones *per se*, or dissimilar particles, by the intervention of others. But then, whether they shoot or explode of themselves, or by the approach of some suitable foreign collection, accidentally brought near by the usual commotions and interchanges of our atmosphere, especially when the higher and lower regions intermix, before change of winds and weather, I leave.

I believe I have now said enough of what I know nothing about. If it should serve for your amusement, or any way oblige you, it is all I aim at, and shall, at your desire, be always ready to say what I think, as I am sure of your candour.

I am, &c.

A ſubſequent PAPER *from the ſame.*

Read at the Royal Society *July* 8, 1756.

SPOUTS have been generally believed aſcents of water from below, to the region of the clouds, and whirlwinds the means of conveyance. The world has been very well ſatisfied with theſe opinions, and prejudiced with reſpect to any obſervations about them. Men of learning and capacity have had many opportunities in paſſing thoſe regions where theſe phænomena were moſt frequent, but ſeem induſtriouſly to have declined any notice of them, unleſs to eſcape danger, as a matter of mere impertinence in a caſe ſo clear and certain as their nature and manner of operation are taken to be. Hence it has been very difficult to get any tolerable accounts of them. None but thoſe they fell near can inform us any thing to be depended on; three or four ſuch inſtances follow, where the veſſels were ſo near, that their crews could not avoid knowing ſomething remarkable with reſpect to the matters in queſtion.

Capt. *John Wakefield*, junior, paſſing the *Streights* of *Gibraltar*, had one fall by the ſide of his ſhip; it came down of a ſudden, as they think, and all agree the deſcent was certain.

Capt.

Capt. *Langſtaff*, on a voyage to the *Weſt-Indies*, had one come acroſs the ſtern of his veſſel, and paſſed away from him. The water came down in ſuch quantity that the preſent Capt. *Melling*, who was then a common ſailor at helm, ſays it almoſt drowned him, running into his mouth, noſe, ears, *&c.* and adds, that it taſted perfectly freſh.

One paſſed by the ſide of Capt. *Howland*'s ſhip, ſo near that it appeared pretty plain that the water deſcended from firſt to laſt.

Mr *Robert Spring* was ſo near one in the *Streights* of *Mallacca*, that he could perceive it to be a ſmall very thick rain.

All theſe aſſure me, that there was no wind drawing towards them, nor have I found any others that have obſerved ſuch a wind.

It ſeems plain, by theſe few inſtances, that whirlwinds do not always attend ſpouts; and that the water really deſcends in ſome of them. But the following conſideration, in confirmation of this opinion, may, perhaps, render it probable that all ſpouts are deſcents.

It ſeems unlikely that there ſhould be two ſorts of ſpouts, one aſcending and the other deſcending.

It has not yet been proved that any one ſpout ever aſcended. A ſpecious appearance is all that can be produced in favour of this; and thoſe who have been moſt poſitive about it, were at more than a league's diſtance when they obſerved, as *Stuart* and others, if I am not

miſtaken

miſtaken. However, I believe it impoſſible to be certain whether water aſcends or deſcends at half the diſtance.

It may not be amiſs to conſider the places where they happen moſt. Theſe are ſuch as are liable to calms from departing winds on both ſides, as on the borders of the Æquinoctial trade, calms on the coaſt of *Guinea*, in the *Streights* of *Malacca*, *&c.* places where the under region of the atmoſphere is drawn off horizontally. I think they don't come where the calms are without departing winds; and I take the reaſon to be, that ſuch places, and places where winds blow towards one another, are liable to whirlwinds, or other aſcents of the lower region, which I ſuppoſe contrary to ſpouts. But the former are liable to deſcents, which I take to be neceſſary to their production. Agreable to this, it ſeems reaſonable to believe, that any *Mediterranean* ſea ſhould be more ſubject to ſpouts than others. The ſea uſually ſo called is ſo. The *Streights* of *Malacca* is. Some large gulphs may probably be ſo, in ſuitable latitudes; ſo the *Red Sea*, *&c.* and all for this reaſon, that the heated lands on each ſide, draw off the under region of the air, and make the upper deſcend, whence ſudden and wonderful condenſations may take place, and make theſe deſcents.

It ſeems to me, that the manner of their appearance and procedure, favour the notion of a deſcent.

More or leſs of a cloud, as I am informed, always appears over the place firſt; then a ſpattering on the ſurface of the water below; and when this is advanced to a conſiderable

ſiderable degree, the ſpout emerges from the cloud, and deſcends, and that, if the cauſes are ſufficient, down to the places of ſpattering, with a roaring in proportion to the quantity of the diſcharge; then it abates, or ſtops, ſometimes more gradually, ſometimes more ſuddenly.

I muſt obſerve a few things on theſe particulars, to ſhew how I think they agree with my hypotheſis.

The preceding cloud over the place ſhews condenſation, and, conſequently, tendency downwards, which therefore muſt naturally prevent any aſcent. Beſides that, ſo far as I can learn, a whirlwind never comes under a cloud, but in a clear ſky.

The ſpattering may be eaſily conceived to be cauſed by a ſtream of drops, falling with great force on the place, imagining the ſpout to begin ſo, when a ſudden and great condenſation happens in a contracted ſpace, as the Ox Eye on the coaſt of *Guinea*.

The ſpout appearing to deſcend from the cloud ſeems to be, by the ſtream of nearly contiguous drops bringing the air into conſent, ſo as to carry down a quantity of the vapour of the cloud; and the pointed appearance it makes may be from the deſcending courſe being ſwifteſt in the middle, or center of the ſpout. This naturally drawing the outer parts inward, and the center to a point; and that will appear foremoſt that moves ſwifteſt. The phænomenon of retiring and advancing, I think may be accounted for, by ſuppoſing the progreſſive motion to exceed or not equal the conſumption of the vapour by con-

den-

densation. Or more plainly thus: The descending vapour which forms the apparent spout, if it be slow in its progress downwards, is condensed as fast as it advances, and so appears at a stand; when it is condensed faster than it advances, it appears to retire; and *vice versa*.

Its duration and manner of ending, are as the causes, and may vary by several accidents.

The cloud itself may be so circumstanced as to stop it; as when, extending wide, it weighs down at a distance round about, while a small circle at the spout being exonerated by the discharge, ascends and shuts up the passage. A new determination of wind may, perhaps, stop it too. Places liable to these appearances are very liable to frequent and sudden alterations of it.

Such accidents as a clap of thunder, firing cannon, &c. may stop them, and the reason may be, that any shock of this kind may occasion the particles that are near cohering, immediately to do so; and then the whole, thus condensed, falls at once (which is what I suppose is vulgarly called the breaking of the spout) and in the interval, between this period and that of the next set of particles being ready to unite, the spout shuts up. So that if this reasoning is just, these phænomena agree with my hypothesis.

The usual temper of the air, at the time of their appearance, if I have a right information, is for me too; it being then pretty cool for the season and climate; and this is worth remark, because cool air is weighty, and will

not

not aſcend; beſides, when the air grows cool, it ſhews that the upper region deſcends, and conveys this temper down; and when the tempers are equal, no whirlwind can take place. But ſpouts have been known, when the lower region has been really cold. *Gordon's* ſpout in the *Downs* is an inſtance of this—(*Vide Philoſophical Tranſactions*—) where the upper region was probably not at all cooler, if ſo cold as the lower: It was a cold day in the month of *March*; hail followed, but not ſnow; and it is obſervable, that not ſo much as hail follows or accompanies them in moderate ſeaſons or climes, when and where they are moſt frequent. However, it is not improbable, that juſt about the place of deſcent may be cooler than the neighbouring parts, and ſo favour the wonderful celerity of condenſation. But, after all, ſhould we allow the under region to be ever ſo much the hotteſt, and a whirlwind to take place in it: Suppoſe then the ſea-water to aſcend, it would certainly cool the ſpout, and then, query, whether it would not very much, if not wholly, obſtruct its progreſs.

It commonly rains when ſpouts diſappear, if it did not before, which it frequently does not, by the beſt accounts I have had; but the cloud encreaſes much faſter after they diſappear, and it ſoon rains. The firſt ſhews the ſpout to be a contracted rain, inſtead of the diffuſed one that follows; and the latter that the cloud was not formed by aſcending water, for then it would have ceaſed growing when the ſpout vaniſhed.

However, it ſeems that ſpouts have ſometimes appeared after it began to rain; but this is one way a proof of my hypotheſis, *viz.* as whirlwinds don't come under a cloud.

I forgot to mention, that the increaſe of cloud, while the ſpout ſubſiſts, is no argument of an aſcent of water, by the ſpout. Since thunder-clouds ſometimes encreaſe greatly while it rains very hard.

Divers effects of ſpouts ſeem not ſo well accounted for any other way as by deſcent.

The buſh round the feet of them ſeems to be a great ſpray of water made by the violence of deſcent, like that in great falls of water from high precipices.

The great roar, like ſome vaſt inland falls, is ſo different from the roar of whirlwinds, by all accounts, as to be no ways compatible.

The throwing things from it with great force, inſtead of carrying them up into the air, is another difference.

There ſeems ſome probability that the ſailors traditionary belief that ſpouts may break in their decks, and ſo deſtroy veſſels, might originate from ſome facts of that ſort in former times. This danger is apparent on my hypotheſis, but it ſeems not ſo on the other: And my reaſon for it is, that the whole column of a ſpout from the ſea to the clouds, cannot, in a natural way, even upon the largeſt ſuppoſition, ſupport more than about three feet water, and from truly ſuppoſeable cauſes, not above one foot, as may appear more plainly by and by. Sup-

poſing

poſing now the largeſt of theſe quantities to riſe, it muſt be diſſeminated into drops, from the ſurface of the ſea to the region of the clouds, or higher; for this reaſon it is quite unlikely to be collected into maſſes, or a body, upon its falling; but would deſcend in progreſſion according to the ſeveral degrees of altitude the different portions had arrived at when it received this new determination.

Now that there cannot more riſe upon the common hypotheſis than I have mentioned, may appear probable, if we attend to the only efficient cauſe in ſuppoſed aſcending ſpouts, *viz.* whirlwinds.

We know that the rarefaction of the lower, and the condenſation of the upper region of air, are the only natural cauſes of whirlwinds. Let us then ſuppoſe the former as hot as their greateſt ſummer heat in *England*, and the latter as cold as the extent of their winter. Theſe extremes have been found there to alter the weight of the air one-tenth, which is equal to a little more than three feet water. Were this caſe poſſible, and a whirlwind take place in it, it might act with a force equal to the mentioned difference. But as this is the whole ſtrength, ſo much water could not riſe; therefore to allow it due motion upwards, we muſt abate, at leaſt, one-fourth part, perhaps more, to give it ſuch a ſwift aſcenſion as ſome think uſual. But here ſeveral difficulties occur, at leaſt they are ſo to me. As, whether this quantity would render the ſpout opaque? Since it is plain that in drops it could not do ſo. How, or by what means it may be re-

 duced

duced ſmall enough? Or, if the water be not reduced into vapour, what will ſuſpend it in the region of the clouds when exonerated there? And, if vapourized while aſcending, how it can be dangerous by what they call the breaking? For it is difficult to conceive how a condenſative power ſhould inſtantaneouſly take place of a rariſying and diſſeminating one.

The ſudden fall of the ſpout, or, rather, the ſudden ceaſing of it, I accounted for, in my way, before. But it ſeems neceſſary to mention ſomething I then forgot. Should it be ſaid to do ſo, (*i. e.*) to fall, becauſe all the lower rarified air is aſcended, whence the whirlwind muſt ceaſe, and its burden drop; I cannot agree to this, unleſs the air be obſerved on a ſudden to have grown much colder, which I can't learn has been the caſe. Or ſhould it be ſuppoſed that the ſpout was, on a ſudden, obſtructed at the top, and this the cauſe of the fall, however plauſible this might appear, yet no more water would fall than what was at the ſame time contained in the column, which is often, by many and ſatisfactory accounts to me, again far from being the caſe.

We are, I think, ſufficiently aſſured, that not only tons, but ſcores or hundreds of tons deſcend in one ſpout. Scores of tons more than can be contained in the trunk of it, ſhould we ſuppoſe water to aſcend.

But, after all, it don't appear that the above-mentioned different degrees of heat and cold concur in any region where ſpouts uſually happen, nor, indeed, in any other.

Ob-

Observations on the METEOROLOGICAL PAPER; *by a Gentleman in* Connecticut.

Read at the Royal Society, *Nov.* 4, 1756.

"AIR and water mutually attract each other, (saith Mr *F.*) hence water will dissolve in air, as salt in water." I think that he hath demonstrated, that the supporting of salt in water is not owing to its superficies being increased, because "the specific gravity of salt is not altered by dividing of it, any more than that of lead, sixteen bullets of which, of an ounce each, weigh as much in water as one of a pound." But yet, when this came to be applied to the supporting of water in air, I found an objection rising in my mind.

In the first place, I have always been loth to seek for any new hypothesis, or particular law of nature, to account for any thing that may be accounted for from the known, general, and universal law of nature; it being an argument of the infinite wisdom of the Author of the World, to effect so many things by one general law. Now I had thought that the rising and support of water in air, might be accounted for from the general law of gravitation, by only supposing the spaces occupied by the same quantity of water increased.

And

And, wirh reſpect to the lead, I queried thus in my own mind; whether if the ſuperficies of a bullet of lead ſhould be increaſed four or five fold by an internal vacuity, it would weigh the ſame in water as before. I mean, if a pound of lead ſhould be formed into a hollow globe, empty within, whoſe ſuperficies ſhould be four or five times as big as that of the ſame lead when a ſolid lump, it would weigh as much in water as before. I ſuppoſed it would not. If this concavity was filled with water, perhaps it might; if with air, it would weigh at leaſt as much leſs, as this difference between the weight of that included air, and that of water.

Now although this would do nothing to account for the diſſolution of ſalt in water, the ſmalleſt lumps of ſalt being no more hollow ſpheres, or any thing of the like nature than the greateſt; yet, perhaps, it might account for water's riſing and being ſupported in air. For you know that ſuch hollow globules, or bubbles, abound upon the ſurface of the water, which even by the breath of our mouths, we can cauſe to quit the water, and riſe in the air.

Theſe bubbles I uſed to ſuppoſe to be coats of water, containing within them air rarified and expanded with fire, and that, therefore, the more friction and daſhing there is upon the ſurface of the waters, and the more heat and fire, the more they abound.

And I uſed to think, that although water be ſpecifically heavier than air, yet ſuch a bubble, filled only with fire and

and very rarified air, may be lighter than a quantity of common air, of the ſame cubical dimenſions, and, therefore, aſcend; for the rarified air incloſed, may more fall ſhort of the ſame bulk of common air, in weight, than the watery coat exceeds a like bulk of common air in gravity.

This was the objection in my mind, though, I muſt confeſs, I know not how to account for the watery coat's encompaſſing the air, as above-mentioned, without allowing the attraction between air and water, which the Gentleman ſuppoſes; ſo that I don't know but that this objection examined by that ſagacious Genius, will be an additional confirmation of the hypotheſis.

The Gentleman obſerves, "That a certain quantity of moiſture ſhould be every moment diſcharged and taken away from the lungs;" and hence accounts for the ſuffocating nature of ſnuffs of candles, as impregnating the air with greaſe, between which and water there is a natural repellency; and of air that hath been frequently breathed in, which is overloaded with water, and, for that reaſon, can take no more air. Perhaps the ſame obſervation will account for the ſuffocating nature of damps in wells.

But then if the air can ſupport and take off but ſuch a proportion of water, and it is neceſſary that water be ſo taken off from the lungs, I queried with myſelf how it is we can breathe in an air full of vapours, ſo full as *that* they continually precipitated. Don't we ſee the air o-

ver-

verloaded, and caſting forth water plentifully when there is no ſuffocation?

The Gentleman again obſerves, "That the air under "the Equator, and between the Tropics, being conſtantly "heated and rarified by the ſun, riſes; its place is ſup-"plied by air from Northern and Southern latitudes, "which coming from parts where the air and earth had "leſs motion, and not ſuddenly acquiring the quicker mo-"tion of the equatorial earth, appears an Eaſt wind blow-"ing Weſtward; the earth moving from Weſt to Eaſt, "and ſlipping under the air."

In reading this, two objections occurred to my mind:

Firſt, That it is ſaid, the trade-wind doth not blow in the forenoon, but only in the afternoon.

Secondly, That either the motion of the Northern and Southern air towards the Equator is ſo ſlow, as to acquire almoſt the ſame motion as the equatorial air when it arrives there, ſo that there will be no ſenſible difference; or elſe the motion of the Northern and Southern air towards the Equator, is quicker, and muſt be ſenſible; and then the trade-wind muſt appear either as a South-Eaſt or North-Eaſt wind: South of the Equator, a South-Eaſt wind; North of the Equator a North-Eaſt. For the apparent wind muſt be compounded of this motion from North to South, or *vice verſa*; and of the difference between its motion from Weſt to Eaſt, and that of the equatorial air.

Ob-

Observations in Answer to the foregoing; by B. F.

Read at the Royal-Society, *Nov.* 4, 1756.

1st. THE supposing a mutual attraction between the particles of water and air, is not introducing a new law of nature; such attractions taking place in many other known instances.

2dly. Water is specifically 850 times heavier than air. To render a bubble of water, then, specifically lighter than air, it seems to me that it must take up more than 850 times the space it did before it formed the bubble; and within the bubble should be either a vacuum or air rarified more than 850 times. If a vacuum, would not the bubble be immediately crush'd by the weight of the atmosphere? And no heat, we know of, will rarify air any thing near so much; much less the common heat of the sun, or that of friction by the dashing on the surface of the water. Besides, water agitated ever so violently, produces no heat, as has been found by accurate experiments.

3dly. A hollow sphere of lead has a firmness and consistency in it, that a hollow sphere or bubble of fluid unfrozen water cannot be supposed to have. The lead may support the pressure of the water it is immerged in, but the bubble could not ſupport the preſſure of the air, if empty within.

4thly. Was ever a viſible bubble ſeen to riſe in air? I have made many, when a boy, with ſoap-ſuds and a tobacco-pipe; but they all deſcended when looſe from the pipe, though ſlowly, the air impeding their motion. They may, indeed, be forced up by a wind from below, but do not *riſe of themſelves*, though filled with warm breath.

5thly. The objection relating to our breathing moiſt air, ſeems weighty, and muſt be farther conſidered. The air that has been breathed, has, doubtleſs, acquired an addition of the perſpirable matter which nature intends to free the body from, and which would be pernicious if retained and returned into the blood; ſuch air then may become unfit for reſpiration, as well for that reaſon, as on account of its moiſture. Yet I ſhould be glad to learn, by ſome accurate experiment, whether a draft of air, two or three times inſpired, and expired, perhaps in a bladder, has, or has not, acquired more moiſture than our common air in the dampeſt weather. As to the precipitation of water in the air we breathe, perhaps it is not always a mark of that air's being overloaded. In the region of the clouds, indeed, the air muſt be overloaded if it lets fall its water in drops, which we call rain; but thoſe drops may fall through a dryer air near the earth; and accordingly we find that the hygroſcope ſometimes ſhews a leſs degree of moiſture, during a ſhower,

than

than at other times when it does not rain at all. The dewy dampness that settles on the insides of our walls and wainscots, seems more certainly to denote an air overloaded with moisture; and yet this is no sure sign: For, after a long continued cold season, if the air grows suddenly warm, the walls, *&c.* continuing longer their coldness, will, for some time, condense the moisture of such air, till they grow equally warm, and then they condense no more, though the air is not become dryer. And, on the other hand, after a warm spell, if the air grows cold, though moister than before, the dew is not so apt to gather on the walls. A tankard of cold water will, in a hot and dry summer's day, collect a dew on its outside; a tankard of hot water will collect none in the moistest weather.

6thly. It is, I think, a mistake that the trade-winds blow only in the afternoon. They blow all day and all night, and all the year round, except in some particular places. The southerly sea-breezes on your coasts, indeed, blow chiefly in the afternoon. In the very long run, from the West side of *America*, to *Guam*, among the *Phillippine Islands*, ships seldom have occasion to hand their sails, so equal and steady is the gale, and yet they make it in about 60 days, which could not be, if the wind blew only in the afternoon.

7thly. That really is, which the Gentleman justly supposes ought to be on my hypothesis. In sailing

Southward, when you firſt enter the trade-wind, you find it North-Eaſt, or thereabouts, and it gradually grows more Eaſt as you approach the line. The ſame obſervation is made of its changing from South-Eaſt to Eaſt gradually, as you come from the Southern latitudes to the equator.

Obſervations on the METEOROLOGICAL PAPER; *ſent by a Gentleman in* New-York, *to* B. F.

Read at the Royal-Society, *Nov.* 4, 1756.

THAT power by which the air expands itſelf, you attribute to a mutual repelling power in the particles which compoſe the air, by which they are ſeparated from each other with ſome degree of force: Now this force, on this ſuppoſition, muſt not only act when the particles are in mutual contact, but likewiſe when they are at ſome diſtance from each other. How can two bodies, whether they be great or ſmall, act at any diſtance, whether that diſtance be ſmall or great, without ſomething intermediate on which they act? For if any body act on another, at any diſtance from it, however ſmall that diſtance be, without ſome medium to continue the action, it muſt act where it is not, which to me ſeems abſurd.

It

It ſeems to me, for the ſame reaſon, equally abſurd to give a mutual attractive power between any other particles ſuppoſed to be at a diſtance from each other, without any thing intermediate to continue their mutual action. I can neither attract nor repel any thing at a diſtance, without ſomething between my hand and that thing, like a ſtring, or a ſtick; nor can I conceive any mutual action without ſome middle thing, when the action is continued to ſome diſtance.

The encreaſe of the ſurface of any body leſſens its weight, both in air, and water, or any other fluid, as appears by the ſlow deſcent of leaf-gold in the air.

The obſervation of the different denſity of the upper and lower air, from heat and cold, is good, and I do not remember it is taken notice of by others; the conſequences alſo are well drawn; but as to winds, they ſeem principally to ariſe from ſome other cauſe. Winds generally blow from ſome large tracts of land, and from mountains. Where I live, on the North ſide of the mountains, we frequently have a ſtrong Southerly wind, when they have as ſtrong a Northerly wind, or calm, on the other ſide of theſe mountains. The continual paſſing of veſſels on *Hudſon*'s *River*, through theſe mountains, give frequent opportunities of obſerving this.

In the ſpring of the year the ſea-wind (by a piercing cold) is always more uneaſy to me, accuſtomed to winds which paſs over a tract of land, than the North-Weſt wind.

You

You have received the common notion of water-ſpouts, which, from my own ocular obſervation, I am perſuaded is a falſe conception. In a voyage to the *Weſt-Indies* I had an opportunity of obſerving many water-ſpouts. One of them paſſed nearer than thirty or forty yards to the veſſel I was in, which I viewed with a good deal of attention; and though it be now forty years ſince I ſaw it, it made ſo ſtrong an impreſſion on me, that I very diſtinctly remember it. Theſe water-ſpouts were in the calm latitudes, that is, between the trade and the variable winds, in the month of *July*. That ſpout which paſſed ſo near us, was an inverted cone, with the *tip* or *apex* towards the ſea, and reached within about eight feet of the ſurface of the ſea, its baſis in a large black cloud. We were entirely becalmed. It paſſed ſlowly by the veſſel. I could plainly obſerve that a violent ſtream of wind iſſued from the ſpout, which made a hollow of about ſix feet diameter in the ſurface of the water, and raiſed the water in a circular uneven ring round the hollow, in the ſame manner that a ſtrong blaſt from a pair of bellows would do when the pipe is placed perpendicular to the ſurface of the water; and we plainly heard the ſame hiſſing noiſe which ſuch a blaſt of wind muſt produce on the water. I am very ſure there was nothing like the ſucking of water from the ſea into the ſpout, unleſs the ſpray which was raiſed in a ring to a ſmall height, could be miſtaken for a raiſing of water. I could plainly diſtinguiſh a diſtance of about eight feet between the ſea and the

the tip of the cone, in which nothing interrupted the ſight, which muſt have been, had the water been raiſed from the ſea.

In the ſame voyage I ſaw ſeveral other ſpouts at a greater diſtance, but none of them whoſe tip of the cone came ſo near the ſurface of the water. In ſome of them the axis of the cone was conſiderably inclined from the perpendicular, but in none of them was there the leaſt appearance of ſucking up of water. Others of them were bent or arched. I believe that a ſtream of wind iſſued from all of them, and it is from this ſtream of wind that veſſels are often overſet, or founder at ſea ſuddenly. I have heard of veſſels being overſet when it was perfectly calm, the inſtant before the ſtream of wind ſtruck them, and immediately after they were overſet; which could not otherwiſe be but by ſuch a ſtream of wind from a cloud.

That wind is generated in clouds will not admit of a diſpute. Now if ſuch wind be generated within the body of the cloud, and iſſue in one particular place, while it finds no paſſage in the other parts of the cloud, I think it may not be difficult to account for all the appearances in water-ſpouts; and from hence the reaſon of breaking thoſe ſpouts, by firing a cannon-ball through them, as thereby a horizontal vent is given to the wind. When the wind is ſpent, which dilated the cloud, or the fermentation ceaſes, which generates the air and wind, the clouds may deſcend in a prodigious fall of water or rain.

A re-

A remarkable inteftine motion, like a violent fermentation, is very obfervable in the cloud from whence the fpout iffues. No falt water, I am perfuaded, was ever obferved to fall from the clouds, which muft certainly have happened if fea-water had been raifed by a fpout.

ANSWER *to the foregoing Obfervations*; *by* B. F.

Read at the Royal Society, *Nov.* 4, 1756.

I Agree with you, that it feems abfurd to fuppofe that a body can act where it is not. I have no idea of bodies at a diftance attracting or repelling one another without the affiftance of fome medium, though I know not what that medium is, or how it operates. When I fpeak of attraction or repulfion, I make ufe of thofe words for want of others more proper, and intend only to exprefs *effects* which I fee, and not *caufes* of which I am ignorant. When I prefs a blown bladder between my knees, I find I cannot bring its fides together, but my knees feel a fpringy matter, pufhing them back to a greater diftance, or repelling them. I conclude that the air it contains is the caufe. And when I operate on the air, and find I cannot by preffure force its particles into contact, but they ftill fpring back againft the preffure, I conceive there muft be fome medium between its particles that prevents their clofing, though I cannot tell what it is.—And if I were ac-

acquainted with that medium, and found its particles to approach and recede from each other, according to the preſſure they ſuffered, I ſhould imagine there muſt be ſome finer medium between them, by which theſe operations were performed.

I allow that increaſe of the ſurface of a body may occaſion it to deſcend ſlower in air, water, or any other fluid; but do not conceive, therefore, that it leſſens its weight. Where the increaſed ſurface is ſo diſpoſed as that in its falling a greater quantity of the fluid it ſinks in muſt be moved out of its way, a greater time is required for ſuch removal. Four ſquare feet of ſheet lead ſinking in water *broadways*, cannot deſcend near ſo faſt as it would *edgeways*, yet its weight in the hydroſtatic ballance would, I imagine, be the ſame, whether ſuſpended by the middle or by the corner.

I make no doubt but that ridges of high mountains do often interrupt, ſtop, reverberate, or turn the winds that blow againſt them, according to the different degrees of ſtrength of the winds, and the angles of incidence. I ſuppoſe, too, that the cold upper parts of mountains may condenſe the warmer air that comes near them, and ſo by making it ſpecifically heavier, cauſe it to deſcend on one or both ſides of the ridge into the warmer valleys, which will ſeem a wind blowing from the mountain.

Damp winds, though not colder by the thermometer, give a more uneaſy ſenſation of cold than dry ones; becauſe (to ſpeak like an Electrician) they *conduct* better;

that is, are better fitted to convey away the heat from our bodies. The body cannot feel *without* itſelf; our ſenſation of cold is not in the air *without* the body, but in thoſe parts of the body which have been deprived of their heat by the air. My deſk, and its lock, are, I ſuppoſe, of the ſame temperament when they have been long expoſed to the ſame air; but now if I lay my hand on the wood, it does not ſeem ſo cold to me as the lock; becauſe (as I imagine) wood is not ſo good a conductor, to receive and convey away the heat from my ſkin, and the adjacent fleſh, as metal is. Take a piece of wood, of the ſize and ſhape of a dollar, between the thumb and finger of one hand, and a dollar, in like manner, with the other hand; place the edges of both, at the ſame time, in the flame of a candle; and though the edge of the wooden piece takes flame, and the metal piece does not, yet you will be obliged to drop the latter before the former, it conducting the heat more ſuddenly to your fingers. Thus we can, without pain, handle glaſs and china cups filled with hot liquors, as tea, &c. but not ſilver ones. A ſilver tea-pot muſt have a wooden handle. Perhaps it is for the ſame reaſon that woollen garments keep the body warmer than linnen ones equally thick; woollen keeping the natural heat in, or, in other words, not conducting it out to air.

In regard to water-ſpouts, having, in a long letter to a Gentleman of the ſame ſentiment with you as to their direction, ſaid all that I have to ſay in ſupport of my opinion;

pinion; I need not repeat the arguments therein contained, as I intend to send you a copy of it by some other opportunity, for your perusal. I imagine you will find all the appearances you saw, accounted for by my hypothesis. I thank you for communicating the account of them. At present I would only say, that the opinion of winds being generated in clouds by fermentation, is new to me, and I am unacquainted with the facts on which it is founded. I likewise find it difficult to conceive of winds confined in the body of clouds, which I imagine have little more solidity than the fogs on the earth's surface. The objection from the freshness of rain-water is a strong one, but I think I have answered it in the letter above-mentioned, to which I must beg leave, at present, to refer you.

LETTER XXIII.

FROM

BENJ. FRANKLIN, *Esq*; of *Philadelphia*.

To C. C. Esq; at *New-York*.

SIR, *Philadelphia, April* 23, 1752.

Read at the Royal Society *Nov.* 11, 1756.

IN considering your favour of the 16th past, I recollected my having wrote you answers to some queries concerning the difference between electrics *per se*, and non-electrics, and the effects of air in electrical experiments, which, I apprehend, you may not have received. The date I have forgot.

We have been used to call those bodies electrics *per se*, which would not conduct the electric fluid: We once imagined that only such bodies contained that fluid; afterwards that they had none of it, and only educ'd it from other bodies: But further experiments shewed our mistakes. It is to be found in all matter we know of; and the distinctions of electrics *per se*, and non-electrics, should now be dropt as improper, and that of *conductors* and *non-conductors* assumed in its place, as I mentioned in those answers.

I do

I do not remember any experiment by which it appeared that high rectified ſpirit will not conduct; perhaps you have made ſuch. This I know, that wax, roſin, brimſtone, and even glaſs, commonly reputed electrics *per ſe*, will, when in a fluid ſtate, conduct pretty well. Glaſs will do it when only red hot. So that my former poſition, that only metals and water were conductors, and other bodies more or leſs ſuch, as they partook of metal or moiſture, was too general.

Your conception of the electric fluid, that it is incomparably more ſubtle than air, is undoubtedly juſt. It pervades denſe matter with the greateſt eaſe; but it does not ſeem to mix or incorporate willingly with meer air, as it does with other matter. It will not quit common matter to join with air. Air obſtructs, in ſome degree, its motion. An electric atmoſphere cannot be communicated at ſo great a diſtance, through intervening air, by far, as through a vacuum.—Who knows then, but there may be, as the Antients thought, a region of this fire above our atmoſphere, prevented by our air, and its own too great diſtance for attraction, from joining our earth? Perhaps where the atmoſphere is rareſt, this fluid may be denſeſt, and nearer the earth where the atmoſphere grows denſer, this fluid may be rarer; yet ſome of it be low enough to attach itſelf to our higheſt clouds, and thence they becoming electrified, may be attracted by, and deſcend towards the earth, and diſcharge their watry contents, together with that etherial fire. Perhaps the Auroræ Boreales,

ales are currents of this fluid in its own region, above our atmoſphere, becoming from their motion viſible. There is no end to conjectures. As yet we are but novices in this branch of natural knowledge.

You mention ſeveral differences of ſalts in electrical experiments? Were they all equally dry? Salt is apt to acquire moiſture from a moiſt air, and ſome ſorts more than others. When perfectly dried by lying before a fire, or on a ſtove, none that I have tried will conduct any better than ſo much glaſs.

New flannel, if dry and warm, will draw the electric fluid from non-electrics, as well as that which has been worn.

I wiſh you had the convenience of trying the experiments you ſeem to have ſuch expectations from, upon various kinds of ſpirits, ſalts, earth, *&c.* Frequently, in a variety of experiments, though we miſs what we expected to find, yet ſomething valuable turns out, ſomething ſurpriſing, and inſtructing, though unthought of.

I thank you for communicating the illuſtration of the theorem concerning light. It is very curious. But I muſt own I am much in the *dark* about *light*. I am not ſatisfied with the doctrine that ſuppoſes particles of matter called light, continually driven off from the ſun's ſurface, with a ſwiftneſs ſo prodigious! Muſt not the ſmalleſt particle conceivable, have with ſuch a motion, a force exceeding that of a twenty-four pounder, diſcharged from a cannon? Muſt not the ſun diminiſh exceedingly

by

by ſuch a waſte of matter; and the planets, inſtead of drawing nearer to him, as ſome have feared, recede to greater diſtances through the leſſened attraction. Yet theſe particles, with this amazing motion, will not drive before them, or remove, the leaſt and lighteſt duſt they meet with: And the ſun, for aught we know, continues of his antient dimenſions, and his attendants move in their antient orbits.

May not all the phænomena of light be more conveniently ſolved, by ſuppoſing univerſal ſpace filled with a ſubtle elaſtic fluid, which, when at reſt, is not viſible, but whoſe vibrations affect that fine ſenſe in the eye, as thoſe of air do the groſſer organs of the ear? We do not, in the caſe of ſound, imagine that any ſonorous particles are thrown off from a bell, for inſtance, and fly in ſtrait lines to the ear; why muſt we believe that luminous particles leave the ſun and proceed to the eye? Some diamonds, if rubbed, ſhine in the dark, without loſing any part of their matter. I can make an electrical ſpark as big as the flame of a candle, much brighter, and, therefore, viſible further; yet this is without fuel; and, I am perſuaded, no part of the electric fluid flies off in ſuch caſe, to diſtant places, but all goes directly, and is to be found in the place to which I deſtine it. May not different degrees of the vibration of the above-mentioned univerſal medium, occaſion the appearances of different colours? I think the electric fluid is always the ſame; yet I find that weaker and ſtronger ſparks differ in apparent

rent colour, ſome white, blue, purple, red; the ſtrongeſt, white; weak ones red. Thus different degrees of vibration given to the air, produce the ſeven different ſounds in muſic, analagous to the ſeven colours, yet the medium, air, is the ſame.

If the ſun is not waſted by expence of light, I can eaſily conceive that he ſhall otherwiſe always retain the ſame quantity of matter; though we ſhould ſuppoſe him made of ſulphur conſtantly flaming. The action of fire only *ſeparates* the particles of matter, it does not *annihilate* them. Water, by heat raiſed in vapour, returns to the earth in rain; and if we could collect all the particles of burning matter that go off in ſmoak, perhaps they might, with the aſhes, weigh as much as the body before it was fired: And if we could put them into the ſame poſition with regard to each other, the maſs would be the ſame as before, and might be burnt over again. The chymiſts have analyſed ſulphur, and find it compoſed, in certain proportions, of oil, ſalt, and earth; and having, by the analyſis, diſcovered thoſe proportions, they can, of thoſe ingredients, make ſulphur. So we have only to ſuppoſe, that the parts of the ſun's ſulphur, ſeparated by fire, riſe into his atmoſphere, and there being freed from the immediate action of the fire, they collect into cloudy maſſes, and growing, by degrees, too heavy to be longer ſupported, they deſcend to the ſun, and are burnt over again. Hence the ſpots appearing on his face, which are obſerved to di-

miniſh

miniſh daily in ſize, their conſuming edges being of particular brightneſs.

It is well we are not, as poor *Galileo* was, ſubject to the Inquiſition for *Philoſophical Hereſy*. My whiſpers againſt the orthodox doctrine, in private letters, would be dangerous; but your writing and printing would be highly criminal. As it is, you muſt expect ſome cenſure, but one Heretic will ſurely excuſe another.

I am heartily glad to hear more inſtances of the ſucceſs of the Poke-Weed, in the cure of that horrible evil to the human body, a Cancer. You will deſerve highly of mankind for the communication. But I find in *Boſton* they are at a loſs to know the right plant, ſome aſſerting it is what they call *Mechoachan*, others other things. In one of their late papers it is publickly requeſted that a perfect deſcription may be given of the plant, its places of growth, *&c.* I have miſlaid the paper, or would ſend it to you. I thought you had deſcribed it pretty fully.

I am, Sir, &c.

B. F.

Extracts from DAMPIER's *Voyages, relating to* WATER-SPOUTS.

Read at the Royal Society, *Dec.* 16, 1756.

"A Spout is a ſmall ragged piece, or part of a cloud, hanging down about a yard ſeemingly, from the blackeſt part thereof. Commonly it hangs down ſloping from thence, or ſometimes appearing with a ſmall bending, or elbow, in the middle. I never ſaw any hang perpendicularly down. It is ſmall at the lower end, ſeeming no bigger than one's arm, but ſtill fuller towards the cloud from whence it proceeds.

When the ſurface of the ſea begins to work, you ſhall ſee the water for about one hundred paces in circumference foam and move gently round, till the whirling motion increaſes; and then it flies upward in a pillar, about one hundred paces in compaſs at the bottom, but gradually leſſening upwards, to the ſmallneſs of the ſpout itſelf, through which the riſing ſea-water ſeems to be conveyed into the clouds. This viſibly appears by the clouds increaſing in bulk and blackneſs. Then you ſhall preſently ſee the cloud drive along, though before it ſeemed to be without any motion. The ſpout alſo keeping the ſame courſe with the cloud, and ſtill ſucking up the water as it goes along, and they make a wind as they go. Thus it con-

continues for half an hour, more or leſs, until the ſucking is ſpent, and then breaking off, all the water which was below the ſpout, or pendulous piece of cloud, falls down again into the ſea, making a great noiſe with its falling and claſhing motion in the ſea.

It is very dangerous for a ſhip to be under a ſpout when it breaks; therefore we always endeavour to ſhun it, by keeping at a diſtance, if poſſibly we can. But for want of wind to carry us away, we are often in great fear and danger, for it is uſually calm when ſpouts are at work, except only juſt where they are. Therefore men at ſea, when they ſee a ſpout a coming, and know not how to avoid it, do ſometimes fire ſhot out of their great guns into it, to give it air or vent, that ſo it may break; but I did never hear that it proved to be of any benefit.

And now we are on this ſubject, I think it not amiſs to give you an account of an accident that happened to a ſhip once on the coaſt of *Guinea*, ſome time in or about the year 1674. One Capt. *Records* of *London*, bound for the coaſt of *Guinea*, in a ſhip of three hundred tons, and ſixteen guns, called the *Bleſſing*, when he came into latitude ſeven or eight degrees North, he ſaw ſeveral ſpouts, one of which came directly towards the ſhip, and he having no wind to get out of the way of the ſpout, made ready to receive it by furling the ſails. It came on very ſwift, and broke a little before it reached the ſhip, making a great noiſe, and raiſing the ſea round it, as if a great houſe, or ſome ſuch thing, had been caſt into the ſea.

The fury of the wind ſtill laſted, and took the ſhip on the ſtar-board-bow with ſuch violence, that it ſnapt off the boltſprit and foremaſt both at once, and blew the ſhip all along, ready to overſet it; but the ſhip did preſently right again, and the wind whirling round, took the ſhip a ſecond time with the like fury as before, but on the contrary ſide, and was again like to overſet her the other way: The mizen-maſt felt the fury of this ſecond blaſt, and was ſnapt ſhort off, as the foremaſt and boltſprit had been before. The main-maſt and main-top-maſt received no damage, for the fury of the wind (which was preſently over) did not reach them. Three men were in the fore-top when the foremaſt broke, and one on the boltſprit, and fell with them into the ſea, but all of them were ſaved. I had this relation from Mr *John Canby*, who was then quarter-maſter and ſteward of her; one *Abraham Wiſe* was chief-mate, and *Leonard Jefferies* ſecond-mate.

We are uſually much afraid of them, yet this was the only damage that I ever heard done by them. They ſeem terrible enough, the rather becauſe they come upon you while you lie becalmed, like a log in the ſea, and cannot get out of their Way. But though I have ſeen and been beſet by them often, yet the fright was always the greateſt of the harm."——*Dampier*, Vol. I. page 451.

An

An Account of a Spout *on the Coast of* New-Guinea. *From the same.*

"WE had fair clear weather, and a fine moderate gale from South-East to East by North; but at day-break the clouds began to fly, and it lightened very much in the East North-East. At sun rising the sky looked very red in the East near the horizon; and there were many black clouds both to the South and North of it. About a quarter of an hour after the sun was up, there was a squall to the windward of us, when, on a sudden, one of our men on the fore-castle, called out that he saw something a-stern, but could not tell what. I looked out for it, and immediately saw a spout beginning to work within a quarter of a mile of us, exactly in the wind: We presently put right before it. It came very swiftly, whirling the water up in a pillar, about six or seven yards high. As yet I could not see any pendulous cloud from whence it might come; and was in hopes it would soon lose its force. In four or five minutes time it came within a cable's length of us, and passed away to leeward; and then I saw a long pale stream coming down to the whirling water. This stream was about the bigness of a rainbow. The upper end seemed vastly high, not descendſcending from any dark cloud, and, therefore, the more ſtrange to me, I never having ſeen the like before. It paſt about a mile to the leeward of us, and then broke. This was but a ſmall ſpout, and not ſtrong nor *laſting; yet I perceived much wind in it as it paſſed by us. Vol. III. page 223.

Account of another SPOUT. *From the ſame.*

" WE ſaw a ſpout but a ſmall diſtance from us; it fell down out of a black cloud that yielded great ſtore of rain, thunder, and lightning. This cloud hovered to the Southward of us for the ſpace of three hours, and then drew to the Weſtward a great pace, at which time it was that we ſaw the ſpout, which hung faſt to the cloud till it broke, and then the cloud whirled about to the South-Eaſt, then to the North-Eaſt, where meeting with an iſland, it ſpent itſelf, and ſo diſperſed; and immediately we had a little of the tail of it, having had none before." Vol. III. page 182.

* Probably if it had been laſting, a cloud would have been formed above it. Theſe extracts from *Dampier*, ſeem, in different inſtances, to favour both opinions, and, therefore, are inſerted entire, for the Reader's conſideration.

Extract

Extract of a Letter from J. B. *Eſq; in* Boſton, *to* B. F. *concerning the Light in Sea-Water.*

November 12, 1753.

Read at the Royal Society, *Dec.* 16, 1756.

WHEN I was at the Eaſtward, I had an opportunity of obſerving the luminous appearance of the ſea when diſturbed: At the head and ſtern of the veſſel, when under way, it appeared very bright. The beſt opportunity I had to obſerve it, was in a boat, in company with ſeveral Gentlemen going from *Portſmouth*, about three miles, to our veſſel lying at the mouth of *Piſcataqua* River. Soon after we ſet off (it being in the evening) we obſerved a luminous appearance, where the oars daſhed the water. Sometimes it was very bright, and afterwards as we rowed along, gradually leſſened, till almoſt imperceptible, and then re-illumined. This we took notice of ſeveral times in the paſſage. When I got on board the veſſel, I ordered a pail to be dipped up, full of ſea-water, in which, on the water's being moved, a ſparkling light appeared. I took a linnen cloth, and ſtrained ſome of the water through it, and there was a like appearance on the cloth, which ſoon went off; but on rubbing the

cloth

cloth with my finger, it was renewed. I then carried the cloth to the light, but could not perceive any thing upon it which ſhould cauſe that appearance.

Several Gentlemen were of opinion, that the ſeparated particles of putrid, animal, and other bodies, floating on the ſurface of the ſea, might cauſe that appearance; for putrid fiſh, *&c.* they ſaid, will cauſe it: And the ſea-animals which have died, and other bodies putrified therein ſince the creation, might afford a ſufficient quantity of theſe particles to cover a conſiderable portion of the ſurface of the ſea; which particles being differently diſperſed, might account for the different degrees of light in the appearance above-mentioned. But this account ſeems liable to this obvious objection, That as putrid fiſh, *&c.* make a luminous appearance without being moved or diſturbed, it might be expected that the ſuppoſed putrid particles on the ſurface of the ſea, ſhould always appear luminous, where there is not a greater light; and, conſequently, that the whole ſurface of the ſea, covered with thoſe particles, ſhould always, in dark nights, appear luminous, without being diſturbed. But this is not fact.

Among the reſt, I threw out my conjecture, That the ſaid appearance might be cauſed by a great number of little animals, floating on the ſurface of the ſea, which, on being diſturbed, might, by expanding their finns, or otherwiſe moving themſelves, expoſe ſuch a part of their bodies as exhibits a luminous appearance, ſomewhat in the manner of a glow-worm, or fire-fly: That theſe animals

may

may be more numerous in ſome places than others; and, therefore, that the appearance above-mentioned being fainter and ſtronger in different places, might be owing to that: That certain circumſtances of weather, *&c.* might invite them to the ſurface, on which, in a calm, they might ſport themſelves and glow; or in ſtorms, being forced up, make the ſame appearance.

There is no difficulty in conceiving that the ſea may be ſtocked with animalcula for this purpoſe, as we find all Nature crowded with life. But it ſeems difficult to conceive that ſuch ſmall portions of matter, even if they were wholly luminous, ſhould affect our ſight; much more ſo, when it is ſuppoſed that only a part of them is luminous. But, if we conſider ſome other appearances, we may find the ſame difficulty to conceive of them; and yet we know they take place. For inſtance, the flame of a candle, which, it is ſaid, may be ſeen four miles round. The light which fills this circle of eight miles diameter, was contained when it firſt left the candle within a circle of half an inch diameter. If the denſity of light, in theſe circumſtances, be as thoſe circles to each other, that is, as the ſquares of their diameters, the candle-light, when come to the eye, will be 1027709337600 times rarer than when it quitted the half inch circle. Now the aperture of the eye, through which the light paſſes, does not exceed one-tenth of an inch diameter, and the portion of the leſſer circle, which correſponds to this ſmall portion of the greater circle, muſt be proportionably, that is, 1027709337600 times leſs than one-tenth of an inch; and yet this infinitely ſmall point (if you will allow the expreſſion) affords light enough to make it viſible four miles; or, rather, affords light ſufficient to affect the ſight at that diſtance.

The ſmallneſs of the animalcula is no objection then to this conjecture; for ſuppoſing them to be ten thouſand times leſs than the *minimum viſibile*, they may, notwithſtanding, emit light enough to affect the eyes, and ſo to cauſe the luminous appearance aforeſaid. This conjecture I ſend you for want of ſomething better.

Farther REMARKS *by a Gentleman of* New-York.

April 2, 1754.

Read at the Royal-Society, *Dec.* 16, 1756.

ANY knowledge I have of the winds, and other changes which happen in the atmoſphere, is ſo very defective that it does not deſerve the name; neither have I received any ſatisfaction from the attempts of others on this ſubject. It deſerves then your thoughts, as a ſubject in which you may diſtinguiſh yourſelf and be uſeful.

Your notion of ſome things conducting heat or cold better than others, pleaſes me, and I wiſh you may purſue the ſcent. If I remember right, Dr *Boerhaave*, in his chy-

chymiſtry, thinks that heat is propagated by the vibration of a ſubtle elaſtic fluid, diſperſed through the atmoſphere and through all bodies. Sir *Iſaac Newton* ſays, there are many phænomena to prove the exiſtence of ſuch a fluid; and this opinion has my aſſent to it. I ſhall only obſerve that it is eſſentially different from that which I call æther; for æther, properly ſpeaking, is neither a fluid nor elaſtic; its power conſiſts in re-acting any action communicated to it, with the ſame force it receives the action.

I long to ſee your explication of water-ſpouts, but I muſt tell you before hand, that it will not be eaſy for you to convince me that the principal phænomena were not occaſioned by a ſtream of wind iſſuing with great force, my eyes and ears both concurring to give me this ſentiment, I could have no more evidence than to feel the effects, which I had no inclination to do.

It ſurpriſes me a little, that wind, generated by fermentation, is new to you, ſince it may be every day obſerved in fermenting liquor. You know with what force fermenting liquors will burſt the veſſels which contain them, if the generated wind have not vent; and with what force it iſſues on giving it a ſmall vent, or by drawing the cork of a bottle. Dr *Boerhaave* ſays, that the ſteam iſſuing from fermenting liquors received through a very ſmall vent-hole, into the noſe, will kill as ſuddenly and certainly as lightning. That air is generated by fermentation, I think you will find fully proved in Dr *Hales's Analyſis of* *the air*, in his Vegetable Statics. If you have not read the book, you have a new pleaſuré to come.

The ſolution you give to the objection I made from the contrary winds blowing from the oppoſite ſides of the mountains, from their being eddies, does not pleaſe me, becauſe the extent of theſe winds is by far too large to be occaſioned by an eddy. It is forty miles from *New-York* to our mountains, through which *Hudſon*'s *River* paſſes. The river runs twelve miles in the mountains, and from the North ſide of the mountains it is about ninety miles to *Albany*. I have myſelf been on board a veſſel more than once, when we have had a ſtrong Northerly wind againſt us, all the way from *New-York*, for two or three days. We have met veſſels from *Albany*, who aſſured us, that, on the other ſide of the mountains, they had, at the ſame time, a ſtrong continued Southerly wind againſt them; and this frequently happens.

I have frequently ſeen both, on the river, in places where there could be no eddy-winds, and on the open ſea, two veſſels ſailing with contrary winds, within half a mile of each other; but this happens only in eaſy winds, and generally calm in other places near theſe winds.

You have, no doubt, frequently obſerved a ſingle cloud paſs, from which a violent guſt of wind iſſues, but of no great extent. I have obſerved ſuch a guſt make a lane through the woods, of ſome miles in length, by laying the trees flat to the ground, and not above eight or ten chains in breadth. Though the violence of the wind be in the ſame

ſame direction in which the cloud moves and precedes it, yet wind iſſues from all ſides of it ; ſo that ſuppoſing the cloud move South-Eaſterly, thoſe on the North-Eaſt ſide of it feel a South-Weſt wind, and others on the South-Weſt ſide, a North-Eaſt. And where the cloud paſſes over, we frequently have a South-Eaſt wind from the hinder part of it, but none violent, except the wind in the direction in which the cloud moves. To ſhew what it is which prevents the wind from iſſuing out equally on all ſides, is not an eaſy problem to me, and I ſhall not attempt to ſolve it ; but when you ſhall ſhew what it is which reſtrains the electrical fluid from ſpreading itſelf into the air ſurrounding it, when it ruſhes with great violence through the air along, or in the conductor, for a great extent in length, then I may hope to explain the other problem, and remove the difficulty we have in conceiving it.

Propoſal of an EXPERIMENT *to meaſure the time taken up by an Electric Spark, in moving through any given Space. By* J. A. *Eſq; of* New-York.

Read at the Royal Society *Dec.* 26, 1756.

IF I remember right, the Royal Society made one experiment to diſcover the velocity of the electric fire, by a wire of about four miles in length, ſupported by ſilk, and by turning it forwards and backwards in a field, ſo that the beginning and end of the wire were at only the diſtance of two people, the one holding the *Leyden* bottle and the beginning of the wire, and the other holding the end of the wire and touching the ring of the bottle; but by this experiment no diſcovery was made, except that the velocity was extremely quick.

As water is a conductor as well as metals, it is to be conſidered whether the velocity of the electric fire might not be diſcovered by means of water; whether a river, or lake, or ſea, may not be made part of the circuit through which the electric fire paſſes? inſtead of the circuit all of wire, as in the above experiment.

Whether in a river, lake, or ſea, the electric fire will not diſſipate and not return to the bottle? or, will it proceed in ſtrait lines through the water the ſhorteſt courſes poſſible back to the bottle.

If the laſt, then ſuppoſe one brook that falls into *Delaware* doth head very near to a brook that falls into *Schuylkill*,

kill, and let a wire be ſtretched and ſupported as before, from the head of the one brook to the head of the other, and let the one end communicate with the water, and let one perſon ſtand in the other brook, holding the *Leyden* bottle, and let another perſon hold that end of the wire not in the water, and touch the ring of the bottle.—If the electric fire will go as in the laſt queſtion, then will it go down the one brook to *Delaware* or *Schuylkill*, and down one of them to their meeting, and up the other and the other brook; the time of its doing this may poſſibly be obſervable, and the further upwards the brooks are choſen, the more obſervable it would be.

Should this be not obſervable, then ſuppoſe the two brooks falling into *Saſquehana* and *Delaware*, and proceeding as before, the electric fire may, by that means, make a circuit round the North Cape of *Virginia*, and go many hundreds of miles, and in doing that, it would ſeem it muſt take ſome obſervable time.

If ſtill no obſervable time is found in that experiment, then ſuppoſe the brooks falling the one into the *Ohio*, and the other into *Saſquehana*, or *Potomack*, in that the electric fire would have a circuit of ſome thouſands of miles to go down *Ohio* to *Miſſiſippi*, to the Bay of *Mexico*, round *Florida*, and round the South Cape of *Virginia*; which, I think, would give ſome obſervable time, and diſcover exactly the velocity.

But if the electric fire diſſipates, or weakens in the water, as I fear it does, theſe experiments will not anſwer.

An-

Anſwer to the foregoing; *by* B. F.

Read at the Royal-Society, *Dec.* 23, 1756.

SUppoſe a tube of any length open at both ends, and containing a moveable wire of juſt the ſame length, that fills its bore. If I attempt to introduce the end of another wire into the ſame tube, it muſt be done by puſhing forward the wire it already contains; and the inſtant I preſs and move one end of that wire, the other end is alſo moved; and in introducing one inch of the ſame wire, I extrude, at the ſame time, an inch of the firſt, from the other end of the tube.

If the tube be filled with water, and I inject an additional inch of water at one end, I force out an equal quantity at the other, in the very ſame inſtant.

And the water forced out at one end of the tube is not the very ſame water that was forced in at the other end at the ſame time, it was only in motion at the ſame time.

The long wire made uſe of in the experiment to diſcover the velocity of the electric fluid, is itſelf filled with what we call its natural quantity of that fluid, before the hook of the *Leyden* bottle is applied to one end of it.

The outſide of the bottle being at the time of ſuch application, in contact with the other end of the wire; the whole

whole quantity of electric fluid contained in the wire is, probably, put in motion at once.

For at the inſtant the hook, connected with the inſide of the bottle, *gives out*; the coating, or outſide of the bottle, *draws in* a portion of that fluid.

If ſuch long wire contains preciſely the quantity that the outſide of the bottle demands, the whole will move out of the wire to the outſide of the bottle, and the over quantity which the inſide of the bottle contained, being exactly equal, will flow into the wire, and remain there, in the place of the quantity the wire had juſt parted with to the outſide of the bottle.

But if the wire be ſo long as that one-tenth (ſuppoſe) of its natural quantity is ſufficient to ſupply what the outſide of the bottle demands, in ſuch caſe the outſide will only receive what is contained in one-tenth of the wire's length, from the end next to it; though the whole will move ſo as to make room at the other end for an equal quantity iſſuing, at the ſame time, from the inſide of the bottle.

So that this experiment only ſhews the extream facility with which the electric fluid moves in metal; it can never determine the velocity.

And, therefore, the propoſed experiment (though well imagined, and very ingenious) of ſending the ſpark round through a vaſt length of ſpace, by the waters of *Suſquehannah*, or *Potowmack*, and *Ohio*, would not afford the ſatisfaction deſired, though we could be ſure that the motion of the electric fluid would be in that tract, and not under ground in the wet earth by the ſhorteſt way.

An Account of the new-invented Pensylvanian FIRE-PLACES: *Wherein their Construction and Manner of Operation is particularly explained; their Advantages above every other Method of warming Rooms demonstrated; and all Objections that have been raised against the Use of them, answered and obviated. With Directions for putting them up, and for using them to the best Advantage. And a Copper-Plate, in which the several Parts of the Machine are exactly laid down, from a Scale of equal Parts.*

By B. F. *first printed at* Philadelphia *in* 1745.

IN these Northern Colonies the inhabitants keep fires to sit by, generally seven months in the year; that is, from the beginning of *October*, to the end of *April*; and, in some winters, near eight months, by taking in part of *September* and *May*.

Wood, our common fuel, which within these hundred years might be had at every man's door, must now be fetched near one hundred miles to some towns, and makes a very considerable article in the expence of families.

As therefore so much of the comfort and conveniency of our lives, for so great a part of the year, depends on the article of *fire*; since fuel is become so expensive, and (as the country is more cleared and settled) will of course grow scarcer and dearer, any new proposal for saving the wood,

wood, and for lessening the charge, and augmenting the benefit of fire, by some particular method of making and managing it, may at least be thought worth consideration.

The new FIRE-PLACES are a late invention to that purpose, of which this paper is intended to give a particular account.

That the reader may the better judge whether this method of managing fire has any advantage over those heretofore in use, it may be proper to consider both the old and new methods separately and particularly, and afterwards make the comparison.

In order to this, 'tis necessary to understand well, some few of the properties of air and fire, *viz.*

1. Air is rarified by *heat*, and condens'd by *cold*; *i. e.* the same quantity of air takes up more space when warm than when cold. This may be shown by several very easy experiments. Take any clear glass bottle (a *Florence* flask stript of the straw is best) place it before the fire, and as the air within is warmed and rarified, part of it will be driven out of the bottle; turn it up, place its mouth in a vessel of water, and remove it from the fire; then, as the air within cools and contracts, you will see the water rise in the neck of the bottle, supplying the place of just so much air as was driven out. Hold a large hot coal near the side of the bottle, and as the air within feels the heat, it will again distend and force out the water.——Or, fill a bladder half full of air, tie the neck tight, and lay it before a fire as near as may be without scorching the blad-

der; as the air within heats, you will perceive it to ſwell and fill the bladder, till it becomes tight, as if full blown: Remove it to a cool place, and you will ſee it fall gradually, till it becomes as lank as at firſt.

2. Air rarified and diſtended by heat, is * ſpecifically lighter than it was before, and will riſe in other air of greater denſity. As wood, oil, or any other matter ſpecifically lighter than water, if placed at the bottom of a veſſel of water, will riſe till it comes to the top; ſo rarified air will riſe in common air, till it either comes to air of equal weight, or is by cold reduced to its former denſity.

A fire then being made in any chimney, the air over the fire is rarified by the heat, becomes lighter, and therefore immediately riſes in the funnel, and goes out; the other air in the room (flowing towards the chimney) ſupplies its place, is rarified in its turn, and riſes likewiſe; the place of the air thus carried out of the room, is ſupplied by freſh air coming in through doors and windows, or, if they be ſhut, through every crevice with violence, as may be ſeen by holding a candle to a key-hole: If the room be ſo tight as that all the crevices together will not ſupply ſo much air as is continually carried off, then, in a little time, the current up the funnel muſt flag, and the ſmoke being no longer driven up, muſt come into the room.

* Body or matter of any ſort, is ſaid to be *ſpecifically* heavier or lighter than other matter, when it has more or leſs ſubſtance or weight in the ſame dimenſions.

1. Fire,

1. Fire, (*i. e.* common fire) throws out light, heat, and smoke (or fume.) The two first move in right lines, and with great swiftness, the latter is but just separated from the fuel, and then moves only as it is carried by the stream of rarified air : And without a continual accession and recession of air, to carry off the smoaky fumes, they would remain crouded about the fire, and stifle it.*

2. Heat may be separated from the smoke as well as from the light, by means of a plate of iron, which will suffer heat to pass through it without the others.

3. Fire sends out its rays of heat, as well as rays of light, equally every way ; but the greatest sensible heat is over the fire, where there is, besides the rays of heat shot upwards, a continual rising stream of hot air, heated by the rays shot round on every side.

These things being understood, we proceed to consider the Fire-places heretofore in use, *viz.*

1. The large open fire-places used in the days of our fathers, and still generally in the country, and in kitchens.

2. The newer-fashioned fire-places, with low breasts, and narrow hearths.

3. Fire-places with hollow backs, hearths and jams of iron, (described by M. *Gauger*, in his tract entitled, *La Mechanique de Feu*) for warming the air as it comes into the room.

4. The *Holland* stoves, with iron doors opening into the room.

5. The

5. The *German* ſtoves, which have no opening in the room where they are uſed, but the fire is put in from ſome other room, or from without.

6. Iron pots, with open charcoal fires, placed in the middle of a room.

1. The firſt of theſe methods has generally the conveniency of two warm ſeats, one in each corner; but they are ſometimes too hot to abide in, and, at other times, incommoded with the ſmoke; there is likewiſe good room for the cook to move, to hang on pots, &c. Their inconveniences are, that they almoſt always ſmoke, if the door be not left open; that they require a large funnel, and a large funnel carries off a great quantity of air, which occaſions what is called a ſtrong draft to the chimney, without which ſtrong draft, the ſmoke would come out of ſome part or other of ſo large an opening, ſo that the door can ſeldom be ſhut; and the cold air ſo nips the backs and heels of thoſe that ſit before the fire, that they have no comfort till either ſcreens or ſettles are provided (at a conſiderable expence) to keep it off, which both cumber the room, and darken the fire ſide. A moderate quantity of wood on the fire, in ſo large a hearth, ſeems but little; and, in ſo ſtrong and cold a draught, warms but little; ſo that people are continually laying on more. In ſhort, 'tis next to impoſſible to warm a room with ſuch a fire-place: And I ſuppoſe our anceſtors never thought of warming rooms to ſit in; all they purpos'd was, to have a place to make a fire in, by which they might warm themſelves when cold.

2. Moſt

2. Most of these old-fashioned chimneys in towns and cities, have been, of late years, reduced to the second sort mentioned, by building jambs within them, narrowing the hearth, and making a low arch or breast. 'Tis strange, methinks, that though chimneys have been so long in use, their construction should be so little understood till lately, that no workman pretended to make one which should always carry off all the smoke, but a chimney-cloth was looked upon as essential to a chimney. This improvement, however, by small openings and low breasts, has been made in our days; and success in the first experiments has brought it into general use in cities, so that almost all new chimnies are now made of that sort, and much fewer bricks will make a stack of chimneys now than formerly. An improvement so lately made, may give us room to believe, that still farther improvements may be found to remedy the inconveniencies yet remaining. For these new chimneys, though they keep rooms generally free from smoke, and, the opening being contracted, will allow the door to be shut, yet the funnel still requiring a considerable quantity of air, it rushes in at every crevice so strongly, as to make a continual whistling or howling, and it is very uncomfortable, as well as dangerous, to sit against any such crevice. Many colds are caught from this cause only, it being safer to sit in the open street, for then the pores do all close together, and the air does not strike so sharply against any particular part of the body.

The *Spaniards* have a proverbial ſaying,

If the wind blows on you through a hole,
Make your will, and take care of your ſoul.

Women, particularly, from this cauſe, as they ſit much in the houſe, get colds in the head, rheums and defluctions, which fall into their jaws and gums, and have deſtroyed early many a fine ſet of teeth in theſe Northern colonies. Great and bright fires do alſo very much contribute to damage the eyes, dry and ſhrivel the ſkin, and bring on early the appearances of old age. In ſhort, many of the diſeaſes proceeding from colds, as fevers, pleuriſies, *&c.* fatal to very great numbers of people, may be aſcribed to ſtrong drawing chimneys, whereby, in ſevere weather, a man is ſcorched before, while he is froze behind *. In the mean time, very little is done by theſe chimneys

* As the writer is neither phyſician nor philoſopher, the reader may expect he ſhould juſtify theſe his opinions by the authority of ſome that are ſo. M. *Clare*, F.R.S. in his treatiſe of *The motion of fluids*, ſays, *pag.* 246, *&c.* " And here it may be remarked, that it is more prejudicial to " health, to ſit near a window or door, in a room where there are many " candles and a fire, than in a room without; for the conſumption of air " thereby occaſioned, will always be very conſiderable, and this muſt ne- " ceſſarily be replaced by cold air from without. Down the chimney can " enter none, the ſtream of warm air, always ariſing therein, abſolutely " forbids it; the ſupply muſt therefore come in wherever other open- " ings ſhall be found. If theſe happen to be ſmall, *let thoſe who ſit near* " *them beware*; the ſmaller the floodgate, the ſmarter will be the ſtream. " Was a man, even in a ſweat, to leap into a cold bath, or jump from " his warm bed, in the intenſeſt cold, even in a froſt, provided he do not " continue over-long therein, and be in health when he does this, we ſee " by experience that he gets no harm. If he ſits a little while againſt a " window, into which a ſucceſſive current of cold air comes, his pores are " cloſed, and he gets a fever. In the firſt caſe, the ſhock the body endures

chimneys towards warming the room; for the air round the fire-place, which is warmed by the direct rays from the fire, does not continue in the room, but is continually crouded and gathered into the chimney by the current of cold air coming behind it, and so is presently carried off.

" dures is general, uniform, and therefore less fierce; in the other a single " part, a neck or ear perchance, is attacked, and that with the greater " violence probably, as it is done by a successive stream of cold air. And " the cannon of a battery, pointed against a single part of a bastion, will " easier make a breach than were they directed to play singly upon the " whole face, and will admit the enemy much sooner into the town."

That warm rooms, and keeping the body warm in winter, are means of preventing such diseases, take the opinion of that learned *Italian* physician *Antonio Porcio*, in the preface to his tract *de Militis Sanitate tuenda*, where, speaking of a particular wet and cold winter, remarkable at *Venice* for its sickliness, he says, *Popularis autem pleuritis quæ Venetiis sæviit mensibus* Dec. Jan. Feb. *ex cæli, aërisque inclementia facta est, quod non habeant hypocausta* [stove-rooms] *& quod non soliciti sunt* Itali *omnes de auribus, temporibus, collo, totoque corpore defendendis ab injuriis aëris; et tegmina domorum* Veneti *disponant parum inclinata, ut nives diutius permaneant super tegmina. E contra,* Germani, *qui experiuntur cæli inclementiam, perdidicere sese defendere ab aëris injuria. Tecta construunt multum inclinata, ut decidant nives.* Germani *abundant lignis, domusque* hypocaustis; *foris autem incedunt pannis, pellibus, gossipio, bene mehercule loricati atque muniti. In* Bavaria *interrogabam (curiositate motus videndi* Germaniam*) quot nam elapsis mensibus pleuritide vel peripneumonia fuissent absumti; dicebant vix unus aut alter illis temporibus pleuritide fuit correptus.*

The great Dr *Boerhaave*, whose authority alone might be sufficient, in his Aphorisms mentions, as one antecedent cause of pleurisies, *a cold air, driven violently through some narrow passage upon the body, overheated by labour or fire.*

The Eastern physicians agree with the *Europeans* in this point; witness the *Chinese* treatise entitled *Tchang seng*, i. e. *The art of procuring health and long life*, as translated in *Pere Du Halde*'s account of *China*, which has this passage. *As of all the passions which ruffle us, Anger does the most mischief, so of all the malignant affections of the air, a* wind that comes thro' any narrow passage, *which is cold and piercing, is* most dangerous; *and coming upon us unawares, insinuates itself into the body, often causing grievous diseases. It should therefore be avoided, according to the advice of the ancient proverb, as carefully as the point of an arrow.* These mischiefs are avoided by the use of the new-invented fire-places, as will be shewn hereafter.

In both theſe ſorts of fire-places, the greateſt part of the heat from the fire is loſt; for as fire naturally darts heat every way, the back, the two jambs, and the hearth, drink up almoſt all that is given them, very little being reflected from bodies ſo dark, porous, and unpoliſhed; and the upright heat, which is by far the greateſt, flies directly up the chimney. Thus five ſixths at leaſt of the heat (and conſequently of the fewel) is waſted, and contributes nothing towards warming the room.

3. To remedy this, the Sieur *Gauger* gives, in his book entitled, *La Mechanique de Feu*, publiſhed in 1709, ſeven different conſtructions of the third ſort of chimneys mentioned above, in which there are hollow cavities made by iron plates in the back, jambs, and hearth, through which plates the heat paſſing, warms the air in thoſe cavities, which is continually coming into the room freſh and warm. The invention was very ingenious, and had many conveniencies: The room was warmed in all parts, by the air flowing into it through the heated cavities: Cold air was prevented ruſhing through the crevices, the funnel being ſufficiently ſupplied by thoſe cavities: Much leſs fuel would ſerve, &c. But the firſt expence, which was very great; the intricacy of the deſign, and the difficulty of the execution, eſpecially in old chimnies, diſcouraged the propagation of the invention; ſo that there are, I ſuppoſe, very few ſuch chimnies now in uſe. [The upright heat, too, was almoſt all loſt in theſe, as in the common chimneys.]

4. The

4. The *Holland* iron ſtove, which has a flue proceeding from the top, and a ſmall iron door opening into the room, comes next to be conſidered. Its conveniencies are, that it makes a room all over warm; for the chimney being wholly cloſed, except the flue of the ſtove, very little air is required to ſupply that, and therefore not much ruſhes in at crevices, or at the door when it is opened. Little fewel ſerves, the heat being almoſt all ſaved; for it rays out almoſt equally from the four ſides, the bottom and the top, into the room, and preſently warms the air around it, which being rarified, riſes to the cieling, and its place is ſupplied by the lower air of the room, which flows gradually towards the ſtove, and is there warmed, and riſes in its turn, ſo that there is a continual circulation till all the air in the room is warmed. The air, too, is gradually changed, by the ſtove-door's being in the room, through which part of it is continually paſſing, and that makes theſe ſtoves wholeſomer, or at leaſt pleaſanter than the *German* ſtoves, next to be ſpoke of.—But they have theſe inconveniences. There is no ſight of the fire, which is in itſelf a pleaſant thing. One cannot conveniently make any other uſe of the fire but that of warming the room. When the room is warm, people not ſeeing the fire, are apt to forget ſupplying it with fuel till it is almoſt out, then, growing cold, a great deal of wood is put in, which ſoon makes it too hot. The change of air is not carried on quite quick enough, ſo that if any ſmoke or ill ſmell happens in the room, it is a long time before it is diſcharged.

For theſe reaſons the *Holland* ſtove has not obtained much among the *Engliſh* (who love the ſight of the fire) unleſs in ſome workſhops, where people are obliged to ſit near windows for the light, and in ſuch places they have been found of good uſe.

5. The *German* ſtove is like a box, one ſide wanting. It is compoſed of five iron plates ſcrued together, and fixed ſo as that you may put the fuel into it from another room, or from the outſide of the houſe. It is a kind of oven reverſed, its mouth being without, and body within the room that is to be warmed by it. This invention certainly warms a room very ſpeedily and thoroughly with little fuel: No quantity of cold air comes in at any crevice, becauſe there is no diſcharge of air which it might ſupply, there being no paſſage into the ſtove from the room. Theſe are its conveniencies.—Its inconveniencies are, That people have not even ſo much ſight or uſe of the fire as in the *Holland* ſtoves, and are, moreover, obliged to breathe the ſame unchang'd air continually, mixed with the breath and perſpiration from one another's bodies, which is very diſagreeable to thoſe who have not been accuſtomed to it.

6. Charcoal fires in pots, are uſed chiefly in the ſhops of handicraftsmen. They warm a room (that is kept cloſe, and has no chimney to carry off the warmed air) very ſpeedily and uniformly; but there being no draught to change the air, the ſulphurous fumes from the coals [be they ever ſo well kindled before they are brought in, there will be ſome] mix with it, render it diſagreeable, hurtful to

ſome

ſome conſtitutions, and ſometimes, when the door is long kept ſhut, produce fatal conſequences.

To avoid the ſeveral inconveniencies, and at the ſame time retain all the advantages of other fire-places, was contrived the PENNSYLVANIA FIRE-PLACE, now to be deſcribed.

This Machine conſiſts of

A bottom-plate, (i) [*See the Plate annexed.*]

A back plate, (ii)

Two ſide plates, (iii iii)

Two middle plates, (iv iv) which joined together, form a tight box, with winding paſſages in it for warming the air.

A front plate, (v)

A top plate, (vi)

Theſe are all caſt of iron, with mouldings or ledges where the plates come together, to hold them faſt, and retain the mortar uſed for pointing to make tight joints. When the plates are all in their places, a pair of ſlender rods with ſcrews, are ſufficient to bind the whole very firmly together, as it appears in *Fig.* 2.

There are, moreover, two thin plates of wrought iron, *viz.* the ſhutter, (vii) and the regiſter, (viii); beſides the ſcrew-rods O P, all which we ſhall explain in their order.

(i) The bottom plate or hearth-piece, is round before, with a riſing moulding that ſerves as a fender to keep coals and aſhes from coming to the floor, *&c.* It has two ears, F G, perforated to receive the ſcrew-rods O P; a long air-hole, *a a*, through which the freſh outward air paſſes up

into

into the air-box; and three ſmoke-holes B C through which the ſmoke deſcends and paſſes away; all repreſented by dark ſquares. It has alſo double ledges to receive between them the bottom edges of the back plate, the two ſide-plates, and the two middle-plates. Theſe ledges are about an inch aſunder, and about half an inch high; a profile of two of them joined to a fragment of plate, appears in *Fig.* 3.

(ii) The back plate is without holes, having only a pair of ledges on each ſide, to receive the back edges of the two

(iii iii) Side plates: Theſe have each a pair of ledges to receive the ſide-edges of the front plate, and a little ſhoulder for it to reſt on; alſo two pair of ledges to receive the ſide edges of the two middle plates which form the air-box; and an oblong air-hole near the top, through which is diſcharged into the room the air warmed in the air-box. Each has alſo a wing or bracket, H and I, to keep in falling brands, coals, *&c*, and a ſmall hole, Q and R, for the axis of the regiſter to turn in.

(iv iv) The air-box is compoſed of the two middle plates D E and F G. The firſt has five thin ledges or partitions caſt on it, two inches deep, the edges of which are received in ſo many pair of ledges caſt in the other. The tops of all the cavities formed by theſe thin deep ledges, are alſo covered by a ledge of the ſame form and depth, caſt with them; ſo that when the plates are put together, and the joints luted, there is no communication between the air-

air-box and the ſmoke. In the winding paſſages of this box, freſh air is warm'd as it paſſes into the room.

(v) The front plate is arched on the under ſide, and ornamented with foliages, *&c.* it has no ledges.

(vi) The top plate has a pair of ears, M N, anſwerable to thoſe in the bottom plate, and perforated for the ſame purpoſe: It has alſo a pair of ledges running round the under ſide, to receive the top edges of the front, back, and ſide-plates. The air-box does not reach up to the top plate by two inches and half.

(vii) The ſhutter is of thin wrought iron and light, of of ſuch a length and breadth as to cloſe well the opening of the fire-place. It is uſed to blow up the fire, and to ſhut up and ſecure it a nights. It has two braſs knobs for handles, *d d*, and commonly ſlides up and down in a groove, left, in putting up the fire-place, between the foremoſt ledge of the ſide-plates, and the face of the front plate; but ſome chuſe to ſet it aſide when it is not in uſe, and apply it on occaſion.

(viii) The regiſter is alſo of thin wrought iron. It is placed between the back plate and air-box, and can, by means of the key S, be turned on its axis, ſo as to lie in any poſition between level and upright.

The ſcrew-rods O P are of wrought iron, about a third of an inch thick, with a button at bottom, and a ſcrew and nut at top, and may be ornamented with two ſmall braſſes ſcrewed on above the nuts.

To

To put this Machine to work,

1. A falſe back of four inch (or, in ſhallow ſmall chimneys, two inch) brick work is to be made in the chimney, four inches or more from the true back: From the top of this falſe back a cloſing is to be made over to the breaſt of the chimney, that no air may paſs into the chimney, but what goes under the falſe back, and up behind it.

2. Some bricks of the hearth are to be taken up, to form a hollow under the bottom plate; acroſs which hollow runs a thin tight partition, to keep apart the air entering the hollow and the ſmoke; and is therefore placed between the air-hole and ſmoke-holes.

3. A paſſage is made, communicating with the outward air, to introduce that air into the fore part of the hollow under the bottom-plate, whence it may riſe thro' the air-hole into the air-box.

4. A paſſage is made from the back part of the hollow, communicating with the flue behind the falſe back: Through this paſſage the ſmoke is to paſs.

The fire-place is to be erected upon theſe hollows, by putting all the plates in their places, and ſcrewing them together.

Its operation may be conceived by obſerving the following

PRO-

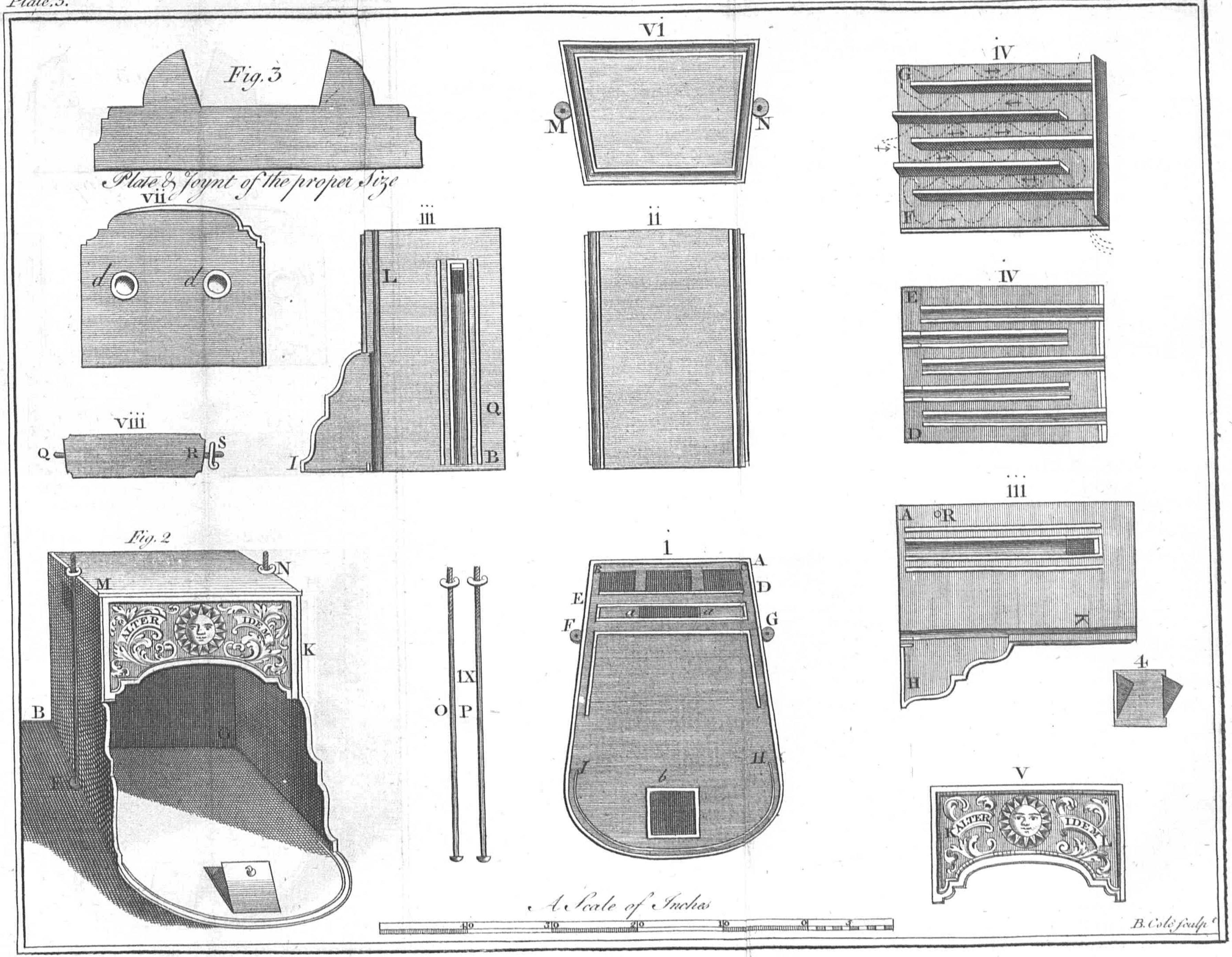


Fig. 3
Plate & Joynt of the proper Size
VI
M
N
IV
G
F
VII
d
d
III
L
Q
B
I
II
IV
E
D
VIII
Q
R
S
III
A
R
K
H
Fig. 2
N
M
ALTER
IDEM
K
B
IX
O
P
I
A
D
E
F
G
a
a
H
b
4
V
ALTER
IDEM
A Scale of Inches
40
30
20
10
0
B. Cole sculp.

PROFILE *of the* CHIMNEY *and* FIRE-PLACE.

M The mantle-piece, or breast of the chimney.

C The funnel.

B The false back and closing.

E True back of the chimney.

T Top of the fire-place.

F The front of it.

A The place where the fire is made.

D The air-box.

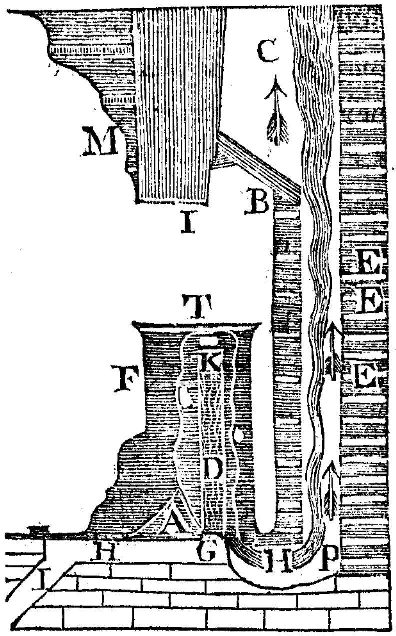
K The hole in the side-plate, through which the warmed air is discharged out of the air-box into the room.

H The hollow filled with fresh air, entering at the passage *I*, and ascending into the air-box through the air-hole in the bottom plate near

G The partition in the hollow to keep the air and smoke apart.

P The passage under the false back and part of the hearth for the smoke.

The arrows show the course of the smoke.



The fire being made at A, the flame and smoke will ascend and strike the top T, which will thereby receive a considerable heat. The smoke, finding no passage upwards, turns over the top of the air-box, and descends between it and the back plate to the holes at B, in the bottom plate, heating, as it passes, both plates of the air-box, and the said back plate; the front plate, bottom and side plates, are also all heated at the same time. The smoke proceeds in the passage that leads it under and behind the false back, and so rises into the chimney. The air of the room, warmed behind the back plate, and by the sides, front, and top plates, becoming ſpecifically lighter than the other air in the room, is obliged to riſe; but the cloſure over the fire-place hindering it from going up the chimney, it is forced out into the room, riſes by the mantlepiece to the cieling, and ſpreads all over the top of the room, whence being crouded down gradually by the ſtream of newly-warm'd air that follows and riſes above it, the whole room becomes in a ſhort time equally warmed.

At the ſame time the air, warmed under the bottom-plate, and in the air-box, riſes and comes out of the holes in the ſide-plates, very ſwiftly if the door of the room be ſhut, and joins its current with the ſtream before mentioned, riſing from the ſide, back, and top plates.

The air that enters the room through the air-box is freſh, though warm; and, computing the ſwiftneſs of its motion with the areas of the holes, it is found that near ten barrels of freſh air are hourly introduced by the air-box; and by this means the air in the room is continually changed, and kept, at the ſame time, ſweet and warm.

It is to be obſerved, that the entering air will not be warm at firſt lighting the fire, but heats gradually as the fire encreaſes.

A ſquare opening for a trap-door ſhould be left in the cloſing of the chimney, for the ſweeper to go up: The door may be made of ſlate or tin, and commonly kept cloſe ſhut, but ſo placed as that turning up againſt the back of the chimney when open, it cloſes the vacancy behind the falſe back, and ſhoots the ſoot, that falls in ſweep-

ing,

ing, out upon the hearth. This trap-door is a very convenient thing.

In rooms where much ſmoking of tobacco is uſed, it is alſo convenient to have a ſmall hole, about five or ſix inches ſquare, cut near the cieling through into the funnel: This hole muſt have a ſhutter, by which it may be clos'd or open'd at pleaſure. When open, there will be a ſtrong draught of air thro' it into the chimney, which will preſently carry off a cloud of ſmoke, and keep the room clear: If the room be too hot likewiſe, it will carry off as much of the warm air as you pleaſe, and then you may ſtop it entirely, or in part, as you think fit. By this means it is, that the tobacco ſmoke does not deſcend among the heads of the company near the fire, as it muſt do before it can get into common chimneys.

The Manner of uſing this FIRE-PLACE.

Your cord-wood muſt be cut into three lengths; or elſe a ſhort piece, fit for the fire-place, cut off, and the longer left for the kitchen or other fires. Dry hickery, or aſh, or any woods that burn with a clear flame are rather to be choſen, becauſe ſuch are leſs apt to foul the ſmoke-paſſages with ſoot; and flame communicates, with its light, as well as by contact, greater heat to the plates and room. But where more ordinary wood is uſed, half a dry faggot of bruſh-wood, burnt at the firſt making of fire in the morning is very advantageous, as it it immediately, by its ſudden blaze, heats the plates, and warms the room (which with bad wood flowly kindling would not be done fo foon) and at the fame time, by the length of its flame, turning in the paffages, confumes and cleanfes away the foot that fuch bad fmoaky wood had produced therein the preceding day, and fo keeps them always free and clean.—When you have laid a little back log, and placed your billets on fmall dogs, as in common chimneys, and put fome fire to them, then flide down your fhutter as low as the dogs, and the opening being by that means contracted, the air rufhes in brifkly, and prefently blows up the flames. When the fire is fufficiently kindled, flide it up again *. In fome of thefe fire-places there is a little fix inch fquare trap-door of thin wrought iron or brafs, covering a hole of like dimenfions near the fore-part of the bottom-plate, which being by a ring lifted up towards the fire, about an inch, where it will be retained by two fpringing fides fixed to it perpendicularly, [*See the Plate, Fig.* 4,] the air rufhes in from the hollow under the bottom plate, and blows the fire. Where this is ufed, the fhutter ferves only to clofe the fire at nights. The more forward you can make your fire on the hearth-plate, not to be incommoded by the fmoke, the fooner and more will the room be warmed. At night when you go

* The fhutter is flid up and down in this manner, only in thofe fire-places which are fo made as that the diftance between the top of the arched opening, and the bottom plate, is the fame as the diftance between it and the top-plate. Where the arch is higher, as it is in the draught annexed, (which is agreeable to the laft improvements) the fhutter is fet by, and applied occafionally; becaufe if it were made deep enough to clofe the whole opening when flid down, it would hide part of it when up.

to

to bed, cover the coals or brands with aſhes as uſual; then take away the dogs, and ſlide down the ſhutter cloſe to the bottom-plate, ſweeping a little aſhes againſt it, that no air may paſs under it; then turn the regiſter, ſo as very near to ſtop the flue behind. If no ſmoke then comes out at crevices into the room, it is right: If any ſmoke is perceived to come out, move the regiſter ſo as to give a little draught, and it will go the right way.—Thus the room will be kept warm all night; for the chimney being almoſt entirely ſtopt, very little cold air, if any, will enter the room at any crevice. When you come to re-kindle the fire in the morning, turn open the regiſter before you lift up the ſlider, otherwiſe, if there be any ſmoke in the fire-place, it will come out into the room. By the ſame uſe of the ſhutter and regiſter, a blazing fire may be preſently ſtifled, as well as ſecured, when you have occaſion to leave it for any time; and at your return you will find the brands warm, and ready for a ſpeedy re-kindling. The ſhutter alone will not ſtifle a fire, for it cannot well be made to fit ſo exactly but that air will enter, and that in a violent ſtream, ſo as to blow up and keep alive the flames, and conſume the wood, if the draught be not check'd by turning the regiſter to ſhut the flue behind. The regiſter has alſo two other uſes. If you obſerve the draught of air into your fire-place to be ſtronger than is neceſſary, (as in extream cold weather it often is) ſo that the wood is conſumed faſter than uſual; in that caſe, a quarter, half, or two thirds turn of the regiſter, will check the violence of the

the draught, and let your fire burn with the moderation you defire: And at the fame time both the fire-place and the room will be the warmer, becaufe lefs cold air will enter and pafs through them.—And if the chimney fhould happen to take fire, (which indeed there is very little danger of, if the preceding direction be obferved in making fires, and it be well fwept once a year; for, much lefs wood being burnt, lefs foot is proportionably made; and the fuel being foon blown into flame by the fhutter (or the trap-door bellows) there is confequently lefs fmoke from the fuel to make foot; then, though the funnel fhould be foul, yet the fparks have fuch a crooked up and down round about way to go, that they are out before they get at it. I fay, if ever it fhould be on fire, a turn of the regifter fhuts all clofe, and prevents any air going into the chimney, and fo the fire may eafily be ftifled and maftered.

The Advantages of this FIRE-PLACE.

Its advantages above the common fire-places are,

1. That your whole room is equally warmed, fo that people need not croud fo clofe round the fire, but may fit near the window, and have the benefit of the light for reading, writing, needle-work, *&c.* They may fit with comfort in any part of the room, which is a very confiderable advantage in a large family, where there muft often be two fires kept, becaufe all cannot conveniently come at one.

2. If you fit near the fire, you have not that cold draught of uncomfortable air nipping your back and heels, as when before

before common fires, by which many catch cold, being scorched before, and, as it were, froze behind.

3. If you sit against a crevice, there is not that sharp draught of cold air playing on you as in rooms where there are fires in the common way; by which many catch cold, whence proceed coughs *, catarrhs, tooth-achs, fevers, pleurisies, and many other diseases.

4. In case of sickness they make most excellent nursing rooms; as they constantly supply a sufficiency of fresh air, so warmed at the same time as to be no way inconvenient or dangerous. A small one does well in a chamber; and, the chimneys being fitted for it, it may be removed from one room to another, as occasion requires, and fixed in half an hour. The equal temper, too, and warmth, of the air of the room, is thought to be particularly advantageous in some distempers; for it was observed in the winters of 1730 and 1736, when the small-pox spread in *Pennsylvania*, that very few children of the *Germans* died of that distemper in proportion to those of the *English*; which was ascribed, by some, to the warmth and equal temper of air in their stove-rooms, which made the disease as favourable as it commonly is in the *West-Indies*. But this conjecture we submit to the judgment of physicians.

* My Lord *Molesworth*, in his account of *Denmark*, says, "That "few or none of the people there, are troubled with coughs, catarrhs, "consumptions, or such like diseases of the lungs; so that in the midst "of winter in the churches, which are very much frequented, there is "no noise to interrupt the attention due to the preacher. I am persuad- "ed (says he) their *warm stoves* contribute to their freedom from these "kind of maladies." pag. 91.

5. In common chimneys, the ſtrongeſt heat from the fire, which is upwards, goes directly up the chimney, and is loſt; and there is ſuch a ſtrong draught into the chimney, that not only the upright heat, but alſo the back, ſides, and downward heats, are carried up the chimney by that draught of air; and the warmth given before the fire by the rays that ſtrike out towards the room, is continually driven back, crouded into the chimney, and carried up by the ſame draught of air. But here the upright heat ſtrikes and heats the top plate, which warms the air above it, and that comes into the room. The heat likewiſe, which the fire communicates to the ſides, back, bottom, and air-box, is all brought into the room; for you will find a conſtant current of warm air coming out of the chimney corner into the room. Hold a candle juſt under the mantle-piece, or breaſt of your chimney, and you will ſee the flame bent outwards: By laying a piece of ſmoaking paper on the hearth, on either ſide, you may ſee how the current of air moves, and where it tends, for it will turn and carry the ſmoke with it.

6. Thus as very little of the heat is loſt, when this fire-place is uſed, *much leſs wood** will ſerve you, which is a conſiderable advantage where wood is dear.

7. When

* People who have uſed theſe fire-places, differ much in their accounts of the wood ſaved by them. Some ſay five-ſixths, others three-fourths, and others much leſs. This is owing to the great difference there was in their former fires; ſome (according to the different circumſtances of their rooms and chimnies) having been uſed to make very large, others midd-

7. When you burn candles near this fire-place, you will find that the flame burns quite upright, and does not blare and run the tallow down, by drawing towards the chimney, as against common fires.

8. This fire-place cures most smoaky chimneys, and thereby preserves both the eyes and furniture.

9. It prevents the fouling of chimneys; much of the lint and dust that contributes to foul a chimney being, by the low arch, obliged to pass through the flame, where it is consumed. Then, less wood being burnt, there is less smoke made. Again, the shutter, or trap-bellows, soon blowing the wood into a flame, the same wood does not yield so much smoke as if burnt in a common chimney: For as soon as flame begins, smoke, in proportion, ceases.

10. And if a chimney should be foul, it is much less likely to take fire. If it should take fire, it is easily stifled and extinguished.

11. A fire may be very speedily made in this fire-place, by the help of the shutter, or trap-bellows, as aforesaid.

12. A fire may be soon extinguished, by closing it with the shutter before, and turning the register behind, which will stifle it, and the brands will remain ready to rekindle.

13. The room being once warm, the warmth may be retained in it all night.

middling, and others, of a more sparing temper, very small ones: While in these fire places (their size and draught being nearly the same) the consumption is more equal. I suppose, taking a number of families together, that two thirds, or half the wood, at least, is saved. My common room, I know, is made twice as warm as it used to be, with a quarter of the wood I formerly consumed there.

14. And, laftly, the fire is fo fecured at night, that not one fpark can fly out into the room to do damage.

With all thefe conveniencies, you do not lofe the pleafant fight nor ufe of the fire, as in the *Dutch* ftoves, but may boil the tea-kettle, warm the flat-irons, heat heaters, keep warm a difh of victuals, by fetting it on the top, *&c.*

OBJECTIONS *anfwered.*

There are fome objections commonly made by people that are unacquainted with thefe fire-places, which it may not be amifs to endeavour to remove, as they arife from prejudices which might otherwife obftruct, in fome degree, the general ufe of this beneficial machine. We frequently hear it faid, *They are of the nature of* Dutch *ftoves*; *ftoves have an unpleafant fmell*; *ftoves are unwholefome*; and, *warm rooms make people tender, and apt to catch cold.*—As to the firft, that they are of the nature of *Dutch* ftoves, the defcription of thofe ftoves, in the beginning of this paper, compared with that of thefe machines, fhows that there is a moft material difference, and that thefe have vaftly the advantage, if it were only in the fingle article of the admiffion and circulation of the frefh air. But it muft be allowed there may have been fome caufe to complain of the offenfive fmell of iron ftoves. This fmell, however, never proceeded from the iron itfelf, which, in its nature, whether hot or cold, is one of the fweeteft of metals, but from the general uncleanly manner of ufing thofe ftoves. If they are kept clean, they are as fweet as an

iron-

ironing-box, which, though ever ſo hot, never offends the ſmell of the niceſt Lady: But it is common to let them be greaſed, by ſetting candleſticks on them, or otherwiſe; to rub greaſy hands on them; and, above all, to ſpit upon them, to try how hot they are, which is an inconſiderate filthy unmannerly cuſtom; for the ſlimy matter of ſpittle drying on, burns and fumes when the ſtove is hot, as well as the greaſe, and ſmells moſt nauſeouſly, which makes ſuch cloſe ſtove-rooms, where there is no draught to carry off thoſe filthy vapours, almoſt intolerable to thoſe that are not from their infancy accuſtomed to them. At the ſame time nothing is more eaſy than to keep them clean; for when by any accident they happen to be fouled, a lee made of aſhes and water, with a bruſh, will ſcour them perfectly; as will alſo a little ſtrong ſoft ſoap and water.

That hot iron of itſelf gives no offenſive ſmell, thoſe know very well who have (as the writer of this has) been preſent at a furnace when the workmen were pouring out the flowing metal to caſt large plates, and not the leaſt ſmell of it to be perceived. That hot iron does not, like lead, braſs, and ſome other metals, give out unwholeſome vapours, is plain from the general health and ſtrength of thoſe who conſtantly work in iron, as furnace-men, forge-men, and ſmiths; that it is in its nature a metal perfectly wholeſome to the body of man, is known from the beneficial uſe of chalybeate or iron mine-waters; from the good done by taking ſteel filings in ſeveral diſorders; and that even the ſmithy water in which hot irons are quenched, is found advantageous to the human conſtitution.—The ingenious and learned Dr *Deſaguliers*, to whoſe inſtructive writings the contriver of this machine acknowledges himſelf much indebted, relates an experiment he made, to try whether heated iron would yield unwholeſome vapours: He took a cube of iron, and having given it a very great heat, he fixed it ſo to a receiver, exhauſted by the air-pump, that all the air ruſhing in to fill the receiver, ſhould firſt paſs through a hole in the hot iron. He then put a ſmall bird into the receiver, who breathed that air without any inconvenience, or ſuffering the leaſt diſorder. But the ſame experiment being made with a cube of hot braſs, a bird put into that air died in a few minutes. Braſs, indeed, ſtinks even when cold, and much more when hot; lead, too, when hot, yields a very unwholeſome ſteam; but iron is always ſweet, and every way taken is wholeſome and friendly to the human body—except in weapons.

That warm rooms make people tender, and apt to catch cold, is a miſtake as great as it is (among the *Engliſh*) general. We have ſeen in the preceding pages how the common rooms are apt to give colds; but the writer of this paper may affirm from his own experience, and that of his family and friends who have uſed warm rooms for theſe four winters paſt, that by the uſe of ſuch rooms, people are rendered *leſs liable* to take cold, and, indeed, *actually hardened.* If ſitting warm in a room made one ſubject to take cold on going out, lying warm in bed ſhould,

by

by a parity of reason, produce the same effect when we rise. Yet we find we can leap out of the warmest bed naked, in the coldest morning, without any such danger; and in the same manner out of warm cloaths into a cold bed. The reason is, that in these cases the pores all close at once, the cold is shut out, and the heat within augmented, as we soon after feel by the glowing of the flesh and skin. Thus no one was ever known to catch cold by the use of the cold bath: And are not cold baths allowed to harden the bodies of those that use them? Are they not therefore frequently prescribed to the tenderest constitutions? Now every time you go out of a warm room into the cold freezing air, you do as it were plunge into a cold bath, and the effect is in proportion the same; for (though perhaps you may feel somewhat chilly at first) you find in a little time your bodies hardened and strengthened, your blood is driven round with a brisker circulation, and a comfortable steady uniform inward warmth succeeds that equal outward warmth you first received in the room. Farther to confirm this assertion, we instance the *Swedes*, the *Danes*, and the *Russians*: These nations are said to live in rooms, compared to ours, as hot as ovens *; yet where are the hardy

* Mr *Boyle*, in his experiments and observations upon cold, *Shaw's Abridgement*, *Vol.* I. p. 684, says, " 'Tis remarkable, that while the cold " has strange and tragical effects at *Moscow*, and elsewhere, the *Russians* " and *Livonians* should be exempt from them, who accustom themselves " to pass immediately from a great degree of heat, to as great an one of " cold, without receiving any visible prejudice thereby. I remember be-" ing told by a person of unquestionable credit, that it was a common " practice

hardy ſoldiers, though bred in their boaſted cool houſes, that can, like theſe people, bear the fatigues of a winter campaign in ſo ſevere a climate, march whole days to the neck in ſnow, and at night entrench in ice, as they do?

The mentioning of thoſe Northern nations, puts me in mind of a conſiderable *public advantage* that may ariſe from the general uſe of theſe fire-places. It is obſervable, that though thoſe countries have been well inhabited for many ages, wood is ſtill their fuel, and yet at no very great price; which could not have been, if they had not univerſally uſed ſtoves, but conſumed it as we do, in great quantities, by open fires. By the help of this ſaving invention our wood may grow as faſt as we conſume it, and our poſterity may warm themſelves at a moderate rate, without being obliged to fetch their fuel over the *Atlantick*; as, if pit-coal ſhould not be here diſcovered, (which is an uncertainty) they muſt neceſſarily do.

We leave it to the *political arithmetician* to compute how much money will be ſaved to a country, by its ſpending two-thirds leſs of fuel; how much labour ſaved in cutting and carriage of it; how much more land may be cleared by cultivation; how great the profit by the additional quantity of work done, in thoſe trades particularly that

" practice among them, to go from a hot ſtove, into cold water; the ſame " was alſo affirmed to me by another who reſided at *Moſcow*. This tra- " dition is likewiſe abundantly confirmed by *Olearius* " '*Tis a ſurprizing* " *thing*, ſays he, *to ſee how far the* Ruſſians *can endure heat; and how,* " *when it makes them ready to faint, they can go out of their ſtoves, ſtark* " *naked, both men and women, and throw themſelves into cold water; and* " *even in winter wallow in the ſnow.*"

do

do not exercise the body so much, but that the workfolks are obliged to run frequently to the fire to warm themselves: And to physicians to say, how much healthier thick-built towns and cities will be, now half suffocated with sulphury smoke, when so much less of that smoke shall be made, and the air breathed by the inhabitants be consequently so much purer. These things it will suffice just to have mentioned; let us proceed to give some necessary directions to the workman who is to fix or set up these fire-places.

DIRECTIONS *to the* BRICKLAYER.

The chimney being first well swept and cleansed from soot, *&c.* lay the bottom plate down on the hearth, in the place where the fire-place is to stand, which may be as forward as the hearth will allow. Chalk a line from one of its back corners round the plate to the other corner, that you may afterwards know its place when you come to fix it; and from those corners, two parallel lines to the back of the chimney: Make marks also on each side, that you may know where the partition is to stand, which is to prevent any communication between the air and smoke. Then removing the plate, make a hollow under it and beyond it, by taking up as many of the bricks or tiles as you can, within your chalked lines, quite to the chimney back. Dig out six or eight inches deep of the earth or rubbish, all the breadth and length of your hollow; then make a passage of four inches square (if the place will allow so much)

much) leading from the hollow to ſome place communicating with the outer air; by *outer air* we mean air without the room you intend to warm. This paſſage may be made to enter your hollow on either ſide, or in the fore part, juſt as you find moſt convenient, the circumſtances of your chimney conſidered. If the fire-place is to be put up in a chamber, you may have this communication of outer air from the ſtair-caſe; or ſometimes more eaſily from between the chamber floor, and the cieling of the lower room, making only a ſmall hole in the wall of the houſe entering the ſpace betwixt thoſe two joiſts with which your air-paſſage in the hearth communicates. If this air-paſſage be ſo ſituated as that mice may enter it, and neſtle in the hollow, a little grate of wire will keep them out. This paſſage being made, and, if it runs under any part of the earth, tiled over ſecurely, you may proceed to raiſe your falſe back. This may be of four inches or two inches thickneſs, as you have room, but let it ſtand at leaſt four inches from the true chimney-back. In narrow chimnies this falſe back runs from jamb to jamb, but in large old faſhioned chimnies, you need not make it wider than the back of the fire-place. To begin it, you may form an arch nearly flat, of three bricks end to end, over the hollow, to leave a paſſage the breadth of the iron fire-place, and five or ſix inches deep, rounding at bottom, for the ſmoke to turn and paſs under the falſe back, and ſo behind it up the chimney. The falſe back is to riſe till it is as high as the breaſt of the chimney, and then to cloſe over

to

to the breaſt *; always obſerving, if there is a wooden mantle-tree, to cloſe above it. If there is no wood in the breaſt, you may arch over and cloſe even with the lower part of the breaſt. By this cloſing the chimney is made tight, that no air or ſmoke can paſs up it, without going under the falſe back. Then from ſide to ſide of your hollow, againſt the marks you made with chalk, raiſe a tight partition, brick-on-edge, to ſeparate the air from the ſmoke, bevelling away to half an inch the brick that comes juſt under the air-hole, that the air may have a free paſſage up into the air-box: Laſtly, cloſe the hearth over that part of the hollow that is between the falſe back and the place of the bottom plate, coming about half an inch under the plate, which piece of hollow hearth may be ſupported by a bit or two of old iron hoop; then is your chimney fitted to receive the fire-place.

To ſet it, lay firſt a little bed of mortar all round the edges of the hollow, and over the top of the partition: Then lay down your bottom plate in its place (with the rods in it) and tread it till it lies firm. Then put a little fine mortar (made of loam and lime with a little hair) into its joints, and ſet in your back plate, leaning it for the preſent againſt the falſe back: Then ſet in your air-box, with a little mortar in its joints: Then put in the two ſides, cloſing them up againſt the air-box with mortar in their grooves, and fixing at the ſame time your regiſter: Then bring up

* See pag. 302, where the trap-door is deſcribed that ought to be in this cloſing.

your back to its place, with mortar in its grooves, and that will bind the ſides together. Then put in your front plate, placing it as far back in the groove as you can, to leave room for the ſliding plate: Then lay on your top plate, with mortar in its grooves alſo, ſcrewing the whole firmly together by means of the rods. The capital letters A B D E, *&c.* in the annexed cut, ſhew the correſponding parts of the ſeveral plates. Laſtly, the joints being pointed all round on the outſide, the fire-place is fit for uſe.

When you make your firſt fire in it, perhaps if the chimney be thoroughly cold, it may not draw, the work too being all cold and damp. In ſuch caſe, put firſt a few ſhovels of hot coals in the fire-place, then lift up the chimney-ſweeper's trap-door, and putting in a ſheet or or two of flaming paper, ſhut it again, which will ſet the chimney a drawing immediately, and when once it is filled with a column of warm air, it will draw ſtrongly and continually.

The drying of the mortar and work by the firſt fire, may ſmell unpleaſantly, but that will ſoon be over.

In ſome ſhallow chimneys, to make more room for the falſe back and its flue, four inches or more of the chimney back may be picked away.

Let the room be made as tight as conveniently it may be, ſo will the outer air that muſt come in to ſupply the room and draught of the fire, be all obliged to enter thro' the paſſage under the bottom-plate, and up through the air-box, by which means it will not come cold to your backs,

backs, but be warmed as it comes in, and mixed with the warm air round the fire-place before it ſpreads in the room.

But as a great quantity of cold air, in extreme cold weather eſpecially, will preſently enter a room if the door be careleſly left open, it is good to have ſome contrivance to ſhut it, either by means of ſcrew hinges, a ſpring, or a pulley.

When the pointing in the joints is all dry and hard, get ſome powder of black-lead, (broken bits of black-lead crucibles from the ſilver-ſmiths, pounded fine, will do) and mixing it with a little rum and water, lay it on, when the plates are warm, with a hard bruſh, over the top and front-plates, part of the ſide and bottom-plates, and over all the pointing; and, as it dries, rub it to a gloſs with the ſame bruſh, ſo the joints will not be diſcerned, but it will look all of a piece, and ſhine like new iron. And the falſe back being plaiſter'd and white-waſh'd, and the hearth redden'd, the whole will make a pretty appearance. Before the black-lead is laid on, it would not be amiſs to waſh the plates with ſtrong lee and a bruſh, or ſoap and water, to cleanſe them from any ſpots of greaſe or filth that may be on them. If any greaſe ſhould afterwards come on them, a little wet aſhes will get it out.

If it be well ſet up, and in a tolerable good chimney, ſmoke will draw in from as far as the fore part of the bottom plate, as you may try by a bit of burning paper.

People are at firſt apt to make their rooms too warm, not imagining how little a fire will be ſufficient. When the plates are no hotter than that one may juſt bear the hand on them, the room will generally be as warm as you deſire it.

Soon after the foregoing piece was publiſhed, ſome perſons in England, *in imitation of Mr* Franklin's *invention, made what they call* Penſylvania Fire-places, with improvements; *the principal of which pretended improvements is a contraction of the paſſages in the air-box originally deſigned for admitting a quantity of freſh air, and warming it as it entered the room. The contracting theſe paſſages, gains indeed more room for the grate, but in a great meaſure defeats their intention. For if the paſſages in the air-box do not greatly exceed in dimenſions the amount of all the crevices by which cold air can enter the room, they will not conſiderably prevent, as they were intended to do, the entry of cold air through theſe crevices.*

LET-

LETTER XXIV.

FROM

BENJAMIN FRANKLIN, *Eſq* of *Philadelphia*,

TO

Dr *L*——, at *Charles-Town*, *South-Carolina*.

SIR, *Philadelphia*, *March* 18, 1755.

I SEND you encloſed a paper containing ſome new experiments I have made, in purſuance of thoſe by Mr *Canton* that are printed with my laſt letters. I hope theſe, with my explanation of them, will afford you ſome entertainment *.

In anſwer to your ſeveral enquiries. The tubes and globes we uſe here, are chiefly made here. The glaſs has a greeniſh caſt, but is clear and hard, and, I think, better for electrical experiments than the white glaſs of *London*, which is not ſo hard. There are certainly great differences in glaſs. A white globe I had made here ſome years ſince, would never, by any means, be excited. Two of my friends tried it, as well as myſelf, without ſucceſs. At length

* See page 155, for the paper here mentioned.

length, putting it on an electric ſtand, a chain from the prime-conductor being in contact with it, I found it had the properties of a non-electric; for I could draw ſparks from any part of it, though it was very clean and dry.

All I know of *Domien*, is, that by his own account he was a native of *Tranſylvania*, of *Tartar* deſcent, but a Prieſt of the *Greek* church: He ſpoke and wrote *Latin* very readily and correctly. He ſet out from his own country with an intention of going round the world, as much as poſſible by land. He travelled through *Germany*, *France*, and *Holland*, to *England*. Reſided ſome time at *Oxford*. From *England* he came to *Maryland*; thence went to *New-England*; returned by land to *Philadelphia*; and from hence travelled through *Maryland*, *Virginia*, and *North-Carolina* to you. He thought it might be of ſervice to him in his travels to know ſomething of Electricity. I taught him the uſe of the tube; how to charge the *Leyden* phial, and ſome other experiments. He wrote to me from *Charles-Town*, that he had lived eight hundred miles upon Electricity, it had been meat, drink, and cloathing to him. His laſt letter to me was, I think, from *Jamaica*, deſiring me to ſend the tubes you mention, to meet him at the *Havanah*, from whence he expected to get a paſſage to *La Vera Cruz*; deſigned travelling over land through *Mexico* to *Acapulco*; thence to get a paſſage to *Manilla*, and ſo through *China*, *India*, *Perſia*, and *Turkey*, home to his own country; propoſing to ſupport himſelf chiefly by Electricity. A ſtrange project! But he was, as you ob-
ſerve,

ſerve, a very ſingular character. I was ſorry the tubes did not get to the *Havanah* in time for him: If they are ſtill in being, pleaſe to ſend for them, and accept of them. What became of him afterwards I have never heard. He promiſed to write to me as often as he could on his journey, and as ſoon as he ſhould get home after finiſhing his tour. It is now ſeven years ſince he was here. If he is ſtill in *New Spain*, as you imagine from that looſe report, I ſuppoſe it muſt be that they confine him there, and prevent his writing: but I think it more likely that he may be dead.

The queſtions you aſk about the pores of glaſs, I cannot anſwer otherwiſe, than that I know nothing of their nature; and ſuppoſitions, however ingenious, are often mere miſtakes. My hypotheſis, that they were ſmaller near the middle of the glaſs, too ſmall to admit the paſſage of Electricity, which could paſs through the ſurface till it came near the middle, was certainly wrong: For ſoon after I had written that letter, I did, in order to *confirm* the hypotheſis, (which indeed I ought to have done before I wrote it) make an experiment. I ground away five-ſixths of the thickneſs of the glaſs, from the ſide of one of my phials, expecting that the ſuppoſed denſer part being ſo removed, the electric fluid might come through the remainder of the glaſs, which I had imagined more open; but I found myſelf miſtaken. The bottle charged as well after the grinding as before. I am now, as much as ever, at a loſs to know how, or where the quantity of electric fluid, on the poſitive ſide of the glaſs, is diſpoſed of.

As

As to the difference of conductors, there is not only this, that ſome will conduct Electricity in ſmall quantities, and yet do not conduct it faſt enough to produce the ſhock; but even among thoſe that will conduct a ſhock, there are ſome that do it better than others. Mr *Kinnerſley* has found, by a very good experiment, that when the charge of a bottle hath an opportunity of paſſing two ways, *i. e.* ſtrait through a trough of water ten feet long, and ſix inches ſquare; or round about through twenty feet of wire, it paſſes through the wire, and not through the water, though that is the ſhorteſt courſe; the wire being the better conductor. When the wire is taken away, it paſſes through the water, as may be felt by a hand plunged in the water; but it cannot be felt in the water when the wire is uſed at the ſame time. Thus, though a ſmall vial containing water will give a ſmart ſhock, one containing the ſame quantity of mercury will give one much ſtronger, the mercury being the better conductor; while one containing oil only, will ſcarce give any ſhock at all.

Your queſtion, how I came firſt to think of propoſing the experiment of drawing down the lightning, in order to aſcertain its ſameneſs with the electric fluid, I cannot anſwer better than by giving you an extract from the minutes I uſed to keep of the experiments I made, with memorandums of ſuch as I purpoſed to make, the reaſons for making them, and the obſervations that aroſe upon them, from which minutes my letters were afterwards drawn.

By

By this extract you will see that the thought was not so much "an out-of-the-way one," but that it might have occurred to any electrician.

"*Nov.* 7, 1749. Electrical fluid agrees with lightning "in these particulars: 1. Giving light. 2. Colour of the "light. 3. Crooked direction. 4. Swift motion. "5. Being conducted by metals. 6. Crack or noise in "exploding. 7. Subsisting in water or ice. 8. Rend-"ing bodies it passes through. 9. Destroying animals. "10. Melting metals. 11. Firing inflammable sub-"stances. 12. Sulphureous smell.—The electric fluid is "attracted by points.—We do not know whether this "property is in lightning.—But since they agree in all "the particulars wherein we can already compare them, "is it not probable they agree likewise in this?—Let the "experiment be made."

I wish I could give you any satisfaction in the article of clouds. I am still at a loss about the manner in which they become charged with Electricity; no hypothesis I have yet formed perfectly satisfying me. Some time since, I heated very hot a brass plate, two feet square, and placed it on an electric stand. From the plate a wire extended horizontally four or five feet, and, at the end of it, hung, by linnen threads, a pair of cork balls. I then repeatedly sprinkled water over the plate, that it might be raised from it in vapour, hoping that if the vapour either carried off the electricity of the plate, or left behind it that of the water, (one of which I supposed it must do, if, like the clouds, it became electrifed itfelf, either pofitively or negatively) I fhould perceive and determine it by the feparation of the balls, and by finding whether they were pofitive or negative; but no alteration was made at all, nor could I perceive that the fteam was itfelf electrifed, though I have ftill fome fufpicion that the fteam was not fully examined, and I think the experiment fhould be repeated. Whether the firft ftate of electrifed clouds is pofitive or negative, if I could find the caufe of that, I fhould be at no lofs about the other, for either is eafily deduced from the other, as one ftate is eafily produced by the other. A ftrongly pofitive cloud may drive out of a neighbouring cloud much of its natural quantity of the electric fluid, and, paffing by it, leave it in a negative ftate. In the fame way, a ftrongly negative cloud may occafion a neighbouring cloud to draw into itfelf from others, an additional quantity, and, paffing by it, leave it in a pofitive ftate. How thefe effects may be produced, you will eafily conceive, on perufing and confidering the experiments in the enclofed paper: And from them too it appears probable, that every change from pofitive to negative, and from negative to pofitive, that, during a thunder guft, we fee in the cork-balls annexed to the apparatus, is not owing to the prefence of clouds in the fame ftate, but often to the abfence of pofitive or negative clouds, that, having juft paffed, leave the rod in the oppofite ftate.

The knocking down of the fix men was performed with two of my large jarrs not fully charged. I laid one end of

my

my discharging rod upon the head of the first; he laid his hand on the head of the second; the second his hand on the head of the third, and so to the last, who held, in his hand, the chain that was connected with the outside of the jarrs. When they were thus placed, I applied the other end of my rod to the prime-conductor, and they all dropt together. When they got up, they all declared they had not felt any stroke, and wondered how they came to fall; nor did any of them either hear the crack, or see the light of it. You suppose it a dangerous experiment; but I had once suffered the same myself, receiving, by accident, an equal stroke through my head, that struck me down, without hurting me: And I had seen a young woman that was about to be electrified through the feet, (for some indisposition) receive a greater charge through the head, by inadvertently stooping forward to look at the placing of her feet, till her forehead (as she was very tall) came too near my prime-conductor: She dropt, but instantly got up again, complaining of nothing. A person so struck, sinks down doubled, or folded together as it were, the joints losing their strength and stiffness at once, so that he drops on the spot where he stood, instantly, and there is no previous staggering, nor does he ever fall lengthwise. Too great a charge might, indeed, kill a man, but I have not yet seen any hurt done by it. It would certainly, as you observe, be the easiest of all deaths.

The experiment you have heard so imperfect an account of, is merely this.—I electrified a silver pint cann, on an electric ſtand, and then lowered into it a cork ball, of about an inch diameter, hanging by a ſilk ſtring, till the cork touched the bottom of the cann. The cork was not attracted to the inſide of the cann as it would have been to the outſide, and though it touched the bottom, yet, when drawn out, it was not found to be electrified by that touch, as it would have been by touching the outſide. The fact is ſingular. You require the reaſon; I do not know it. Perhaps you may diſcover it, and then you will be ſo good as to communicate it to me *. I find a frank acknowledgment of one's ignorance is not only the eaſieſt way to get rid of a difficulty, but the likelieſt way to obtain information, and therefore I practice it: I think it an honeſt policy. Thoſe who affect to be thought to know every thing, and ſo undertake to explain every thing, often remain long ignorant of many things that others could and would inſtruct them in, if they appeared leſs conceited.

The treatment your friend has met with is ſo common, that no man who knows what the world is, and ever has been, ſhould expect to eſcape it. There are every where a number of people, who, being totally deſtitute of any inventive faculty themſelves, do not readily conceive that others may poſſeſs it: They think of inventions as of miracles; there might be ſuch formerly, but they are ceaſed.

* Mr *F.* has ſince thought, that, poſſibly, the mutual repulſion of the inner oppoſite ſides of the electrifed cann, may prevent the accumulating an electric atmoſphere upon them, and occaſion it to ſtand chiefly on the outſide. But recommends it to the farther examination of the curious.

With

With theſe, every one who offers a new invention is deem'd a pretender: He had it from ſome other country, or from ſome book: A man of *their own acquaintance*; one who has no more ſenſe than themſelves, could not poſſibly, in their opinion, have been the inventer of any thing. They are confirmed, too, in theſe ſentiments, by frequent inſtances of pretenſions to invention, which vanity is daily producing. That vanity too, though an incitement to invention, is, at the ſame time, the peſt of inventors. Jealouſy and Envy deny the merit or the novelty of your invention; but Vanity, when the novelty and merit are eſtabliſhed, claims it for its own. The ſmaller your invention is, the more mortification you receive in having the credit of it diſputed with you by a rival, whom the jealouſy and envy of others are ready to ſupport againſt you, at leaſt ſo far as to make the point doubtful. It is not in itſelf of importance enough for a diſpute; no one would think your proofs and reaſons worth their attention: And yet if you do not diſpute the point, and demonſtrate your right, you not only loſe the credit of being in that inſtance *ingenious*, but you ſuffer the diſgrace of not being *ingenuous*; not only of being a plagiary, but of being a plagiary for trifles. Had the invention been greater it would have diſgrac'd you leſs; for men have not ſo contemptible an idea of him that robs for gold on the highway, as of him that can pick pockets for half-pence and farthings. Thus through Envy, Jealouſy, and the Vanity of competitors for Fame, the origin of many of the moſt extraordinary inventions,

tions, though produced within but a few centuries paſt, is involved in doubt and uncertainty. We ſcarce know to whom we are indebted for the *compaſs*, and for *ſpectacles*, nor have even *paper* and *printing*, that record every thing elſe, been able to preſerve with certainty the name and reputation of their inventors. One would not, therefore, of all faculties, or qualities of the mind, wiſh, for a friend, or a child, that he ſhould have that of invention. For his attempts to benefit mankind in that way, however well imagined, if they do not ſucceed, expoſe him, though very unjuſtly, to general ridicule and contempt; and, if they do ſucceed, to envy, robbery, and abuſe.

I am, &c.

B. F.

LET-

LETTER XXV.

FROM

R. J. *Esq*; of *London*,

TO

BENJ. FRANKLIN, *Esq*; of *Philadelphia*.

DEAR SIR,

IT is now near three years since I received your excellent *Observations on the Increase of Mankind, &c**. in which you have with so much sagacity and accuracy shewn in what manner, and by what causes, that principal means of political grandeur is best promoted; and have so well supported those just inferences you have occasionally drawn, concerning the general state of our *American* colonies, and the views and conduct of some of the inhabitants of *Great-Britain*.

You have abundantly proved that natural fecundity is hardly to be considered, because the *vis generandi*, as far as we know, is unlimited, and because experience shews that the numbers of nations is altogether governed by collateral causes, and among these none of so much force as

* See page 197.

quan-

quantity of ſubſiſtence, whether ariſing from climate, ſoil, improvement of tillage, trade, fiſheries, ſecure property, conqueſt of new countries, or other favourable circumſtances.

As I perfectly concurred with you in your ſentiments on theſe heads, I have been very deſirous of building ſomewhat on the foundation you have there laid; and was induced by your hints in the twenty-firſt ſection, to trouble you with ſome thoughts on the influence manners have always had, and are always likely to have on the numbers of a people, and their political proſperity in general.

The end of every individual is its own private good. The rules it obſerves in the purſuit of this good, are a ſyſtem of propoſitions, almoſt every one founded in authority, that is, derive their weight from the credit given to one or more perſons, and not from demonſtration.

And this, in the moſt important as well as the other affairs of life, is the caſe even of the wiſeſt and philoſophical part of the human ſpecies; and that it ſhould be ſo is the leſs ſtrange, when we conſider that it is, perhaps, impoſſible to prove, that *being*, or life itſelf, has any other value than what is ſet on it by authority.

A confirmation of this may be derived from the obſervation, that in every country in the univerſe, happineſs is ſought upon a different plan; and, even in the ſame country, we ſee it placed by different ages, profeſſions, and ranks of men, in the attainment of enjoyments utterly unlike.

These propositions, as well as others, framed upon them, become habitual by degrees, and, as they govern the determination of the will, I call them *moral habits.*

There are another set of habits that have the direction of the members of the body, that I call therefore *mechanical habits.* These compose what we commonly call *The Arts*, which are more or less liberal or mechanical, as they more or less partake of assistance from the operations of the mind.

The *cumulus* of the moral habits of each individual, is the manners of that individual; the *cumulus* of the manners of individuals makes up the manners of a nation.

The happiness of individuals is evidently the ultimate end of political society; and political welfare, or the strength, splendour, and opulence of the state, have been always admitted, both by political writers, and the valuable part of mankind in general, to conduce to this end, and are therefore desirable.

The causes that advance or obstruct any one of these three objects, are external or internal. The latter may be divided into physical, civil, and personal, under which last head I comprehend the moral and mechanical habits of mankind. The physical causes are principally climate, soil, and number of subjects; the civil are government and laws; and political welfare is always in a ratio composed of the force of these particular causes; a multitude of external causes, and all these internal ones, not only controul and qualify, but are constantly acting on, and thereby inſenſibly, as well as ſenſibly, altering one another, both for the better and the worſe, and this not excepting the climate itſelf.

The powerful efficacy of manners in increaſing a people, is manifeſt from the inſtance you mention, the Quakers; among them induſtry and frugality multiplies and extends the uſe of the neceſſaries of life; to manners of a like kind are owing the populouſneſs of *Holland*, *Switzerland*, *China*, *Japan*, moſt parts of *Indoſtan*, *&c.* in every one of which the force of extent of territory and fertility of ſoil is multiplied, or their want compenſated by induſtry and frugality.

Neither nature nor art have contributed much to the production of ſubſiſtence in *Switzerland*, yet we ſee frugality preſerves, and even increaſes families that live on their fortunes, and which, in *England*, we call the Gentry; and the obſervation we cannot but make in the Southern part of this kingdom, that thoſe families, including all ſuperior ones, are gradually becoming extinct, affords the cleareſt proof that luxury (that is, a greater expence of ſubſiſtence than in prudence a man ought to conſume) is as deſtructive as a proportionable want of it; but in *Scotland*, as in *Switzerland*, the Gentry, though one with another they have not one-fourth of the income, increaſe in number.

And here I cannot help remarking, by the by, how well founded your diſtinction is between the increaſe of mankind in old and new ſettled countries in general, and more particularly in the caſe of families of condition. In

America

America, where their expences are more confined to necessaries, and those necessaries are cheap, it is common to see above one hundred persons descended from one living old man. In *England* it frequently happens, where a man has seven, eight, or more children, there has not been a descendant in the next generation, occasioned by the difficulties the number of children has brought on the family, in a luxurious dear country, and which have prevented their marrying.

That this is more owing to luxury than meer want, appears from what I have said of *Scotland*, and more plainly from parts of *England* remote from *London*, in most of which the necessaries of life are nearly as dear, in some dearer than in *London*, yet the people of all ranks marry and breed up children.

Again; among the lower ranks of life, none produce so few children as servants. This is, in some measure, to be attributed to their situation, which hinders marriage, but it is also to be attributed to their luxury, and corruption of manners, which are greater than among any other set of people in *England*, and is the consequence of a nearer view of the lives and persons of a superior rank, than any inferior rank, without a proper education, ought to have.

The quantity of subsistence in *England* has unquestionably become greater for many ages; and yet if the inhabitants are more numerous, they certainly are not so in proportion to our improvement of the means of support. I am apt to think there are few parts of this kingdom that have not been at some former time more populous than at present. I have several cogent reasons for thinking so, of great part of the counties I am most intimately acquainted with; but as they were probably not all most populous at the same time, and as some of our towns are visibly and vastly grown in bulk, I dare not suppose, as judicious men have done, that *England* is less peopled than heretofore.

This growth of our towns is the effect of a change of manners, and improvement of arts, common to all *Europe*; and though it is not imagined that it has lessened the country growth of necessaries, it has evidently, by introducing a greater consumption of them, (an infallible consequence of a nation's dwelling in towns) counteracted the effects of our prodigious advances in the arts.

But however frugality may supply the place of, or prodigality counteract the effects of the natural or acquired subsistence of a country, industry is, beyond doubt, a more efficacious cause of plenty than any natural advantage of extent or fertility. I have mentioned instances of frugality and industry united with extent and fertility; in *Spain* and *Asia* minor, we see frugality joined to extent and fertility, without industry; in *Ireland* we once saw the same; *Scotland* had then none of them but frugality. The change in these two countries is obvious to every one, and it is owing to industry not yet very widely diffused in either.

The effects of industry and frugality in *England* are surprizing; both the rent and the value of the inheritance of land depend on them greatly more than on nature, and

this

this though there is no considerable difference in the prices of our markets. Land of equal goodness lets for double the rent of other land lying in the same county, and there are many years purchase difference between different counties, where rents are equally well paid and secure.

Thus manners operate upon the number of inhabitants, but of their silent effects upon a civil constitution, history and even our own experience, yields us abundance of proofs, though they are not uncommonly attributed to external causes: Their support of a government against external force is so great, that it is a common maxim among the advocates of liberty, that no free government was ever dissolved, or overcome, before the manners of its subjects were corrupted.

The superiority of *Greece* over *Persia*, was singly owing to their difference of manners; and that, though all natural advantages were on the side of the latter, to which I might add the civil ones; for though the greatest of all civil advantages, Liberty, was on the side of *Greece*, yet that added no political strength to her, than as it operated on her manners, and, when they were corrupted, the restoration of their liberty by the *Romans*, overturned the remains of their power.

Whether the manners of ancient *Rome* were, at any period, calculated to promote the happiness of individuals, it is not my design to examine; but that their manners, and the effects of those manners on their government, and pub-

lick

lick conduct, founded, enlarged, and supported, and afterwards overthrew their empire, is beyond all doubt. One of the effects of their conquest furnishes us with a strong proof how prevalent manners are, even beyond quantity of subsistence; for, when the custom of bestowing on the citizens of *Rome* corn enough to support themselves and families, was become established, and *Egypt* and *Sicily* produced the grain that fed the inhabitants of *Italy*, this became less populous every day, and the *Jus trium liberorum* was but an expedient that could not balance the want of industry and frugality.

But corruption of manners did not only thin the inhabitants of the *Roman* empire, it rendered the remainder incapable of defence, long before its fall, perhaps before the dissolution of the Republic: so that without standing disciplined armies composed of men, whose moral habits principally, and mechanical habits secondarily, made them different from the body of the people, the *Roman* empire had been a prey to the Barbarians many ages before it was.

By the mechanical habits of the soldiery, I mean their discipline, and the art of war; and that this is but a secondary quality, appears from the inequality that has in all ages been between raw, though well disciplined armies, and veterans, and more from the irresistible force of a single moral habit, Religion, has conferred on troops frequently neither disciplined nor experienced.

The military manners of the Noblesse in *France*, compose the chief force of that kingdom, and the enterprizing man-

manners, and restless dispositions of the inhabitants of *Canada* have enabled a handful of men to harrass our populous, and, generally, less martial colonies; yet neither are of the value they seem at first sight, because, overbalanced by the defect they occasion of other habits that would produce more eligible political good: And military manners in a people are not necessary in an age and country where such manners may be occasionally formed and preserved among men enough to defend the state; and such a country is *Great-Britain*, where, though the lower class of people are by no means of a military cast, yet they make better soldiers than even the Noblesse of *France*.

The inhabitants of this country, a few ages back, were to the populous and rich provinces of *France*, what *Canada* is now to the *British* colonies. It is true, there was less disproportion between their natural strength; but I mean that the riches of *France* were a real weakness opposed to the military manners founded upon poverty and a rugged disposition, then the character of the *English*; but it must be remembered, that at this time the manners of a people were not distinct from that of their soldiery, for the use of standing armies has deprived a military people of the advantages they before had over others; and though it has been often said, that civil wars give power, because they render all men soldiers, I believe this has only been found true in internal wars, following civil wars, and not in external ones; for now, in foreign wars, a small army with ample means to support it, is of greater force than one

one more numerous, with leſs. This laſt fact has often happened between *France* and *Germany*.

The means of ſupporting armies, and, conſequently, the power of exerting external ſtrength, are beſt found in the induſtry and frugality of the body of a people living under a government and laws that encourage commerce, for commerce is at this day almoſt the only *ſtimulus* that forces every one to contribute a ſhare of labour for the publick benefit.

But ſuch is the human frame, and the world is ſo conſtituted, that it is a hard matter to poſſeſs ones ſelf of a benefit, without laying ones ſelf open to a loſs on ſome other ſide; the improvements of manners of one ſort, often deprave thoſe of another: Thus we ſee induſtry and frugality under the influence of commerce, which I call a commercial ſpirit, tend to deſtroy, as well as ſupport, the government it flouriſhes under.

Commerce perfects the arts, but more the mechanical than the liberal, and this for an obvious reaſon; it ſoftens and enervates the manners. Steady virtue, and unbending integrity, are ſeldom to be found where a ſpirit of commerce pervades every thing; yet the perfection of commerce is, that every thing ſhould have its price. We every day ſee its progreſs, both to our benefit and detriment here. Things that *boni mores* are forbid to be ſet to ſale, are become its objects, and there are few things indeed *extra commercium*. The legiſlative power itſelf has been *in commercio*, and church-livings are ſeldom given without con-

conſideration, even by ſincere Chriſtians, and for conſideration not ſeldom to very unworthy perſons. The rudeneſs of ancient military times, and the fury of more modern enthuſiaſtic ones, are worn off; even the ſpirit of forenſic contention is aſtoniſhingly diminiſhed, all marks of manners ſoftening; but Luxury and Corruption have taken their places, and ſeem the inſeparable companions of Commerce and the Arts.

I cannot help obſerving, however, that this is much more the caſe in extenſive countries, eſpecially at their metropolis, than in other places. It is an old obſervation of politicians, and frequently made by hiſtorians, that ſmall ſtates always beſt preſerve their manners; whether this happens from the greater room there is for attention in the legiſlature, or from the leſs room there is for Ambition and Avarice, it is a ſtrong argument, among others, againſt an incorporating union of the colonies in *America*, or even a federal one, that may tend to the future reducing them under one government.

Their power, while united, is leſs, but their liberty, as well as manners, is more ſecure; and, conſidering the little danger of any conqueſt to be made upon them, I had rather they ſhould ſuffer ſomething through diſunion, than ſee them under a general adminiſtration leſs equitable than that concerted at *Albany*.

I take it, the inhabitants of *Pennſylvania* are both frugal and induſtrious beyond thoſe of any province in *America*. If luxury ſhould ſpread, it cannot be extirpated by laws.

We are told by *Plutarch*, that *Plato* uſed to ſay, *It was a hard thing to make laws for the* Cyrenians, *a people abounding in plenty and opulence.*

But from what I ſet out with, it is evident, if I be not miſtaken, that education only can ſtem the torrent, and without checking either true induſtry or frugality, prevent the ſordid frugality and lazineſs of the old *Iriſh*, and many of the modern *Scotch*, (I mean the inhabitants of that country, thoſe who leave it for another being generally induſtrious) or the induſtry, mixed with luxury, of this capital, from getting ground, and by rendering ancient manners familiar, produce a reconciliation between diſintereſtedneſs and commerce; a thing we often ſee, but almoſt always in men of a liberal education.

To conclude; when we would form a people, ſoil and climate may be found, at leaſt ſufficiently good: Inhabitants may be encouraged to ſettle, and even ſupported for a while; a good government and laws may be framed, and even arts may be eſtabliſhed, or their produce imported; but many neceſſary moral habits are hardly ever found among thoſe who voluntarily offer themſelves in times of quiet at home, to people new colonies; beſides that the moral, as well as mechanical habits, adapted to a mother country, are frequently not ſo to the new ſettled one, and to external events, many of which are always unforeſeen. Hence it is we have ſeen ſuch fruitleſs attempts to ſettle colonies, at an immenſe public and private expence, by ſeveral of the powers of *Europe*: And it is particularly obſervable

vable that none of the *English* colonies became any way considerable, till the necessary manners were born and grew up in the county, excepting those to which singular circumstances at home forced manners fit for the forming a new state.

I am, Sir, &c. R. J.

LETTER XXVI.

FROM

BENJAMIN FRANKLIN, *Esq*; of *Philadelphia*,

TO

Dr *L*——, at *Charles-Town*, *South-Carolina*.

SIR, *New-York*, *April* 14, 1757.

IT is a long time since I had the pleasure of a line from you; and, indeed, the troubles of our country, with the hurry of business I have been engaged in on that account, have made me so bad a correspondent, that I ought not to expect punctuality in others.

But being about to embark for *England*, I could not quit the Continent without paying my respects to you, and, at the same time, taking leave to introduce to your acquaintance a Gentleman of learning and merit, Colonel

Henry Bouquet, who does me the favour to preſent you this letter, and with whom I am ſure you will be much pleaſed.

Profeſſor *Simpſon*, of *Glaſgow*, lately communicated to me ſome curious experiments of a phyſician of his acquaintance, by which it appeared, that an extraordinary degree of cold, even to freezing, might be produced by evaporation. I have not had leiſure to repeat and examine more than the firſt and eaſieſt of them, *viz.*—Wet the ball of a thermometer by a feather dipt in ſpirit of wine, which has been kept in the ſame room, and has, of courſe, the ſame degree of heat or cold. The mercury ſinks preſently three or four degrees, and the quicker, if, during the evaporation, you blow on the ball with bellows; a ſecond wetting and blowing, when the mercury is down, carries it yet lower. I think I did not get it lower than five or ſix degrees from where it naturally ſtood, which was, at that time, ſixty. But it is ſaid, that a veſſel of water being placed in another ſomewhat larger, containing ſpirit, in ſuch a manner that the veſſel of water is ſurrounded with the ſpirit, and both placed under the receiver of an air-pump; on exhauſting the air, the ſpirit, evaporating, leaves ſuch a degree of cold as to freeze the water, though the thermometer, in the open air, ſtands many degrees above the freezing point.

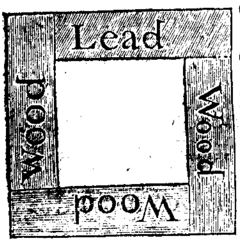
I know not how this phenomenon is to be accounted for, but it gives me occaſion to mention ſome looſe notions relating to heat and cold, which I have for ſome time enter-

entertained, but not yet reduced into any form. Allowing common fire, as well as electrical, to be a fluid capable of permeating other bodies, and seeking an equilibrium, I imagine some bodies are better fitted by nature to be conductors of that fluid than others; and that, generally, those which are the best conductors of the electrical fluid, are also the best conductors of this; and *e contra.*

Thus a body which is a good conductor of fire, readily receives it into its substance, and conducts it through the whole to all the parts, as metals and water do; and if two bodies, both good conductors, one heated, the other in its common state, are brought into contact with each other, the body which has most fire, readily communicates of it to that which had least, and that which had least readily receives it, till an equilibrium is produced. Thus if you take a dollar between your fingers with one hand, and a piece of wood, of the same dimensions, with the other, and bring both at the same time to the flame of a candle, you will find yourself obliged to drop the dollar before you drop the wood, because it conducts the heat of the candle sooner to your flesh. Thus if a silver tea-pot had a handle of the same metal, it would conduct the heat from the water to the hand, and become too hot to be used; we therefore give to a metal tea-pot a handle of wood, which is not so good a conductor as metal. But a china or stone tea-pot being in some degree of the nature of glass, which is not a good conductor of heat, may have a handle of the same stuff. Thus, also, a damp moist air shall make

a man

a man more ſenſible of cold, or chill him more than a dry air that is colder, becauſe a moiſt air is fitter to receive and conduct away the heat of his body. This fluid entering bodies in great quantity, firſt expands them, by ſeparating their parts a little, afterwards by farther ſeparating their parts, it renders ſolids fluid, and at length diſſipates their parts in air. Take this fluid from melted lead, or from water, the parts cohere again, the firſt grows ſolid, the latter becomes ice: And this is ſooner done by the means of good conductors. Thus if you take, as I have done, a ſquare bar of lead, four inches long, and one inch thick, together with three pieces of wood planed to the ſame



dimenſions, and lay them, as in the margin, on a ſmooth board, fixt ſo as not to be eaſily ſeparated or moved, and pour into the cavity they form, as much melted lead as will fill it, you will ſee the melted lead chill, and become firm, on the ſide next the leaden bar, ſome time before it chills on the other three ſides in contact with the wooden bars, though before the lead was poured in, they might all be ſuppoſed to have the ſame degree of heat or coldneſs, as they had been expoſed in the ſame room to the ſame air. You will likewiſe obſerve, that the leaden bar, as it has cooled the melted lead more than the wooden bars have done, ſo it is itſelf more heated by the melted lead. There is a certain quantity of this fluid called fire, in every living human body, which fluid being in due proportion, keeps the parts of the fleſh

and

and blood, at ſuch a juſt diſtance from each other, as that the fleſh and nerves are ſupple, and the blood fit for circulation. If part of this due proportion of fire be conducted away by means of a contact with other bodies, as air, water, or metals, the parts of our ſkin and fleſh that come into ſuch contact, firſt draw more near together than is agreeable, and give that ſenſation which we call cold; and if too much be conveyed away, the body ſtiffens, the blood ceaſes to flow, and death enſues. On the other hand, if too much of this fluid be communicated to the fleſh, the parts are ſeparated too far, and pain enſues, as when they are ſeparated by a pin or lancet. The ſenſation that the ſeparation by fire occaſions, we call heat, or burning. My deſk on which I now write, and the lock of my deſk, are both expoſed to the ſame temperature of the air, and have therefore the ſame degree of heat or cold; yet if I lay my hand ſucceſſively on the wood and on the metal, the latter feels much the coldeſt, not that it is really ſo, but being a better conductor, it more readily than the wood takes away and draws into itſelf the fire that was in my ſkin. Accordingly if I lay one hand, part on the lock, and part on the wood, and after it had lain ſo ſome time, I feel both parts with my other hand, I find the part that has been in contact with the lock, very ſenſibly colder to the touch, than the part that lay on the wood. How a living animal obtains its quantity of this fluid called fire, is a curious queſtion. I have ſhewn that ſome bodies (as metals) have a power of attracting it ſtronger than others; and

I have ſometimes ſuſpected that a living body had ſome power of attracting out of the air, or other bodies, the heat it wanted. Thus metals hammered, or repeatedly bent, grow hot in the bent or hammered part. But when I conſider that air, in contact with the body, cools it; that the ſurrounding air is rather heated by its contact with the body; that every breath of cooler air drawn in, carries off part of the body's heat when it paſſes out again; that therefore there muſt be in the body a fund for producing it, or otherwiſe the animal would ſoon grow cold; I have been rather inclined to think that the fluid *fire*, as well as the fluid *air*, is attracted by plants in their growth, and becomes conſolidated with the other materials of which they are formed, and makes a great part of their ſubſtance: That when they come to be digeſted, and to ſuffer in the veſſels a kind of fermentation, part of the fire, as well as part of the air, recovers its fluid active ſtate again, and diffuſes itſelf in the body digeſting and ſeparating it: That the fire ſo reproduced, by digeſtion and ſeparation, continually leaving the body, its place is ſupplied by freſh quantities, ariſing from the continual ſeparation. That whatever quickens the motion of the fluids in an animal, quickens the ſeparation, and reproduces more of the fire; as exerciſe. That all the fire emitted by wood, and other combuſtibles, when burning, exiſted in them before, in a ſolid ſtate, being only diſcovered when ſeparating. That ſome foſſils, as ſulphur, ſea-coal, *&c.* contain a great deal of ſolid fire: That ſome bodies are almoſt wholly ſolid fire; and that, in in ſhort, what eſcapes and is diſſipated in the burning of bodies, beſides water and earth, is generally the air and fire that before made parts of the ſolid.—Thus I imagine that animal heat ariſes by or from a kind of fermentation in the juices of the body, in the ſame manner as heat ariſes in the liquors preparing for diſtillation, wherein there is a ſeparation of the ſpirituous, from the watry and earthy parts. —And it is remarkable, that the liquor in a diſtiller's vat, when in its higheſt and beſt ſtate of fermentation, as I have been informed, has the ſame degree of heat with the human body; that is, about 94 or 96.

Thus, as by a conſtant ſupply of fuel in a chimney, you keep a warm room, ſo, by a conſtant ſupply of food in the ſtomach, you keep a warm body; only where little exerciſe is uſed, the heat may poſſibly be conducted away too faſt; in which caſe ſuch materials are to be uſed for cloathing and bedding, againſt the effects of an immediate contact of the air, as are, in themſelves, bad conductors of heat, and, conſequently, prevent its being communicated thro' their ſubſtance to the air. Hence what is called *warmth* in wool, and its preference, on that account, to linnen; wool not being ſo good a conductor: And hence all the natural coverings of animals, to keep them warm, are ſuch as retain and confine the natural heat in the body, by being bad conductors, ſuch as wool, hair, feathers, and the ſilk by which the ſilk-worm, in its tender embrio ſtate, is firſt cloathed. Cloathing, thus conſidered, does not make a man warm by *giving* warmth, but by *preventing* the too quick diſſipation of the heat produced in his body, and ſo occaſioning an accumulation.

There is another curious queſtion I will juſt venture to touch upon, *viz.* Whence ariſes the ſudden extraordinary degree of cold, perceptible on mixing ſome chemical liquors, and even on mixing ſalt and ſnow, where the compoſition appears colder than the coldeſt of the ingredients? I have never ſeen the chemical mixtures made, but ſalt and ſnow I have often mixed myſelf, and am fully ſatisfied that the compoſition feels much colder to the touch, and lowers the mercury in the thermometer more than either ingredient would do ſeparately. I ſuppoſe, with others, that cold is nothing more than the abſence of heat or fire. Now if the quantity of fire before contained or diffuſed in the ſnow and ſalt, was expelled in the uniting of the two matters, it muſt be driven away either through the air or the veſſel containing them. If it is driven off through the air, it muſt warm the air, and a thermometer held over the mixture, without touching it, would diſcover the heat, by the riſing of the mercury, as it muſt, and always does in warm air.

This, indeed, I have not tried, but I ſhould gueſs it would rather be driven off through the veſſel, eſpecially if the veſſel be metal, as being a better conductor than air; and ſo one ſhould find the baſon warmer after ſuch mixture. But, on the contrary, the veſſel grows cold, and even water in which the veſſel is ſometimes placed for the experiment, freezes into hard ice on the baſon. Now I know

not

not how to account for this, otherwise than by supposing that the composition is a better conductor of fire than the ingredients separately, and, like the lock compared with the wood, has a stronger power of attracting fire, and does accordingly attract it suddenly from the fingers, or a thermometer put into it, from the bason that contains it, and from the water in contact with the outside of the bason; so that the fingers have the sensation of extreme cold, by being deprived of much of their natural fire; the thermometer sinks, by having part of its fire drawn out of the mercury; the bason grows colder to the touch, as by having its fire drawn into the mixture, it is become more capable of drawing and receiving it from the hand; and through the bason, the water loses its fire that kept it fluid; so it becomes ice.—One would expect that from all this attracted acquisition of fire to the composition, it should become warmer; and, in fact, the snow and salt dissolve at the same time into water, without freezing.

I am, Sir, &c. B. F.

LETTER XXVII.

FROM

BENJAMIN FRANKLIN, *Esq*; of *Philadelphia*,

TO

PETER COLLINSON, *Esq*; at *London*.

SIR,

ACCORDING to your requeſt, I now ſend you the Arithmetical Curioſity, of which this is the hiſtory.

Being one day in the country, at the houſe of our common friend, the late learned Mr. *Logan*, he ſhewed me a folio *French* book, filled with magic ſquares, wrote, if I forget not, by one M. *Frenicle*, in which he ſaid the author had diſcovered great ingenuity and dexterity in the management of numbers; and, though ſeveral other foreigners had diſtinguiſhed themſelves in the ſame way, he did not recollect that any one *Engliſhman* had done any thing of the kind remarkable.

I ſaid, it was, perhaps, a mark of the good ſenſe of our *Engliſh* mathematicians, that they would not ſpend their time in things that were merely *difficiles nugæ*, incapable of any uſeful application. He anſwered, that many of the arithmetical or mathematical queſtions, publickly propoſed and

and anfwered in *England*, were equally trifling and ufelefs. Perhaps the confidering and anfwering fuch queftions, I, replied, may not be altogether ufelefs, if it produces by practice an habitual readinefs and exactnefs in mathematical difquifitions, which readinefs may, on many occafions, be of real ufe. In the fame way, fays he, may the making of thefe fquares be of ufe. I then confeffed to him, that in my younger days, having once fome leifure, (which I ftill think I might have employed more ufefully) I had amufed myfelf in making thefe kind of magic fquares, and, at length, had acquired fuch a knack at it, that I could fill the cells of any magic fquare, of reafonable fize, with a feries of numbers as faft as I could write them, difpofed in fuch a manner, as that the fums of every row, horizontal, perpendicular, or diagonal, fhould be equal; but not being fatisfied with thefe, which I looked on as common and eafy things, I had impofed on myfelf more difficult tafks, and fucceeded in making other magic fquares, with a variety of properties, and much more curious. He then fhewed me feveral in the fame book, of an uncommon and more curious kind; but as I thought none of them equal to fome I remembered to have made, he defired me to let him fee them; and accordingly, the next time I vifited him, I carried him a fquare of 8, which I found among my old papers, and which I will now give you, with an account of its properties. (*See Plate* IV.)

The

The properties are,

1. That every ſtrait row (horizontal or vertical) of 8 numbers added together, makes 260, and half each row half 260.

2. That the bent row of 8 numbers, aſcending and deſcending diagonally, *viz.* from 16 aſcending to 10, and from 23 deſcending to 17; and every one of its parallel bent rows of 8 numbers, make 260.—Alſo the bent row from 52, deſcending to 54, and from 43 aſcending to 45; and every one of its parallel bent rows of 8 numbers, make 260.—Alſo the bent row from 45 to 43 deſcending to the left, and from 23 to 17 deſcending to the right, and every one of its parallel bent rows of 8 numbers make 260.—Alſo the bent row from 52 to 54 deſcending to the right, and from 10 to 16 deſcending to the left, and every one of its parallel bent rows of 8 numbers make 260.—Alſo the parallel bent rows next to the above-mentioned, which are ſhortened to 3 numbers aſcending, and 3 deſcending, *&c.* as from 53 to 4 aſcending, and from 29 to 44 deſcending, make, with the 2 corner numbers, 260.—Alſo the 2 numbers 14, 61 aſcending, and 36, 19 deſcending, with the lower 4 numbers ſituated like them, *viz.* 50, 1, deſcending, and 32, 47, aſcending, make 260.—And, laſtly, the 4 corner numbers, with the 4 middle numbers, make 260.

So this magical ſquare ſeems perfect in its kind. But theſe are not all its properties; there are 5 other curious ones, which, at ſome other time, I will explain to you.

Mr.

PLATE IV.

A Magic Square of Squares.

200	217	232	249	8	25	40	57	72	89	104	121	136	153	168	181
58	39	26	7	250	231	218	199	186	167	154	135	122	103	90	71
198	219	230	251	6	27	38	59	70	91	102	123	134	155	166	187
60	37	28	5	252	229	220	197	188	165	156	133	124	101	92	69
201	216	233	248	9	24	41	56	73	88	105	120	137	152	169	184
55	42	23	10	247	234	215	202	183	170	151	138	119	106	87	74
203	214	235	246	11	22	43	54	75	86	107	118	139	150	171	182
53	44	21	12	245	236	213	204	181	172	149	140	117	108	85	76
205	212	237	244	13	20	45	52	77	84	109	116	141	148	173	180
51	46	19	14	243	238	241	206	179	174	147	142	115	110	83	78
207	210	239	242	15	18	47	50	79	82	111	114	143	146	175	178
49	48	17	16	241	240	209	208	177	176	145	144	113	112	81	80
196	221	228	253	4	29	36	61	68	93	100	125	132	157	164	189
62	35	30	3	254	227	222	195	190	163	158	131	126	99	94	67
194	223	226	255	2	31	34	63	66	95	98	127	130	159	162	191
64	33	32	1	256	225	224	193	192	161	160	129	128	97	96	65

J. Hulett Sculp.

Mr. *Logan* then shewed me an old arithmetical book, in quarto, wrote, I think, by one *Stifelius*, which contained a square of 16, that he said he should imagine must have been a work of great labour; but if I forget not, it had only the common properties of making the same sum, *viz.* 2056, in every row, horizontal, vertical, and diagonal. Not willing to be out-done by Mr *Stifelius*, even in the size of my square, I went home, and made, that evening, the following magical square of 16, which, besides having all the properties of the foregoing square of 8, *i.e.* it would make the 2056 in all the same rows and diagonals, had this added, that a four square hole being cut in a piece of paper of such a size as to take in and shew through it, just 16 of the little squares, when laid on the greater square, the sum of the 16 numbers so appearing through the hole, wherever it was placed on the greater square, should likewise make 2056. This I sent to our friend the next morning, who, after some days, sent it back in a letter, with these words:——" I return to thee thy asto-" nishing or most stupendous piece of the magical square, " in which" ——but the compliment is too extravagant, and therefore, for his sake, as well as my own, I ought not to repeat it. Nor is it necessary; for I make no question but you will readily allow this square of 16 to be the most magically magical of any magic square ever made by any magician. (*See the Plate.*)

I did

I did not, however, end with ſquares, but compoſed alſo a magick circle, conſiſting of 8 concentric circles, and 8 radial rows, filled with a ſeries of numbers, from 12 to 75, incluſive, ſo diſpoſed as that the numbers of each circle, or each radial row, being added to the central number 12, they made exactly 360, the number of degrees in a circle; and this circle had, moreover, all the properties of the ſquare of 8. If you deſire it, I will ſend it; but at preſent, I believe, you have enough on this ſubject.

I am, &c. B. F.

LETTER XXVIII.

To the ſame.

SIR,

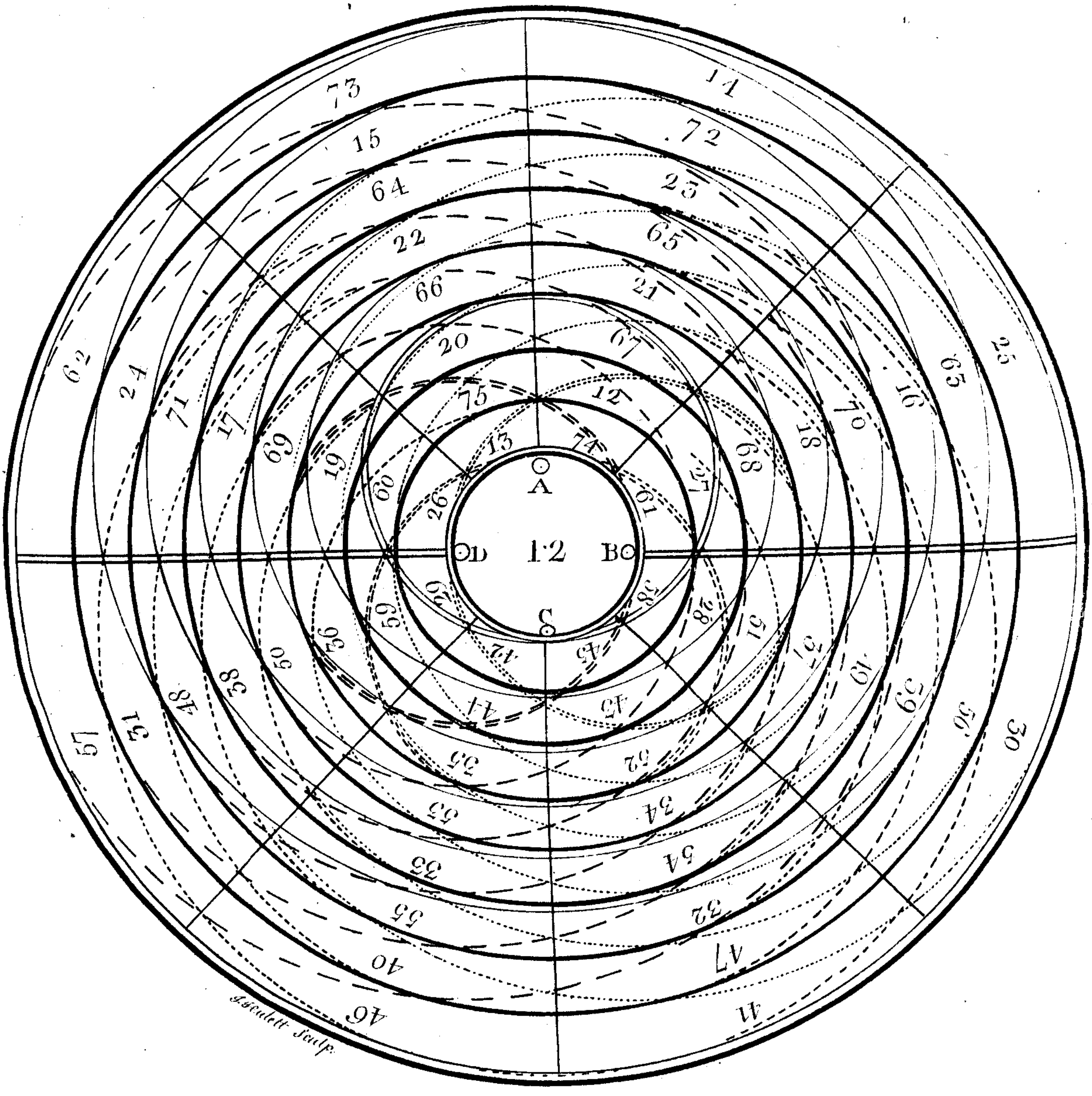
I AM glad the peruſal of the magical ſquares afforded you any amuſement. I now ſend you the magical circle. (*See Plate* V.)

Its properties, beſides thoſe mentioned in my former, are theſe.

Half the number in any radial row, added with half the central number, make 280, equal to the number of degrees in a ſemi-circle.

Alſo

PLATE V. *A Magic Circle of Circles.*



Also half the numbers in any one of the concentric circles, taken either above or below the horizontal double line, with half the central number, make 180.

And if any four adjoining numbers, standing nearly in a square, be taken from any part, and added with half the central number, they make 180.

There are, moreover, included four other sets of circular spaces, excentric with respect to the first, each of these sets containing five spaces. The centers of the circles that bound them, are at A, B, C, and D. Each set, for the more easy distinguishing them from the first, are drawn with a different colour'd ink, red, blue, green, and yellow*.

These sets of excentric circular spaces intersect those of the concentric, and each other; and yet the numbers contained in each of the twenty excentric spaces, taken all around, make, with the central number, the same sum as those in each of the 8 concentric, *viz.* 360. The halves, also of those drawn from the centers A and C, taken above or below the double horizontal line, and of those drawn from centers B and D, taken to the right or left of the vertical line, do, with half the central number, make just 180.

It may be observed, that there is not one of the numbers but what belongs at least to two of the different circular spaces; some to three, some to four, some to five; and yet they are all so placed as never to break the required num-

* In the plate they are distinguished by dashed or dotted lines, as different as the engraver could well make them.

ber 360, in any of the 28 circular ſpaces within the primitive circle.

Theſe interwoven circles make ſo perplexed an appearance, that it is not eaſy for the eye to trace every circle of numbers one would examine, through all the maze of circles interſected by it; but if you fix one foot of the compaſſes in either of the centers, and extend the other to any number in the circle you would examine belonging to that center, the moving foot will point the others out, by paſſing round over all the numbers of that circle ſucceſſively.

I am, &c. B. F.

LETTER XXIX.

To the ſame.

Dear Sir, *Philadelphia, Aug.* 25, 1755.

AS you have my former papers on Whirlwinds, *&c.* I now ſend you an account of one which I had lately an opportunity of ſeeing and examining myſelf.

Being in *Maryland*, riding with Col. *Taſker*, and ſome other gentlemen to his country-ſeat, where I and my ſon were entertained by that amiable and worthy man, with great hoſpitality and kindneſs, we ſaw in the vale below us, a ſmall whirlwind beginning in the road, and ſhewing itſelf by the duſt it raiſed and contained. It appeared in the

the form of a ſugar-loaf, ſpinning on its point, moving up the hill towards us, and enlarging as it came forward. When it paſſed by us, its ſmaller part near the ground, appeared not bigger than a common barrel, but widening upwards, it ſeemed, at 40 or 50 feet high, to be 20 or 30 feet in diameter. The reſt of the company ſtood looking after it, but my curioſity being ſtronger, I followed it, riding cloſe by its ſide, and obſerved its licking up, in its progreſs, all the duſt that was under its ſmaller part. As it is a common opinion that a ſhot, fired through a water-ſpout, will break it, I tried to break this little whirlwind, by ſtriking my whip frequently through it, but without any effect. Soon after, it quitted the road and took into the woods, growing every moment larger and ſtronger, raiſing, inſtead of duſt, the old dry leaves with which the ground was thick covered, and making a great noiſe with them and the branches of the trees, bending ſome tall trees round in a circle ſwiftly and very ſurprizingly, though the progreſſive motion of the whirl was not ſo ſwift but that a man on foot might have kept pace with it, but the circular motion was amazingly rapid. By the leaves it was now filled with, I could plainly perceive that the current of air they were driven by, moved upwards in a ſpiral line; and when I ſaw the trunks and bodies of large trees invelop'd in the paſſing whirl, which continued intire after it had left them, I no longer wondered that my whip had no effect on it in its ſmaller ſtate. I accompanied it about three quarters of a mile, till ſome limbs of dead trees, broken off by the whirl, flying about, and falling near me, made me more apprehenſive of danger; and then I ſtopped, looking at the top of it as it went on, which was viſible, by means of the leaves contained in it, for a very great height above the trees. Many of the leaves, as they got looſe from the upper and wideſt part, were ſcattered in the wind; but ſo great was their height in the air, that they appeared no bigger than flies. My ſon, who was, by this time, come up with me, followed the whirlwind till it left the woods, and croſſed an old tobacco-field, where, finding neither duſt nor leaves to take up, it gradually became inviſible below as it went away over that field. The courſe of the general wind then blowing was along with us as we travelled, and the progreſſive motion of the whirlwind was in a direction nearly oppoſite, though it did not keep a ſtrait line, nor was its progreſſive motion uniform, it making little ſallies on either hand as it went, proceeding ſometimes faſter, and ſometimes ſlower, and ſeeming ſometimes for a few ſeconds almoſt ſtationary, then ſtarting forwards pretty faſt again. When we rejoined the company, they were admiring the vaſt height of the leaves, now brought by the common wind, over our heads. Theſe leaves accompanied us as we travelled, ſome falling now and then round about us, and ſome not reaching the ground till we had gone near three miles from the place where we firſt ſaw the whirlwind begin. Upon

my

my asking Col. *Tasker* if such whirlwinds were common in *Maryland*, he answered pleasantly, *No, not at all common; but we got this on purpose to treat Mr.* Franklin. And a very high treat it was, to

Dear Sir,
Your affectionate friend,
and humble servant
B. F.

LETTER XXX.

TO

John Pringle, M.D. and F.R.S.

SIR, *Craven-street, Dec.* 21, 1757.

IN compliance with your request, I send you the following account of what I can at present recollect relating to the effects of electricity in paralytic cases, which have fallen under my observation.

Some years since, when the news-papers made mention of great cures performed in *Italy* and *Germany*, by means of electricity, a number of paralytics were brought to me from different parts of *Pensylvania*, and the neighbouring provinces, to be electrised, which I did for them at their request. My method was, to place the patient first in a chair,

chair, on an electric ſtool, and draw a number of large ſtrong ſparks from all parts of the affected limb or ſide. Then I fully charged two ſix-gallon glaſs jars, each of which had about three ſquare feet of ſurface coated; and I ſent the united ſhock of theſe through the affected limb or limbs, repeating the ſtroke commonly three times each day. The firſt thing obſerved, was an immediate greater ſenſible warmth in the lame limbs that had received the ſtroke, than in the others; and the next morning the patients uſually related, that they had in the night felt a pricking ſenſation in the fleſh of the paralytic limbs; and would ſometimes ſhew a number of ſmall red ſpots, which they ſuppoſed were occaſioned by thoſe prickings. The limbs, too, were found more capable of voluntary motion, and ſeemed to receive ſtrength. A man, for inſtance, who could not the firſt day lift the lame hand from off his knee, would the next day raiſe it four or five inches, the third day higher; and on the fifth day was able, but with a feeble languid motion, to take off his hat. Theſe appearances gave great ſpirits to the patients, and made them hope a perfect cure; but I do not remember that I ever ſaw any amendment after the fifth day; which the patients perceiving, and finding the ſhocks pretty ſevere, they became diſcouraged, went home, and in a ſhort time relapſed; ſo that I never knew any advantage from electricity in palſies that was permanent. And how far the apparent temporary advantage might ariſe from the exerciſe in the patients

patients journey, and coming daily to my houſe, or from the ſpirits given by the hope of ſucceſs, enabling them to exert more ſtrength in moving their limbs, I will not pretend to ſay.

Perhaps ſome permanent advantage might have been obtained, if the electric ſhocks had been accompanied with proper medicine and regimen, under the direction of a ſkilful phyſician. It may be, too, that a few great ſtrokes as given in my method, may not be ſo proper as many ſmall ones; ſince, by the account from *Scotland*, of a caſe, in which two hundred ſhocks from a phial were given daily, it ſeems, that a perfect cure has been made. As to any uncommon ſtrength ſuppoſed to be in the machine uſed in that caſe, I imagine it could have no ſhare in the effect produced; ſince the ſtrength of the ſhock from charged glaſs, is in proportion to the quantity of ſurface of the glaſs coated; ſo that my ſhocks from thoſe large jars, muſt have been much greater than any that could be received from a phial held in the hand.

I am, with great reſpect,

SIR,

Your moſt obedient Servant,

B. F.

LET-

LETTER XXXI.

To the ſame.

SIR, *Craven-ſtreet*, *Jan.* 6, 1758.

I Return Mr. *Mitchell's* paper on the ſtrata of the earth* with thanks. The reading of it, and peruſal of the draft that accompanies it, have reconciled me to thoſe convulſions which all naturaliſts agree this globe has ſuffered. Had the different ſtrata of clay, gravel, marble, coals, lime-ſtone, ſand, minerals, &c. continued to lie level, one under the other, as they may be ſuppoſed to have done before thoſe convulſions, we ſhould have had the uſe only of a few of the uppermoſt of the ſtrata, the others lying too deep and too difficult to be come at; but the ſhell of the earth being broke, and the fragments thrown into this oblique poſition, the disjointed ends of a great number of ſtrata of different kinds are brought up to day, and a great variety of uſeful materials put into our power, which would otherwiſe have remained eternally concealed from us. So that what has been uſually looked upon as a *ruin* ſuffered by this part of the univerſe, was, in reality, only a preparation, or means of rendering the earth more fit for uſe, more capable of being to mankind a convenient and comfortable habitation.

I am, Sir, with great eſteem, yours, &c. B. F.

* See this Paper afterwards printed in the *Philoſophical Tranſactions*.

LETTER XXXII.

To Dr. *L.* of *Charles-Town, South-Carolina.*

Dear Sir, *London, June* 17, 1758.

IN a former letter I mentioned the experiment for cooling bodies by evaporation, and that I had, by repeatedly wetting the thermometer with common ſpirits, brought the mercury down five or ſix degrees. Being lately at *Cambridge*, and mentioning this in converſation with Dr. *Hadley*, profeſſor of chemiſtry there, he propoſed repeating the experiments with ether, inſtead of common ſpirits, as the ether is much quicker in evaporation. We accordingly went to his chamber, where he had both ether and a thermometer. By dipping firſt the ball of the thermometer into the ether, it appeared that the ether was preciſely of the ſame temperament with the thermometer, which ſtood then at 65; for it made no alteration in the height of the little column of mercury. But when the thermometer was taken out of the ether, and the ether with which the ball was wet, began to evaporate, the mercury ſunk ſeveral degrees. The wetting was then repeated by a feather that had been dipped into the ether, when the mercury ſunk ſtill lower. We continued this operation, one of us wetting the ball, and another of the company blowing on it with the bellows, to quicken the evaporation, the mercury ſinking all the time, till it came down to 7, which is 25 degrees below the freezing point, when we left off.—Soon after it paſſed the freezing point, a thin coat of ice began to cover the ball. Whether this was water collected and condenſed by the coldneſs of the ball, from the moiſture in the air, or from our breath; or whether the feather, when dipped into the ether, might not ſometimes go through it, and bring up ſome of the water that was under it, I am not certain; perhaps all might contribute. The ice continued increaſing till we ended the experiment, when it appeared near a quarter of an inch thick all over the ball, with a number of ſmall ſpicula, pointing outwards. From this experiment one may ſee the poſſibility of freezing a man to death on a warm ſummer's day, if he were to ſtand in a paſſage thro' which the wind blew briſkly, and to be wet frequently with ether, a ſpirit that is more inflammable than brandy, or common ſpirits of wine.

It is but within theſe few years, that the *European* philoſophers ſeem to have known this power in nature, of cooling bodies by evaporation. But in the eaſt they have long been acquainted with it. A friend tells me, there is a paſſage in *Bernier's* travels through *Indoſtan*, written near one hundred years ago, that mentions it as a practice (in travelling over dry deſarts in that hot climate) to carry water in flaſks wrapt in wet woollen cloths, and hung on the

ſhady

ſhady ſide of the camel, or carriage, but in the free air; whereby, as the cloths gradually grow drier, the water contained in the flaſks is made cool. They have likewiſe a kind of earthen pots, unglaz'd, which let the water gradually and ſlowly ooze through their pores, ſo as to keep the outſide a little wet, notwithſtanding the continual evaporation, which gives great coldneſs to the veſſel, and the water contained in it. Even our common ſailors ſeem to have had ſome notion of this property; for I remember, that being at ſea, when I was a youth, I obſerved one of the ſailors, during a calm in the night, often wetting his finger in his mouth, and then holding it up in the air, to diſcover, as he ſaid, if the air had any motion, and from which ſide it came; and this he expected to do, by finding one ſide of his finger grow ſuddenly cold, and from that ſide he ſhould look for the next wind; which I then laughed at as a fancy.

May not ſeveral phænomena, hitherto unconſidered, or unaccounted for, be explained by this property? During the hot *Sunday* at *Philadelphia*, in *June* 1750, when the thermometer was up at 100 in the ſhade, I ſat in my chamber without exerciſe, only reading or writing, with no other cloaths on than a ſhirt, and a pair of long linen drawers, the windows all open, and a briſk wind blowing through the houſe, the ſweat ran off the backs of my hands, and my ſhirt was often ſo wet, as to induce me to call for dry ones to put on; in this ſituation, one might have expected, that the natural heat of the body 96, added to the heat of the air 100, ſhould jointly have created or produced a much greater degree of heat in the body; but the fact was, that my body never grew ſo hot as the air that ſurrounded it, or the inanimate bodies immers'd in the ſame air. For I remember well, that the deſk, when I laid my arm upon it; a chair, when I ſat down in it; and a dry ſhirt out of the drawer, when I put it on, all felt exceeding warm to me, as if they had been warmed before a fire. And I ſuppoſe a dead body would have acquired the temperature of the air, though a living one, by continual ſweating, and by the evaporation of that ſweat, was kept cold.—May not this be a reaſon why our reapers in *Penſylvania*, working in the open field, in the clear hot ſunſhine common in our harveſt-time *, find themſelves well able to go through that labour, without being much incommoded by the heat, while they continue to ſweat, and while they ſupply matter for keeping up that ſweat, by drinking frequently of a thin evaporable liquor, water mixed with rum; but if the ſweat ſtops, they drop, and ſometimes die ſuddenly, if a ſweating is not again brought on by drinking that liquor, or, as ſome rather chuſe in that caſe, a kind of hot punch, made with water, mixed with

* *Penſylvania* is in about lat. 40, and the ſun, of courſe, about 12 degrees higher, and therefore much hotter than in *England*. Their harveſt is about the end of *June*, or beginning of *July*, when the ſun is nearly at the higheſt.

honey,

honey, and a considerable proportion of vinegar?—May there not be in negroes a quicker evaporation of the perspirable matter from their skins and lungs, which, by cooling them more, enables them to bear the sun's heat better than whites do? (if that is a fact, as it is said to be; for the alledg'd necessity of having negroes rather than whites, to work in the *West-India* fields, is founded upon it) though the colour of their skins would otherwise make them more sensible of the sun's heat, since black cloth heats much sooner, and more, in the sun, than white cloth. I am persuaded, from several instances happening within my knowledge, that they do not bear cold weather so well as the whites; they will perish when exposed to a less degree of it, and are more apt to have their limbs frost-bitten; and may not this be from the same cause? Would not the earth grow much hotter under the summer sun, if a constant evaporation from its surface, greater as the sun shines stronger, did not, by tending to cool it, balance, in some degree, the warmer effects of the sun's rays?—Is it not owing to the constant evaporation from the surface of every leaf, that trees, though shone on by the sun, are always, even the leaves themselves, cool to our sense? at least much cooler than they would otherwise be?—May it not be owing to this, that fanning ourselves when warm, does really cool us, though the air is itself warm that we drive with the fan upon our faces; for the atmosphere round, and next to our bodies, having imbibed as much of the perspired

perſpired vapour as it can well contain, receives no more, and the evaporation is therefore check'd and retarded, till we drive away that atmoſphere, and bring dryer air in its place, that will receive the vapour, and thereby facilitate and increaſe the evaporation? Certain it is, that mere blowing of air on a dry body does not cool it, as any one may ſatisfy himſelf, by blowing with a bellows on the dry ball of a thermometer; the mercury will not fall; if it moves at all, it rather riſes, as being warmed by the friction of the air on its ſurface?—To theſe queries of imagination, I will only add one practical obſervation; that wherever it is thought proper to give eaſe, in caſes of painful inflammation in the fleſh, (as from burnings, or the like) by cooling the part; linen cloths, wet with ſpirit, and applied to the part inflamed, will produce the coolneſs required, better than if wet with water, and will continue it longer. For water, though cold when firſt applied, will ſoon acquire warmth from the fleſh, as it does not evaporate faſt enough; but the cloths wet with ſpirit, will continue cold as long as any ſpirit is left to keep up the evaporation, the parts warmed eſcaping as ſoon as they are warmed, and carrying off the heat with them.

I am, Sir, &c. B. F.

LET-

LETTER XXXIII.

TO

J. B. Eſq; *at* Boſtón, *in* New-England.

Dear Sir, *London, Dec.* 2, 1758.

I HAVE executed here an eaſy ſimple contrivance, that I have long ſince had in ſpeculation, for keeping rooms warmer in cold weather than they generally are, and with leſs fire. It is this. The opening of the chimney is contracted, by brick-work faced with marble ſlabs, to about two feet between the jambs, and the breaſt brought down to within about three feet of the hearth.—An iron frame is placed juſt under the breaſt, and extending quite to the back of the chimney, ſo that a plate of the ſame metal may ſlide horizontally backwards and forwards in the grooves on each ſide of the frame. This plate is juſt ſo large as to fill the whole ſpace, and ſhut the chimney entirely when thruſt quite in, which is convenient when there is no fire; drawing it out, ſo as to leave a ſpace between its farther edge and the back, of about two inches; this ſpace is ſufficient for the ſmoke to paſs; and ſo large a part of the

the funnel being ſtopt by the reſt of the plate, the paſſage of warm air out of the room, up the chimney, is obſtructed and retarded, and by that means much cold air is prevented from coming in through crevices, to ſupply its place. This effect is made manifeſt three ways. Firſt, when the fire burns briſkly in cold weather, the howling or whiſtling noiſe made by the wind, as it enters the room through the crevices, when the chimney is open as uſual, ceaſes as ſoon as the plate is ſlid in to its proper diſtance. Secondly, opening the door of the room about half an inch, and holding your hand againſt the opening, near the top of the door, you feel the cold air coming in againſt your hand, but weakly, if the plate be in. Let another perſon ſuddenly draw it out, ſo as to let the air of the room go up the chimney, with its uſual freedom where chimneys are open, and you immediately feel the cold air ruſhing in ſtrongly. Thirdly, if ſomething be ſet againſt the door, juſt ſufficient, when the plate is in, to keep the door nearly ſhut, by reſiſting the preſſure of the air that would force it open: Then, when the plate is drawn out, the door will be forced open by the increaſed preſſure of the outward cold air endeavouring to get in to ſupply the place of the warm air, that now paſſes out of the room to go up the chimney. In our common open chimneys, half the fuel is waſted, and its effect loſt, the air it has warmed being immediately drawn off. Several of my acquaintance having ſeen this ſimple machine in my room, have imitated

it

it at their own houses, and it seems likely to become pretty common. I describe it thus particularly to you, because I think it would be useful in *Boston*, where firing is often dear.

Mentioning chimneys puts me in mind of a property I formerly had occasion to observe in them, which I have not found taken notice of by others; it is, that in the summer time, when no fire is made in the chimneys, there is, nevertheless, a regular draft of air through them; continually passing upwards, from about five or six o'clock in the afternoon, till eight or nine o'clock the next morning, when the current begins to slacken and hesitate a little, for about half an hour, and then sets as strongly down again, which it continues to do till towards five in the afternoon, then slackens and hesitates as before, going sometimes a little up, then a little down, till in about half an hour it gets into a steady upward current for the night, which continues till eight or nine the next day; the hours varying a little as the days lengthen and shorten, and sometimes varying from sudden changes in the weather; as if, after being long warm, it should begin to grow cool about noon, while the air was coming down the chimney, the current will then change earlier than the usual hour, &c.

This property in chimneys I imagine we might turn to some account, and render improper, for the future, the old saying, *as useless as a chimney in summer.* If the opening of the chimney, from the breast down to the hearth, be closed by a slight moveable frame, or two in the manner of doors, covered with canvas, that will let the air through, but keep out the flies; and another little frame ſet within upon the hearth, with hooks on which to hang joints of meat, fowls, *&c.* wrapt well in wet linen cloths, three or four fold, I am confident that if the linen is kept wet, by ſprinkling it once a day, the meat would be ſo cooled by the evaporation, carried on continually by means of the paſſing air, that it would keep a week or more in the hotteſt weather. Butter and milk might likewiſe be kept cool, in veſſels or bottles covered with wet cloths. A ſhallow tray, or keeler, ſhould be under the frame to receive any water that might drip from the wetted cloths. I think, too, that this property of chimneys might, by means of ſmoak-jack vanes, be applied to ſome mechanical purpoſes, where a ſmall but pretty conſtant power only is wanted.

If you would have my opinion of the cauſe of this changing current of air in chimneys, it is, in ſhort, as follows. In ſummer time there is generally a great difference in the warmth of the air at mid-day and midnight, and, of courſe, a difference of ſpecific gravity in the air, as the more it is warmed the more it is rarefied. The funnel of a chimney being for the moſt part ſurrounded by the houſe, is protected, in a great meaſure, from the direct action of the ſun's rays, and alſo from the coldneſs of the night air. It thence preſerves a middle temperature between the heat of the day, and the coldneſs of the night. This middle temperature it communicates to the air contained in it. If the

the ſtate of the outward air be cooler than that in the funnel of the chimney, it will, by being heavier, force it to riſe, and go out at the top. What ſupplies its place from below, being warmed, in its turn, by the warmer funnel, is likewiſe forced up by the colder and weightier air below, and ſo the current is continued till the next day, when the ſun gradually changes the ſtate of the outward air, makes it firſt as warm as the funnel of the chimney can make it, (when the current begins to heſitate) and afterwards warmer. Then the funnel being cooler than the air that comes into it, cools that air, makes it heavier than the outward air; of courſe it deſcends; and what ſucceeds it from above, being cool'd in its turn, the deſcending current continues till towards evening, when it again heſitates and changes its courſe, from the change of warmth in the outward air, and the nearly remaining ſame middle temperature in the funnel.

Upon this principle, if a houſe were built behind *Beacon-hill*, an adit carried from one of the doors into the hill horizontally, till it met with a perpendicular ſhaft ſunk from its top, it ſeems probable to me, that thoſe who lived in the houſe, would conſtantly, in the heat even of the calmeſt day, have as much cool air paſſing through the houſe, as they ſhould chuſe; and the ſame, though reverſed in its current, during the ſtilleſt night.

I think, too, this property might be made of uſe to miners; as where ſeveral ſhafts or pits are ſunk perpendicularly into the earth, communicating at bottom by horizontal paſſages, which is a common caſe, if a chimney of thirty or forty feet high were built over one of the ſhafts, or ſo near the ſhaft, that the chimney might communicate with the top of the ſhaft, all air being excluded but what ſhould paſs up or down by the ſhaft, a conſtant change of air would, by this means, be produced in the paſſages below, tending to ſecure the workmen from thoſe damps which ſo frequently incommode them. For the freſh air would be almoſt always going down the open ſhaft, to go up the chimney, or down the chimney to go up the ſhaft. Let me add one obſervation more, which is, That if that part of the funnel of a chimney, which appears above the roof of a houſe, be pretty long, and have three of its ſides expoſed to the heat of the ſun ſucceſſively, *viz.* when he is in the eaſt, in the ſouth, and in the weſt; while the north ſide is ſheltered by the building from the cool northerly winds. Such a chimney will often be ſo heated by the ſun, as to continue the draft ſtrongly upwards, through the whole twenty-four hours, and often for many days together. If the outſide of ſuch a chimney be painted black, the effect will be ſtill greater, and the current ſtronger.

I am, dear Sir, yours, &c. B. F.

LET-

LETTER XXXIV.

To Dr. *H.* at *London.*

SIR, *Craven-street, June* 7, 1759.

I NOW return the smallest of your two *Tourmalins*, with hearty thanks for your kind present of the other, which, though I value highly for its rare and wonderful properties, I shall ever esteem it more for the friendship I am honoured with by the giver.

I hear that the negative electricity of one side of the *Tourmalin*, when heated, is absolutely denied, (and all that has been related of it, ascribed to prejudice in favour of a system) by some ingenious gentlemen abroad, who profess to have made the experiments on the stone with care and exactness. The experiments have succeeded differently with me; yet I would not call the accuracy of those gentlemen in question. Possibly the Tourmalins they have tried were not properly cut; so that the positive and negative powers were obliquely placed, or in some manner whereby their effects were confused, or the negative part more easily supplied by the positive. Perhaps the lapidaries who have hitherto cut these stones, had no regard to the situation of the two powers, but chose to make the faces of the stone where they could obtain the greatest breadth, or some other advantage in the form. If any of these

these ſtones, in their natural ſtate, can be procured here, I think it would be right to endeavour finding, before they are cut, the two ſides that contain the oppoſite powers, and make the faces there. Poſſibly, in that caſe, the effects might be ſtronger, and more diſtinct; for though both theſe ſtones that I have examined have evidently the two properties, yet, without the full heat given by boiling water, they are ſomewhat confuſed; the virtue ſeems ſtrongeſt towards one end of the face; and in the middle, or near the other end, ſcarce diſcernible; and the negative, I think, always weaker than the poſitive.

I have had the large one new cut, ſo as to make both ſides alike, and find the change of form has made no change of power, but the properties of each ſide remain the ſame as I found them before. It is now ſet in a ring in ſuch a manner as to turn on an axis, that I may conveniently, in making experiments, come at both ſides of the ſtone. The little rim of gold it is ſet in, has made no alteration in its effects. The warmth of my finger, when I wear it, is ſufficient to give it ſome degree of electricity, ſo that it is always ready to attract light bodies.

The following experiments have ſatisfied me that M. *Æpinus*'s account of the poſitive and negative ſtates of the oppoſite ſides of the heated Tourmalin, is well founded.

I heated the large ſtone in boiling water.

As ſoon as it was dry, I brought it near a very ſmall cork ball, that was ſuſpended by a ſilk thread.

The

The ball was attracted by one face of the ſtone, which I call A, and then repelled.

The ball in that ſtate was alſo repelled by the poſitively charg'd wire of a phial, and attracted by the other ſide of the ſtone, B.

The ſtone being a-freſh heated, and the ſide B brought near the ball, it was firſt attracted, and preſently after repelled by that ſide.

In this ſecond ſtate it was repelled by the negatively charged wire of a phial.

Therefore, if the principles now generally received, relating to poſitive and negative electricity, are true, the ſide A of the large ſtone, when the ſtone is heated in water, is in a poſitive ſtate of electricity; and the ſide B, in a negative ſtate.

The ſame experiments being made with the ſmall ſtone ſtuck by one edge on the end of a ſmall glaſs tube, with ſealing-wax, the ſame effects are produced. The flat ſide of the ſmall ſtone gives the ſigns of poſitive electricity; the high ſide gives the ſigns of negative electricity.

Again;

I ſuſpended the ſmall ſtone by a ſilk thread.

I heated it as it hung, in boiling water.

I heated the large one in boiling water.

Then I brought the large ſtone near to the ſuſpended ſmall one.

Which

Which immediately turned its flat ſide to the ſide B of the large ſtone, and would cling to it.

I turned the ring, ſo as to preſent the ſide A of the large ſtone, to the flat ſide of the ſmall one.

The flat ſide was repelled, and the ſmall ſtone, turning quick, applied its high ſide to the ſide A of the large one.

This was preciſely what ought to happen, on the ſuppoſition that the flat ſide of the ſmall ſtone, when heated in water, is poſitive, and the high ſide negative; the ſide A of the large ſtone poſitive, and the ſide B negative.

The effect was apparently the ſame as would have been produced, if one magnet had been ſuſpended by a thread, and the different poles of another brought alternately near it.

I find that the face A, of the large ſtone, being coated with leaf-gold, (attach'd by the white of an egg, which will bear dipping in hot water) becomes quicker and ſtronger in its effect on the cork-ball, repelling it the inſtant it comes in contact; which I ſuppoſe to be occaſioned by the united force of different parts of the face, collected and acting together through the metal.

I am, &c. B. F.

LET-

LETTER XXXV.

To Mr. P. F. *in* Newport.

SIR, *London, May* 7, 1760.

* * * * * * It has, indeed, as you obſerve, been the opinion of ſome very great naturaliſts, that the ſea is ſalt only from the diſſolution of mineral or rock ſalt, which its waters happened to meet with. But this opinion takes it for granted that all water was originally freſh, of which we can have no proof. I own I am inclined to a different opinion, and rather think all the water on this globe was originally ſalt, and that the freſh water we find in ſprings and rivers, is the produce of diſtillation. The ſun raiſes the vapours from the ſea, which form clouds, and fall in rain upon the land, and ſprings and rivers are formed of that rain.——As to the rock-ſalt found in mines, I conceive, that inſtead of communicating its ſaltneſs to the ſea, it is itſelf drawn from the ſea, and that of courſe the ſea is now freſher than it was originally. This is only another effect of nature's diſtillery, and might be performed various ways.

It is evident from the quantities of ſea-ſhells, and the bones and teeth of fiſhes found in high lands, that the ſea has formerly covered them. Then, either the ſea has been higher than it now is, and has fallen away from thoſe high lands; or they have been lower than they are, and were lifted up out of the water to their prefent height, by fome internal mighty force, fuch as we ftill feel fome remains of, when whole continents are moved by earthquakes. In either cafe it may be fuppofed that large hollows, or valleys among hills, might be left filled with fea-water, which evaporating, and the fluid part drying away in a courfe of years, would leave the falt covering the bottom; and that falt coming afterwards to be covered with earth, from the neighbouring hills, could only be found by digging through that earth. Or, as we know from their effects, that there are deep fiery caverns under the earth, and even under the fea, if at any time the fea leaks into any of them, the fluid parts of the water muft evaporate from that heat, and pafs off through fome vulcano, while the falt remains, and by degrees, and continual accretion, becomes a great mafs. Thus the cavern may at length be filled, and the volcano connected with it ceafe burning, as many it is faid have done; and future miners penetrating fuch cavern, find what we call a falt mine.——This is a fancy I had on vifiting the falt-mines at *Northwich*, with my fon. I fend you a piece of the rock-falt which he brought up with him out of the mine. * * * * * *

I am, Sir, &c. B. F.

LETTER XXXVI.

To Mr. ALEXANDER SMALL, *London*.

Dear Sir, *May* 12, 1760.

AGreeable to your requeſt, I ſend you my reaſons for thinking that our North-Eaſt ſtorms in *North-America* begin firſt, in point of time, in the South-Weſt parts: That is to ſay, the air in *Georgia*, the fartheſt of our colonies to the South-Weſt, begins to move South-Weſterly befere the air of *Carolina*, which is the next colony North-Eaſtward; the air of *Carolina* has the ſame motion before the air of *Virginia*, which lies ſtill more North-Eaſtward; and ſo on North-Eaſterly through *Penſylvania*, *New-York*, *New-England*, &c. quite to *Newfoundland*.

Theſe North-Eaſt ſtorms are generally very violent, continue ſometimes two or three days, and often do conſiderable damage in the harbours along the coaſt. They are attended with thick clouds and rain.

What firſt gave me this idea, was the following circumſtance. About twenty years ago, a few more or leſs, I cannot from my memory be certain, we were to have an eclipſe of the moon at *Philadelphia*, on a *Friday* evening, about nine o'clock. I intended to obſerve it, but was prevented by a North-Eaſt ſtorm, which came on about ſe-

 ven,

ven, with thick clouds as uſual, that quite obſcured the whole hemiſphere. Yet when the poſt brought us the *Boſton* news-paper, giving an account of the effects of the ſame ſtorm in thoſe parts, I found the beginning of the eclipſe had been well obſerved there, though *Boſton* lies N. E. of *Philadelphia* about 400 miles. This puzzled me, becauſe the ſtorm began with us ſo ſoon as to prevent any obſervation, and being a N. E. ſtorm, I imagined it muſt have began rather ſooner in places farther to the North Eaſtward, than it did at *Philadelphia*. I therefore mentioned it in a letter to my brother who lived at *Boſton*; and he informed me the ſtorm did not begin with them till near eleven o'clock, ſo that they had a good obſervation of the eclipſe: And upon comparing all the other accounts I received from the ſeveral colonies, of the time of beginning of the ſame ſtorm, and ſince that of other ſtorms of the ſame kind, I found the beginning to be always later the farther North-Eaſtward. I have not my notes with me here in *England*, and cannot, from memory, ſay the proportion of time to diſtance, but I think it is about an hour to every hundred miles.

From thence I formed an idea of the cauſe of theſe ſtorms, which I would explain by a familiar inſtance or two.——Suppoſe a long canal of water ſtopp'd at the end by a gate. The water is quite at reſt till the gate is open, then it begins to move out through the gate; the water next the gate is firſt in motion, and moves towards the gate;

gate ; the water next to that firſt water moves next, and ſo on ſucceſſively, till the water at the head of the canal is in motion, which is laſt of all. In this caſe all the water moves indeed towards the gate, but the ſucceſſive times of beginning motion are the contrary way, *viz.* from the gate backwards to the head of the canal. Again, ſuppoſe the air in a chamber at reſt, no current through the room till you make a fire in the chimney. Immediately the air in the chimney being rarefied by the fire, riſes ; the air next the chimney flows in to ſupply its place, moving towards the chimney ; and, in conſequence, the reſt of the air ſucceſſively, quite back to the door. Thus to produce our North-Eaſt ſtorms, I ſuppoſe ſome great heat and rarefaction of the air in or about the Gulph of *Mexico* ; the air thence riſing has its place ſupplied by the next more northern, cooler, and therefore denſer and heavier, air ; that, being in motion, is followed by the next more northern air, *&c. &c.* in a ſucceſſive current, to which current our coaſt and inland ridge of mountains give the direction of North-Eaſt, as they lie N. E. and S. W.

This I offer only as an hypotheſis to account for this particular fact ; and, perhaps, on farther examination, a better and truer may be found. I do not ſuppoſe all ſtorms generated in the ſame manner. Our North-Weſt thunder-guſts in *America* I know are not ; but of them I have written my opinion fully in a paper which you have ſeen.

I am, &c. B. F.

LET-

LETTER XXXVII.

From Mr. KINNERSLEY.

SIR, *Philadelphia, March* 12, 1761.

HAVING lately made the following experiments, I very chearfully communicate them, in hopes of giving you ſome degree of pleaſure, and exciting you to further explore your favourite, but not quite exhauſted ſubject, ELECTRICITY.

I placed myſelf on an electric ſtand, and, being well electriſed, threw my hat to an unelectriſed perſon, at a conſiderable diſtance, on another ſtand, and found that the hat carried ſome of the electricity with it; for, upon going immediately to the perſon who received it, and holding a flaxen thread near him, I perceived he was electriſed ſufficiently to attract the thread.

I then ſuſpended, by ſilk, a broad plate of metal, and electriſed ſome boiling water under it, at about four feet diſtance, expecting that the vapour, which aſcended plentifully to the plate, would, upon the principle of the foregoing experiment, carry up ſome of the electricity with it; but was at length fully convinced, by ſeveral repeated trials, that it left all its ſhare thereof behind. This I know not how to account for; but does it not ſeem to corroborate your

your hypothesis, That the vapours of which the clouds are formed, leave their share of electricity behind, in the common stock, and ascend in the negative state?

I put boiling water into a coated *Florence* flask, and found that the heat so enlarged the pores of the glass, that it could not be charged. The electricity passed through as readily, to all appearance, as through metal; the charge of a three pint bottle went freely through, without injuring the flask in the least. When it became almost cold, I could charge it as usual. Would not this experiment convince the Abbe *Nollet* of his egregious mistake? For while the electricity went fairly through the glass, as he contends it always does, the glass could not be charged at all.

I took a slender piece of cedar, about eighteen inches long, fixed a brass cap in the middle, thrust a pin horizontally and at right angles, through each end, (the points in contrary directions) and hung it, nicely ballanc'd, like the needle of a compass, on a pin, about six inches long, fixed in the center of an electric stand. Then, electrising the stand, I had the pleasure of seeing what I expected; the wooden needle turned round, carrying the pins with their heads foremost. I then electrised the stand negatively, expecting the needle to turn the contrary way, but was extremely disappointed, for it went still the same way as before. When the stand was electrised positively, I suppose that the natural quantity of electricity in the air being increased on one side, by what issued from the points, the needle was attracted by the leſſer quantity on the other ſide. When electriſed negatively, I ſuppoſe that the natural quantity of electricity in the air was diminiſhed near the points; in conſequence whereof, the equilibrium being deſtroyed, the needle was attracted by the greater quantity on the oppoſite ſide.

The doctrine of repulſion, in electriſed bodies, I begin to be ſomewhat doubtful of. I think all the phænomena on which it is founded, may be well enough accounted for without it. Will not cork balls, electriſed negatively, ſeparate as far as when electriſed poſitively? And may not their ſeparation in both caſes be accounted for upon the ſame principle, namely, the mutual attraction of the natural quantity in the air, and that which is denſer or rarer in the cork balls? it being one of the eſtabliſhed laws of this fluid, that quantities of different denſities ſhall mutually attract each other, in order to reſtore the equilibrium.

I can ſee no reaſon to conclude that the air has not its ſhare of the common ſtock of electricity, as well as glaſs, and, perhaps, all other electrics *per ſe*. For though the air will admit bodies to be electriſed in it either poſitively or negatively, and will not readily carry off the redundancy in the one caſe, or ſupply the deficiency in the other, yet let a perſon in the negative ſtate, out of doors in the dark, when the air is dry, hold, with his arm extended, a long ſharp needle, pointing upwards, and he will ſoon be convinced that electricity may be drawn out of the air; not very

very plentifully, for, being a bad conductor, it seems loth to part with it, but yet some will evidently be collected. The air near the person's body having less than its natural quantity, will have none to spare; but, his arm being extended, as above, some will be collected from the remoter air, and will appear luminous, as it converges to the point of the needle.

Let a person electrised negatively present the point of a needle, horizontally, to a cork ball, suspended by silk, and the ball will be attracted towards the point, till it has parted with so much of its natural quantity of electricity as to be in the negative state in the same degree with the person who holds the needle; then it will recede from the point, being, as I suppose, attracted the contrary way by the electricity of greater density in the air behind it. But, as this opinion seems to deviate from electrical orthodoxy, I should be glad to see these phænomena better accounted for by your superior and more penetrating genius.

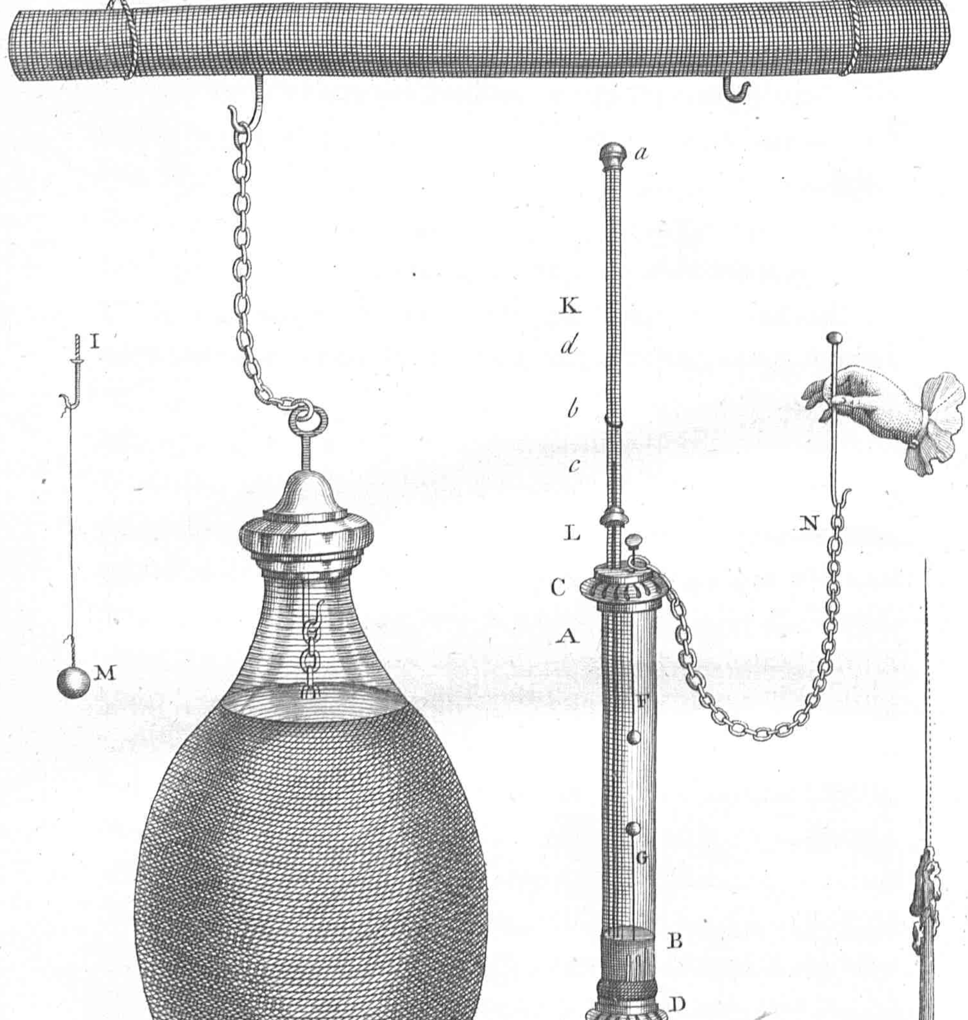
Whether the electricity in the air, in clear dry weather, be of the same density at the height of two or three hundred yards, as near the surface of the earth, may be satisfactorily determined by your old experiment of the kite. The twine should have, throughout, a very small wire in it, and the ends of the wire, where the several lengths are united, ought to be tied down with a waxed thread, to prevent their acting in the manner of points. I have tried the experiment twice, when the air was as dry as we ever have

 it,

it, and ſo clear that not a cloud could be ſeen, and found the twine each time in a ſmall degree electriſed poſitively. The kite had three metalline points fixed to it; one on the top, and one on each ſide. That the twine was electriſed, appeared by the ſeparating of two ſmall cork balls, ſuſpended on the twine by fine flaxen threads, juſt above where the ſilk was tied to it, and ſheltered from the wind. That the twine was electriſed poſitively, was proved, by applying to it the wire of a charged bottle, which cauſed the balls to ſeparate further, without firſt coming nearer together. This experiment ſhewed that the electricity in the air, at thoſe times, was denſer above than below. But that cannot be always the caſe; for you know we have frequently found the thunder clouds in the negative ſtate, attracting electricity from the earth; which ſtate, it is probable, they are always in when firſt formed, and till they have received a ſufficient ſupply. How they come afterwards, towards the latter end of the guſt, to be in the poſitive ſtate, which is ſometimes the caſe, is a ſubject for further enquiry.

After the above experiments with the wooden needle, I formed a croſs, of two pieces of wood, of equal length, interſecting each other at right angles in the middle, hung it horizontally upon a central pin, and ſet a light horſe, with his rider, upon each extremity; whereupon, the whole being nicely balanced, and each courſer urged on by

an



J. Hulett sculp.

an electrised point instead of a pair of spurs, I was entertained with an electrical horse-race.

I have contrived an electrical air thermometer, and made several experiments with it, that have afforded me much satisfaction and pleasure. It is extremely sensible of any alteration in the state of the included air, and fully determines that controverted point, Whether there be any heat in the electric fire? By the enclosed draught, and the following description, you will readily apprehend the construction of it.

A B is a glass tube, about eleven inches long, and one inch diameter in the bore. It has a brass feril, cemented on each end, with a top and bottom part, C and D, to be screwed on, air-tight, and taken off at pleasure. In the center of the bottom part D, is a male screw, which goes into a brass nut, in the mahogany pedestal E. The wires F and G, are for the electric fire to pass through, darting from one to the other. The wire G extends through the pedestal to H, and may be raised and lowered by means of a male screw on it. The wire F may be taken out, and the hook I be screwed into its place. K is a glass tube, with a small bore, open at both ends, cemented in the brass tube L, which screws into the top part C. The lower end of the tube K is immersed in water, coloured with cochineal, at the bottom of the tube A B. (I used, at first, coloured spirits of wine, but in one experiment I made, it took fire.) On the top of the tube K is cemented, for ornament, a braſs feril, with a head ſcrewed on it, which has a ſmall air-hole through its ſide, at *a*. The wire *b*, is a ſmall round ſpring, that embraces the tube K, ſo as to ſtay wherever it is placed. The weight M is to keep ſtrait whatever may be ſuſpended in the tube A B, on the hook I. Air muſt be blown through the tube K, into the tube A B, till enough is intruded to raiſe, by its elaſtic force, a column of the coloured water in the tube K, up to *c*, or thereabouts; and then, the gage-wire *b*, being ſlipt down to the top of the column, the thermometer is ready for uſe.

I ſet the thermometer on an electric ſtand, with the chain N fixed to the prime conductor, and kept it well electriſed a conſiderable time; but this produced no ſenſible effect; which ſhews, that the electric fire, when in a ſtate of reſt, has no more heat than the air, and other matter wherein it reſides.

When the wires F and G are in contact, a large charge of electricity ſent through them, even that of my caſe of five and thirty bottles, containing above thirty ſquare feet of coated glaſs, will produce no rarefaction of the air included in the tube A B; which ſhews that the wires are not heated by the fire's paſſing through them.

When the wires are about two inches apart, the charge of a three pint bottle, darting from one to the other, rarefies the air very evidently; which ſhews, I think, that the

the electric fire must produce heat in itself, as well as in the air, by its rapid motion.

The charge of one of my glass jars, (which will contain about five gallons and a half, wine measure) darting from wire to wire, will, by the disturbance it gives the air, repelling it in all directions, raise the column in the tube K, up to *d*, or thereabouts; and the charge of the above-mentioned case of bottles will raise it to the top of the tube. Upon the air's coalescing, the column, by its gravity, instantly subsides, till it is in equilibrio with the rarefied air; it then gradually descends, as the air cools, and settles where it stood before. By carefully observing at what height above the gage-wire *b*, the descending column first stops, the degree of rarefaction is discovered, which, in great explosions, is very considerable.

I hung in the thermometer, successively, a strip of wet writing paper, a wet flaxen and woolen thread, a blade of green grass, a filament of green wood, a fine silver thread, a very small brass wire, and a strip of gilt paper; and found that the charge of the above-mentioned glass jar, passing through each of these, especially the last, produced heat enough to rarefy the air very perceptibly.

I then suspended, out of the thermometer, a piece of small harpsichord wire, about twenty-four inches long, with a pound weight at the lower end, and sent the charge of the case of five and thirty bottles through it, whereby I discovered a new method of wire-drawing. The wire

was

was red hot the whole length, well annealed, and above an inch longer than before. A second charge melted it; it parted near the middle, and measured, when the ends were put together, four inches longer than at first. This experiment, I remember, you proposed to me before you left *Philadelphia*; but I never tried it till now. That I might have no doubt of the wire's being *hot* as well as red, I repeated the experiment on another piece of the same wire, encompassed with a goose-quill, filled with loose grains of gun-powder; which took fire as readily as if it had been touched with a red hot poker. Also tinder, tied to another piece of the wire, kindled by it. I tried a wire about three times as big, but could produce no such effects with that.

Hence it appears that the electric fire, though it has no sensible heat when in a state of rest, will, by its violent motion, and the resistance it meets with, produce heat in other bodies when passing through them, provided they be small enough. A large quantity will pass through a large wire, without producing any sensible heat; when the same quantity passing through a very small one, being there confined to a narrower passage, the particles crowding closer together, and meeting with greater resistance, will make it red hot, and even melt it.

Hence lightning does not melt metal by a cold fusion, as we formerly supposed; but, when it passes through the blade of a sword, if the quantity be not very great, it may heat

heat the point ſo as to melt it, while the broadeſt and thickeſt part may not be ſenſibly warmer than before.

And when trees or houſes are ſet on fire by the dreadful quantity which a cloud, or the earth, ſometimes diſcharges, muſt not the heat, by which the wood is firſt kindled, be generated by the lightning's violent motion, through the reſiſting combuſtible matter?

If lightning, by its rapid motion, produces heat in *itſelf*, as well as in other bodies, (and that it does I think is evident from ſome of the foregoing experiments made with the thermometer) then its ſometimes ſingeing the hair of animals killed by it, may eaſily be accounted for. And the reaſon of its not always doing ſo, may, perhaps, be this; The quantity, though ſufficient to kill a large animal, may ſometimes not be great enough, or not have met with reſiſtance enough, to become, by its motion, burning hot.

We find that dwelling-houſes, ſtruck with lightning, are ſeldom ſet on fire by it; but when it paſſes through barns, with hay or ſtraw in them, or ſtore-houſes, containing large quantities of hemp, or ſuch like matter, they ſeldom, if ever, eſcape a conflagration; which may, perhaps, be owing to ſuch combuſtibles being apt to kindle with a leſs degree of heat than is neceſſary to kindle wood.

We had four houſes in this city, and a veſſel at one of the wharfs, ſtruck and damaged by lightning laſt ſummer. One of the houſes was ſtruck twice in the ſame ſtorm. But I have the pleaſure to inform you, that your method of of preventing ſuch terrible diſaſters, has, by a fact which had like to have eſcaped our knowledge, given a very convincing proof of its great utility, and is now in higher repute with us than ever.

Hearing, a few days ago, that Mr. *William Weſt*, merchant in this city, ſuſpected that the lightning in one of the thunder-ſtorms laſt ſummer, had paſſed through the iron conductor which he had provided for the ſecurity of his houſe; I waited on him, to enquire what ground he might have for ſuch ſuſpicion. Mr. *Weſt* informed me, that his family and neighbours were all ſtunned with a very terrible exploſion, and that the flaſh and crack were ſeen and heard at the ſame inſtant. Whence he concluded, that the lightning muſt have been very near, and, as no houſe in the neighbourhood had ſuffered by it, that it muſt have paſſed through his conductor. Mr. *White*, his clerk, told me that he was ſitting, at the time, by a window, about two feet diſtant from the conductor, leaning againſt the brick wall with which it was in contact; and that he felt a ſmart ſenſation, like an electrick ſhock, in that part of his body which touched the wall. Mr. *Weſt* further informed me, that a perſon of undoubted veracity aſſured him, that, being in the door of an oppoſite houſe, on the other ſide of *Water-ſtreet*, (which you know is but narrow) he ſaw the lightning diffuſed over the pavement, which was then very wet with rain, to the diſtance of two or three yards from the foot of the conductor; and that

another

another perſon of very good credit told him, that he being a few doors off on the other ſide of the ſtreet, ſaw the lightning above, darting in ſuch direction that it appeared to him to be directly over that pointed rod.

Upon receiving this information, and being deſirous of further ſatisfaction, there being no traces of the lightning to be diſcovered in the conductor, as far as we could examine it below, I propoſed to Mr. *Weſt* our going to the top of the houſe, to examine the pointed rod, aſſuring him, that if the lightning had paſſed through it, the point muſt have been melted ; and, to our great ſatisfaction, we found it ſo. This iron rod extended in height about nine feet and a half above a ſtack of chimneys to which it was fixed, (though I ſuppoſe three or four feet would have been ſufficient.) It was ſomewhat more than half an inch diameter in the thickeſt part, and tapering to the upper end. The conductor, from the lower end of it to the earth, conſiſted of ſquare iron nail-rods, not much above a quarter of an inch thick, connected together by interlinking joints. It extended down the cedar roof to the eaves, and from thence down the wall of the houſe, four ſtory and a half, to the pavement in *Water-ſtreet*, being faſtened to the wall, in ſeveral places, by ſmall iron hooks. The lower end was fixed to a ring, in the top of an iron ſtake that was drove about four or five feet into the ground.

The above-mentioned iron rod had a hole in the top of it, about two inches deep, wherein was inſerted a braſs wire, about two lines thick, and, when firſt put there, about ten inches long, terminating in a very acute point; but now its whole length was no more than ſeven inches and a half, and the top very blunt. Some of the metal appears to be miſſing, the ſlendereſt part of the wire being, as I ſuſpect, conſumed into ſmoke. But ſome of it, where the wire was a little thicker, being only melted by the lightning, ſunk down, while in a fluid ſtate, and formed a rough irregular cap, lower on one ſide than the other, round the upper end of what remained, and became intimately united therewith.

This was all the damage that Mr. *Weſt* ſuſtained by a terrible ſtroke of lightning;—a moſt convincing proof of the great utility of this method of preventing its dreadful effects. Surely it will now be thought as expedient to provide conductors for the lightning, as for the rain.

Mr. *Weſt* was ſo good as to make me a preſent of the melted wire, which I keep as a great curioſity, and long for the pleaſure of ſhewing it to you. In the mean time, I beg your acceptance of the beſt repreſentation I can give of it, which you will find by the ſide of the thermometer, drawn in its full dimenſions as it now appears. The dotted lines above are intended to ſhew the form of the wire before the lightning melted it.

And now, Sir, I moſt heartily congratulate you on the pleaſure you muſt have in finding your great and well-grounded expectations ſo far fulfilled. May this method of of security from the destructive violence of one of the most awful powers of nature, meet with such further success, as to induce every good and grateful heart to bless God for the important discovery! May the benefit thereof be diffused over the whole globe! May it extend to the latest posterity of mankind, and make the name of *FRANKLIN*, like that of *NEWTON*, *immortal*.

I am, Sir, with sincere respect,

Your most obedient and most humble servant,

EBEN. KINNERSLEY.

LETTER XXXVIII.

To Mr. KINNERSLEY, in answer to the foregoing.

SIR, *London, Feb.* 20, 1762.

I Received your ingenious letter of the 12th of *March* last, and thank you cordially for the account you give me of the new experiments you have lately made in Electricity.—It is a subject that still affords me pleasure, though of late I have not much attended to it.

Your second experiment, in which you attempted, without success, to communicate positive electricity by vapour

 ascending

ascending from electrised water, reminds me of one I formerly made, to try if negative electricity might be produced by evaporation only. I placed a large heated brass plate, containing four or five square feet, on an electric stand; a rod of metal, about four feet long, with a bullet at its end, extended from the plate horizontally. A light lock of cotton, suspended by a fine thread from the cieling, hung opposite to, and within an inch of the bullet. I then sprinkled the heated plate with water, which arose fast from it in vapour. If vapour should be disposed to carry off the electrical, as it does the common fire from bodies, I expected the plate would, by losing some of its natural quantity, become negatively electrised. But I could not perceive, by any motion in the cotton, that it was at all affected; nor by any separation of small cork-balls suspended from the plate, could it be observed that the plate was in any manner electrified. Mr. *Canton* here has also found, that two tea-cups, set on electric stands, and filled, one with boiling, the other with cold water, and equally electrified, continued equally so, notwithstanding the plentiful evaporation from the hot water. Your experiment and his agreeing, show another remarkable difference between electric and common fire. For the latter quits most readily the body that contains it, where water, or any other fluid, is evaporating from the surface of that body, and escapes with the vapour. Hence the method long in use in the east, of cooling liquors, by wrapping

wrapping the bottles round with a wet cloth, and exposing them to the wind. Dr. *Cullen*, of *Edinburgh*, has given some experiments of cooling by evaporation; and I was present at one made by Dr. *Hadley*, then professor of chemistry at *Cambridge*, when, by repeatedly wetting the ball of a thermometer with spirit, and quickening the evaporation by the blast of a bellows, the mercury fell from 65, the state of warmth in the common air, to 7, which is 22 degrees below freezing; and, accordingly, from some water mixed with the spirit, or from the breath of the assistants, or both, ice gathered in small spicula round the ball, to the thickness of near a quarter of an inch. To such a degree did the mercury lose the fire it before contained, which, as I imagine, took the opportunity of escaping, in company with the evaporating particles of the spirit, by adhering to those particles.

Your experiment of the *Florence* flask, and boiling water, is very curious. I have repeated it, and found it to succeed as you describe it, in two flasks out of three. The third would not charge when filled with either hot or cold water. I repeated it, because I remembered I had once attempted to make an electric bottle of a *Florence* flask, filled with cold water, but could not charge it at all; which I then imputed to some imperceptible cracks in the small, extremely thin bubbles, of which that glass is full, and I concluded none of that kind would do. But you have shewn me my mistake.—Mr. *Wilson* had formerly

merly acquainted us, that red hot glaſs would conduct electricity; but that ſo ſmall a degree of heat as that communicated by boiling water, would ſo open the pores of extremely thin glaſs, as to ſuffer the electric fluid freely to paſs, was not before known. Some experiments ſimilar to yours, have, however, been made here, before the receipt of your letter, of which I ſhall now give you an account.

I formerly had an opinion that a *Leyden* bottle, charg'd and then ſeal'd hermetically, might retain its electricity for ever; but having afterwards ſome ſuſpicion that poſſibly that ſubtil fluid might, by ſlow imperceptible degrees, ſoak through the glaſs, and in time eſcape, I requeſted ſome of my friends, who had conveniences for doing it, to make trial, whether, after ſome months, the charge of a bottle ſo ſealed would be ſenſibly diminiſhed. Being at *Birmingham*, in *September* 1760, Mr. *Bolton* of that place opened a bottle that had been charged, and its long tube neck hermetically ſealed in the *January* preceding. On breaking off the end of the neck, and introducing a wire into it, we found it poſſeſſed of a conſiderable quantity of electricity, which was diſcharged by a ſnap and ſpark. This bottle had lain near ſeven months on a ſhelf, in a cloſet, in contact with bodies that would undoubtedly have carried off all its electricity, if it could have come readily through the glaſs. Yet as the quantity manifeſted by the diſcharge was not apparently ſo great as might

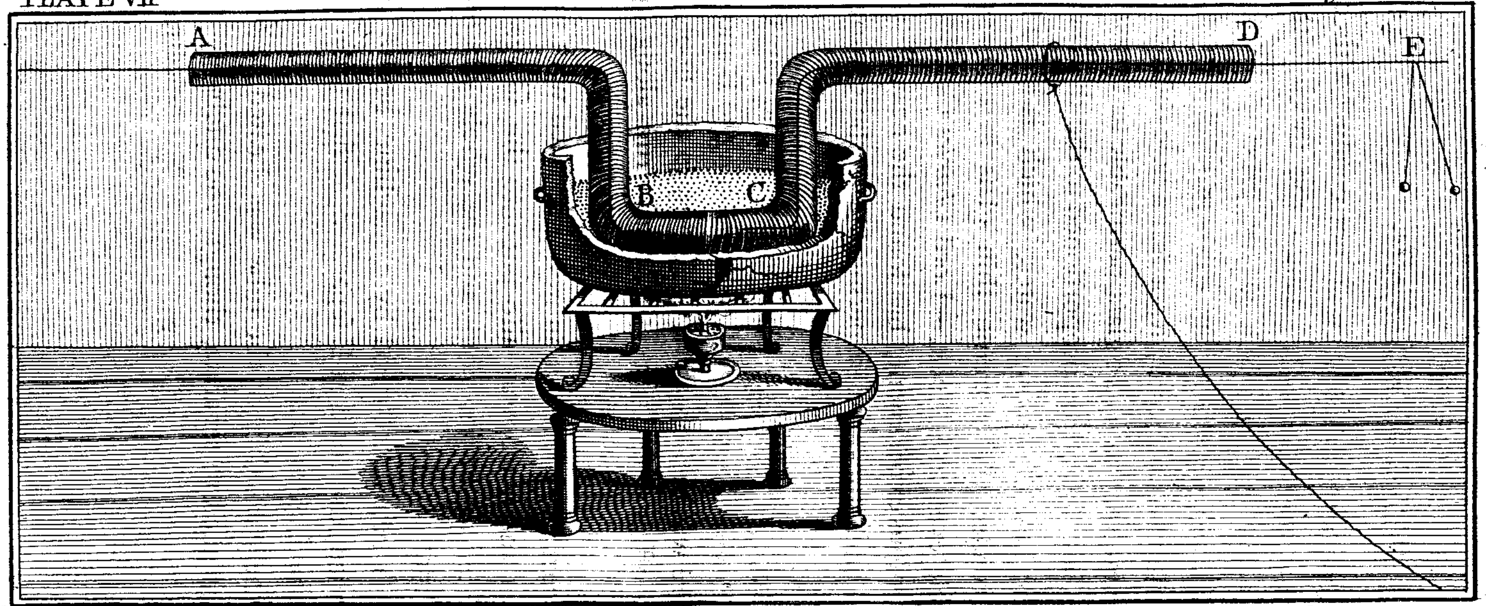
might have been expected from a bottle of that ſize well charged, ſome doubt remained whether part had eſcaped while the neck was ſealing, or had ſince, by degrees, ſoaked through the glaſs. But an experiment of Mr. *Canton's*, in which ſuch a bottle was kept under water a week, without having its electricity in the leaſt impaired, ſeems to ſhow, that when the glaſs is cold, though extremely thin, the electric fluid is well retained by it. As that ingenious and accurate experimenter made a diſcovery, like yours, of the effect of heat in rendering thin glaſs permeable by that fluid, it is but doing him juſtice to give you his account of it, in his own words, extracted from his letter to me, in which he communicated it, dated *Oct.* 31, 1760, *viz.*

" Having procured ſome thin glaſs balls, of about an inch and a half in diameter, with ſtems, or tubes, of eight or nine inches in length, I electrified them, ſome poſitively on the inſide, and others negatively, after the manner of charging the *Leyden* bottle, and ſealed them hermetically. Soon after I applied the naked balls to my electrometer, and could not diſcover the leaſt ſign of their being electrical; but holding them before the fire, at the diſtance of ſix or eight inches, they became ſtrongly electrical in a very ſhort time, and more ſo when they were cooling. Theſe balls will, every time they are heated, give the electrical fluid to, or take it from other bodies, according to the *plus* or *minus* ſtate of it within them. Heating them frequently, I find will ſenſibly diminiſh their power; but keeping

keeping one of them under water a week, did not appear in the least degree to impair it. That which I kept under water, was charged on the 22d of *September* last, was several times heated before it was kept in water, and has been heated frequently since, and yet it still retains its virtue to a very considerable degree. The breaking two of my balls accidentally, gave me an opportunity of measuring their thickness, which I found to be between seven and eight parts in a thousand of an inch.

A down feather, in a thin glass ball, hermetically sealed, will not be affected by the application of an excited tube, or the wire of a charged vial, unless the ball be considerably heated; and if a glass pane be heated till it begins to grow soft, and in that state be held between the wire of a charged vial, and the discharging wire, the course of the electrical fluid will not be through the glass, but on the surface, round by the edge of it."

By this last experiment of Mr. *Canton*'s, it appears, that though by a moderate heat, thin glass becomes, in some degree, a conductor of electricity, yet, when of the thickness of a common pane, it is not, though in a state near melting, so good a conductor as to pass the shock of a discharged bottle. There are other conductors which suffer the electric fluid to pass through them gradually, and yet will not conduct a shock. For instance, a quire of paper will conduct through its whole length, so as to electrify a person, who, standing on wax, presents the paper to an electrified



J. Hulett Sculp.

fied prime conductor; but it will not conduct a ſhock even through its thickneſs only; hence the ſhock either fails, or paſſes by rending a hole in the paper. Thus a ſieve will paſs water gradually, but a ſtream from a fire engine would either be ſtopped by it, or tear a hole through it.

It ſhould ſeem, that to make glaſs permeable to the electric fluid, the heat ſhould be proportioned to the thickneſs. You found the heat of boiling water, which is but 210, ſufficient to render the extreme thin glaſs in a *Florence* flaſk permeable even to a ſhock.—Lord *Charles Cavendiſh*, by a very ingenious experiment, has found the heat of 400 requiſite to render thicker glaſs permeable to the common current.

" A glaſs tube, (See *Plate* VI.) of which the part C B was ſolid, had wire thruſt in each end, reaching to B and C.

A ſmall wire was tied on at D, reaching to the floor, in order to carry off any electricity that might run along upon the tube.

The bent part was placed in an iron pot, filled with iron filings; a thermometer was alſo put into the filings; a lamp was placed under the pot; and the whole was ſupported upon glaſs.

The wire A being electrified by a machine, before the heat was applied, the corks at E ſeparated, at firſt upon the principle of the *Leyden* vial.

But after the part C B of the tube was heated to 600, the corks continued to ſeparate, though you diſcharged the electricity by touching the wire at E, the electrical machine continuing in motion.

Upon letting the whole cool, the effect remained till the thermometer was ſunk to 400."

It were to be wiſhed, that this noble philoſopher would communicate more of his experiments to the world, as he makes many, and with great accuracy.

You know I have always look'd upon and mentioned the equal repulſion in caſes of poſitive and of negative electricity, as a phænomenon difficult to be explained. I have ſometimes, too, been inclined, with you, to reſolve all into attraction; but beſides that attraction ſeems in itſelf as unintelligible as repulſion, there are ſome appearances of repulſion that I cannot ſo eaſily explain by attraction; this for one inſtance. When the pair of cork balls are ſuſpended by flaxen threads, from the end of the prime conductor, if you bring a rubbed glaſs tube near the conductor, but without touching it, you ſee the balls ſeparate, as being electrified poſitively; and yet you have communicated no electricity to the conductor, for, if you had, it would have remained there, after withdrawing the tube; but the cloſing of the balls immediately thereupon, ſhews that the conductor has no more left in it than its natural quantity. Then again approaching the conductor with the rubbed tube, if, while the balls are ſeparated, you touch with a finger that end of the conductor to which they hang, they will come together again, as being, with that part of the

conductor

conductor, brought to the same state with your finger, *i. e.* the natural state. But the other end of the conductor, near which the tube is held, is not in that state, but in the negative state, as appears on removing the tube; for then part of the natural quantity left at the end near the balls, leaving that end to supply what is wanting at the other, the whole conductor is found to be equally in the negative state. Does not this indicate that the electricity of the rubbed tube had repelled the electric fluid, which was diffused in the conductor while in its natural state, and forced it to quit the end to which the tube was brought near, accumulating itself on the end to which the balls were suspended? I own I find it difficult to account for its quitting that end, on the approach of the rubbed tube, but on the supposition of repulsion; for, while the conductor was in the same state with the air, *i. e.* the natural state, it does not seem to me easy to suppose, that an attraction should suddenly take place between the air and the natural quantity of the electric fluid in the conductor, so as to draw it to, and accumulate it on the end opposite to that approached by the tube; since bodies, possessing only their natural quantity of that fluid, are not usually seen to attract each other, or to affect mutually the quantities of electricity each contains.

There are likewise appearances of repulsion in other parts of nature. Not to mention the violent force with which the particles of water, heated to a certain degree, separate from each other, or those of gunpowder, when touch'd with the ſmalleſt ſpark of fire, there is the ſeeming repulſion between the ſame poles of the magnet, a body containing a ſubtle moveable fluid, in many reſpects analagous to the electric fluid. If two magnets are ſo ſuſpended by ſtrings, as that their poles of the ſame denomination are oppoſite to each other, they will ſeparate, and continue ſo; or if you lay a magnetic ſteel bar on a ſmooth table, and approach it with another parallel to it, the poles of both in the ſame poſition, the firſt will recede from the ſecond, ſo as to avoid the contact, and may thus be puſh'd (or at leaſt appear to be puſh'd) off the table. Can this be aſcribed to the attraction of any ſurrounding body or matter drawing them aſunder, or drawing the one away from the other? If not, and repulſion exiſts in nature, and in magnetiſm, why may it not exiſt in electricity? We ſhould not, indeed, multiply cauſes in philoſophy without neceſſity; and the greater ſimplicity of your hypotheſis would recommend it to me, if I could ſee that all appearances might be ſolved by it. But I find, or think I find, the two cauſes more convenient than one of them alone. Thus I would ſolve the circular motion of your horizontal ſtick, ſupported on a pivot, with two pins at their ends, pointing contrary ways, and moving in the ſame direction when electrified, whether poſitively or negatively: When poſitively, the air oppoſite to the points being electriſed poſitively, repels the points; when negatively, the air oppoſite the points being alſo, by their means, electriſed trifed negatively, attraction takes place between the electricity in the air behind the heads of the pins, and the negative pins, and fo they are, in this cafe, drawn in the fame direction that in the other they were driven.—You fee I am willing to meet you half way, a complaifance I have not met with in our brother *Nollet*, or any other hypothefis-maker, and therefore may value myfelf a little upon it, efpecially as they fay I have fome ability in defending even the wrong fide of a queftion, when I think fit to take it in hand.

What you give as an eftablifhed law of the electric fluid, "That quantities of different denfities mutually attract "each other, in order to reftore the equilibrium," is, I think, not well founded, or elfe not well exprefs'd. Two large cork balls, fufpended by filk ftrings, and both well and equally electrified, feparate to a great diftance. By bringing into contact with one of them, another ball of the fame fize, fufpended likewife by filk, you will take from it half its electricity. It will then, indeed, hang at a lefs diftance from the other, but the full and the half quantities will not appear to attract each other, that is, the balls will not come together. Indeed, I do not know any proof we have, that one quantity of electric fluid is attracted by another quantity of that fluid, whatever difference there may be in their denfities. And, fuppofing in nature, a mutual attraction between two parcels of any kind of matter, it would be ftrange if this attraction fhould fubfift ftrongly while

while those parcels were unequal, and ceaſe when more matter of the ſame kind was added to the ſmalleſt parcel, ſo as to make it equal to the biggeſt. By all the laws of attraction in matter, that we are acquainted with, the attraction is ſtronger in proportion to the increaſe of the maſſes, and never in proportion to the difference of the maſſes. I ſhould rather think the law would be, "That the electric fluid is attracted ſtrongly by all other matter that we know of, while the parts of that fluid mutually repel each other." Hence its being equally diffuſed (except in particular circumſtances) throughout all other matter. But this you jokingly call "electrical orthodoxy." It is ſo with ſome at preſent, but not with all; and, perhaps, it may not always be orthodoxy with any body. Opinions are continually varying, where we cannot have mathematical evidence of the nature of things; and they muſt vary. Nor is that variation without its uſe, ſince it occaſions a more thorough diſcuſſion, whereby error is often diſſipated, true knowledge is encreaſed, and its principles become better underſtood and more firmly eſtabliſhed.

Air ſhould have, as you obſerve, "its ſhare of the common ſtock of electricity, as well as glaſs, and, perhaps, all other electrics *per ſe*." But I ſuppoſe, that, like them, it does not eaſily part with what it has, or receive more, unleſs when mix'd with ſome non-electric, as moiſture for inſtance, of which there is ſome in our drieſt air. This, however, is only a ſuppoſition; and your experiment

periment of restoring electricity to a negatively electrised person, by extending his arm upwards into the air, with a needle between his fingers, on the point of which light may be seen in the night, is, indeed, a curious one. In this town the air is generally moister than with us, and here I have seen Mr. *Canton* electrify the air in one room positively, and in another, which communicated by a door, he has electrised the air negatively. The difference was easily discovered by his cork balls, as he passed out of one room into another.——*Pere Beccaria*, too, has a pretty experiment, which shews that air may be electrised. Suspending a pair of small light balls, by flaxen threads, to the end of his prime conductor, he turns his globe some time, electrising positively, the balls diverging and continuing separate all the time. Then he presents the point of a needle to his conductor, which gradually drawing off the electric fluid, the balls approach each other, and touch, before all is drawn from the conductor; opening again as more is drawn off, and separating nearly as wide as at first, when the conductor is reduced to the natural state. By this it appears, that when the balls came together, the air surrounding the balls was just as much electrised as the conductor at that time; and more than the conductor, when that was reduced to its natural state. For the balls, though in the natural state, will diverge, when the air that surrounds them is electrised *plus* or *minus*, as well as when that is in its natural state and they are electrised *plus* or *minus* themselves.

I foresee

I foresee that you will apply this experiment to the support of your hypothesis, and I think you may make a good deal of it.

It was a curious enquiry of yours, Whether the electricity of the air, in clear dry weather, be of the same density at the height of two or three hundred yards, as near the surface of the earth; and I am glad you made the experiment. Upon reflection, it should seem probable, that whether the general state of the atmosphere at any time be positive or negative, that part of it which is next the earth will be nearer the natural state, by having given to the earth in one case, or having received from it in the other. In electrising the air of a room, that which is nearest the walls, or floor, is least altered. There is only one small ambiguity in the experiment, which may be cleared by more trials; it arises from the supposition that bodies may be electrised positively by the friction of air blowing strongly on them, as it does on the kite and its string. If at some times the electricity appears to be negative, as that friction is the same, the effect must be from a negative state of the upper air.

I am much pleased with your electrical thermometer, and the experiments you have made with it. I formerly satisfied myself by an experiment with my phial and syphon, that the elasticity of the air was not increased by the mere existence of an electric atmosphere within the phial; but I did not know, till you now inform me, that heat may be be given to it by an electric explosion. The continuance of its rarefaction, for some time after the discharge of your glass jar and of your case of bottles, seem to make this clear. The other experiments on wet paper, wet thread, green grass, and green wood, are not so satisfactory; as possibly the reducing part of the moisture to vapour, by the electric fluid passing through it, might occasion some expansion which would be gradually reduced by the condensation of such vapour. The fine silver thread, the very small brass wire, and the strip of gilt paper, are also subject to a similar objection, as even metals, in such circumstances, are often partly reduced to smoke, particularly the gilding on paper.

But your subsequent beautiful experiment on the wire, which you made hot by the electric explosion, and in that state fired gunpowder with it, puts it out of all question, that heat is produced by our artificial electricity, and that the melting of metals in that way, is not by what I formerly called a cold fusion. A late instance here, of the melting a bell-wire, in a house struck by lightning, and parts of the wire burning holes in the floor on which they fell, has proved the same with regard to the electricity of nature. I was too easily led into that error by accounts given, even in philosophical books, and from remote ages downwards, of melting money in purses, swords in scabbards, *&c.* without burning the inflammable matters that were so near those melted metals. But men are, in gene-

 ral,

ral, ſuch careleſs obſervers, that a philoſopher cannot be too much on his guard in crediting their relations of things extraordinary, and ſhould never build an hypotheſis on any thing but clear facts and experiments, or it will be in danger of ſoon falling, as this does, like a houſe of cards.

How many ways there are of kindling fire, or producing heat in bodies! By the ſun's rays, by colliſion, by friction, by hammering, by putrefaction, by fermentation, by mixtures of fluids, by mixtures of ſolids with fluids, and by electricity. And yet the fire when produced, though in different bodies it may differ in circumſtances, as in colour, vehemence, *&c.* yet in the ſame bodies is generally the ſame. Does not this ſeem to indicate that the fire exiſted in the body, though in a quieſcent ſtate, before it was by any of theſe means excited, diſengaged, and brought forth to action and to view? May it not conſtitute part, and even a principal part, of the ſolid ſubſtance of bodies? If this ſhould be the caſe, kindling fire in a body would be nothing more than developing this inflammable principle, and ſetting it at liberty to act in ſeparating the parts of that body, which then exhibits the appearances of ſcorching, melting, burning, *&c.* When a man lights an hundred candles from the flame of one, without diminiſhing that flame, can it be properly ſaid to have *communicated* all that fire? When a ſingle ſpark from a flint, applied to a magazine of gunpowder, is immediately attended with this conſequence, that the whole is in flame, exploding with immenſe

immenſe violence, could all this fire exiſt firſt in the ſpark? We cannot conceive it. And thus we ſeem led to this ſuppoſition, that there is fire enough in all bodies to ſinge, melt, or burn them, whenever it is, by any means, ſet at liberty, ſo that it may exert itſelf upon them, or be diſengaged from them. This liberty ſeems to be afforded it by the paſſage of electricity through them, which we know can and does, of itſelf, ſeparate the parts even of water; and perhaps the immediate appearances of fire are only the effects of ſuch ſeparations? If ſo, there would be no need of ſuppoſing that the electric fluid *heats itſelf* by the ſwiftneſs of its motion, or heats bodies by the reſiſtance it meets with in paſſing through them. They would only be heated in proportion as ſuch ſeparation could be more eaſily made. Thus a melting heat cannot be given to a large wire in the flame of a candle, though it may to a ſmall one; and this not becauſe the large wire reſiſts *leſs* that action of the flame which tends to ſeparate its parts, but becauſe it reſiſts it *more* than the ſmaller wire; or becauſe the force being divided among more parts, acts weaker on each.

This reminds me, however, of a little experiment I have frequently made, that ſhews, at one operation, the different effects of the ſame quantity of electric fluid paſſing through different quantities of metal. A ſtrip of tinfoil, three inches long, a quarter of an inch wide at one end, and tapering all the way to a ſharp point at the other, fixed between two pieces of glaſs, and having the electricity of a large glaſs jar ſent through it, will not be diſcompoſed in the broadeſt part; towards the middle will appear melted in ſpots; where narrower, it will be quite melted; and about half an inch of it next the point will be reduced to ſmoke.

You were not miſtaken in ſuppoſing that your account of the effect of the pointed rod, in ſecuring Mr. *Weſt*'s houſe from damage by a ſtroke of lightning, would give me great pleaſure. I thank you for it moſt heartily, and for the pains you have taken in giving me ſo complete a deſcription of its ſituation, form, and ſubſtance, with the draft of the melted point. There is one circumſtance, *viz.* that the lightning was ſeen to diffuſe itſelf from the foot of the rod over the wet pavement, which ſeems, I think, to indicate, that the earth under the pavement was very dry, and that the rod ſhould have been ſunk deeper, till it came to earth moiſter and therefore apter to receive and diſſipate the electric fluid. And although, in this inſtance, a conductor formed of nail rods, not much above a quarter of an inch thick, ſerved well to convey the lightning, yet ſome accounts I have ſeen from *Carolina*, give reaſon to think, that larger may be ſometimes neceſſary, at leaſt for the ſecurity of the conductor itſelf, which, when too ſmall, may be deſtroyed in executing its office, though it does, at the ſame time, preſerve the houſe. Indeed, in the conſtruction of an inſtrument ſo new, and of which we could have ſo little experience, it is rather lucky that we ſhould at firſt be ſo near the truth as we ſeem to be, and commit ſo few errors.

There

There is another reason for sinking deeper the lower end of the rod, and also for turning it outwards under ground to some distance from the foundation; it is this, that water dripping from the eaves falls near the foundation, and sometimes soaks down there in greater quantities, so as to come near the end of the rod though the ground about it be drier. In such case, this water may be exploded, that is, blown into vapour, whereby a force is generated that may damage the foundation. Water reduced to vapour, is said to occupy 14,000 times its former space.—I have sent a charge through a small glass tube, that has borne it well while empty, but when filled first with water, was shattered to pieces and driven all about the room:—Finding no part of the water on the table, I suspected it to have been reduced to vapour; and was confirmed in that suspicion afterwards, when I had filled a like piece of tube with ink, and laid it on a sheet of clean paper, whereon, after the explosion, I could find neither any moisture nor any sully from the ink. This experiment of the explosion of water, which I believe was first made by that most ingenious electrician father *Beccaria*, may account for what we sometimes see in a tree struck by lightning, when part of it is reduced to fine splinters like a broom; the sap vessels being so many tubes containing a watry fluid, which when reduced to vapour, rends every tube lengthways. And perhaps it is this rarefaction of the fluids in animal bodies killed by lightning or electricity, that by separating

ſeparating its fibres, renders the fleſh ſo tender, and apt ſo much ſooner to putrify. I think too, that much of the damage done by lightning to ſtone and brick walls, may ſometimes be owing to the exploſion of water, found, during ſhowers, running or lodging in the joints or ſmall cavities or cracks that happen to be in the walls.

Here are ſome electricians that recommend knobs inſtead of points on the upper end of the rods, from a ſuppoſition that the points invite the ſtroke. It is true that points draw electricity at greater diſtances in the gradual ſilent way; — but knobs will draw at the greateſt diſtance a ſtroke. There is an experiment that will ſettle this. Take a crooked wire of the thickneſs of a quill, and of ſuch a length as that one end of it being applied to the lower part of a charged bottle, the upper may be brought near the ball on the top of the wire that is in the bottle. Let one end of this wire be furniſhed with a knob, and the other be gradually tapered to a fine point. When the point is preſented to diſcharge the bottle it muſt be brought much nearer before it will receive the ſtroke, than the knob requires to be. Points beſides tend to repel the fragments of an electriſed cloud, knobs draw them nearer. An experiment which I believe I have ſhewn you, of cotton fleece hanging from an electriſed body, ſhows this clearly when a point or a knob is preſented under it.

You ſeem to think highly of the importance of this diſcovery, as do many others on our ſide of the water.

Here

Here it is very little regarded; ſo little, that though it is now ſeven or eight years ſince it was made publick, I have not heard of a ſingle houſe as yet attempted to be ſecured by it. It is true the miſchiefs done by lightning are not ſo frequent here as with us, and thoſe who calculate chances may perhaps find that not one death (or the deſtruction of one houſe) in a hundred thouſand happens from that cauſe, and that therefore it is ſcarce worth while to be at any expence to guard againſt it.—But in all countries there are particular ſituations of buildings more expoſed than others to ſuch accidents, and there are minds ſo ſtrongly impreſſed with the apprehenſion of them, as to be very unhappy every time a little thunder is within their hearing;—it may therefore be well to render this little piece of new knowledge as general and as well underſtood as poſſible, ſince to make us *ſafe* is not all its advantage, it is ſome to make us *eaſy*. And as the ſtroke it ſecures us from might have chanced perhaps but once in our lives, while it may relieve us a hundred times from thoſe painful apprehenſions, the latter may poſſibly on the whole contribute more to the happineſs of mankind than the former.

Your kind wiſhes and congratulations are very obliging. I return them cordially;—being with great regard and eſteem,

My dear Sir,

Your affectionate friend,

and moſt obedient humble ſervant,

B. F.

LETTER

LETTER XXXIX.

Accounts from *Carolina (mention'd in the foregoing Letter)* of the effects of Lightning, on two of the Rods commonly affix'd to Houses there, for securing them against Lightning.

Charles-town, Nov. 1, 1760.

" ——It is some Years since Mr. *Raven*'s Rod was struck by lightning. I hear an account of it was published at the time, but I cannot find it. According to the best information I can now get, he had fix'd to the outside of his chimney a large iron Rod, several feet in length, reaching above the chimney; and to the top of this rod the points were fixed. From the lower end of this rod, a small brass wire was continued down to the top of another iron rod driven into the earth. On the ground-floor in the chimney stood a gun, leaning against the back wall, nearly opposite to where the brass wire came down on the outside. The lightning fell upon the points, did no damage to the rod they were fix'd to; but the brass wire, all down till it came opposite to the top of the gun-barrel, was destroyed.*

There

* A proof that it was not of sufficient substance to conduct with safety to itself (tho' with safety *so far* to the wall) so large a quantity of the electric fluid.

There the lightning made a hole through the wall or back of the chimney, to get to the gun-barrel,† down which it seems to have pass'd, as, although it did not hurt the barrel, it damaged the butt of the stock, and blew up some bricks of the hearth. The brass wire below the hole in the wall remain'd good.—No other damage, as I can learn, was done to the house.—I am told the same house had formerly been struck by lightning, and much damaged, before these rods were invented."——

LETTER XL.

Mr. *William Maine*'s Account of the Effects of Lightning on his Rod, dated at *Indian Land*, in *South Carolina*, *Aug.* 28, 1760.

——" I had a set of electrical points, consisting of three prongs, of large brass wire tipt with silver, and perfectly sharp, each about seven inches long; these were riveted at equal distances into an iron nut about three quarters of an inch square, and opened at top equally to the distance of six or seven inches from point to point, in a regular triangle. This nut was screwed very tight on the top of an iron rod of above half an inch diameter, or the thickness of a common

† A more substantial conductor.

curtain rod, compofed of feveral joints, annexed by hooks turned at the ends of each joint, and the whole fixed to the chimney of my houfe by iron ftaples. The points were elevated *(a)*, fix or feven inches above the top of the chimney; and the lower joint funk three feet in the earth, in a perpendicular direction.

Thus ftood the points on Tuefday laft about five in the evening, when the lightning broke with a violent explofion on the chimney, cut the rod fquare off juft under the nutt, and I am perfuaded, melted the points, nut, and top of the rod, entirely up; as after the moft diligent fearch, nothing of either was found *(b)*, and the top of the remaining rod was cafed over with a congealed folder. The lightning ran down the rod, ftarting almoft all the ftaples *(c)*, and unhooking the joints, without affecting the rod *(d)*, except on the infide of each hook where the joints were coupled, the furface of which was melted *(e)*, and left as cafed over with folder.—No part of the chimney was damaged *(f)*, only at the foundation *(g)*, where it was fhattered almoft quite round, and feveral bricks were torn out (*h*). Confiderable cavities were made in the earth quite round the foundation, but moft within eight or nine inches of the rod. It alfo fhattered the bottom weather-board (*i*), at one corner of the houfe, and made a large hole in the earth by the corner poft. On the other fide of the chimney, it ploughed up feveral furrows in the earth, fome yards in length. It ran down the infide of the chimney *(k)*, carrying only foot with

with it; and filled the whole houſe with its flaſh (*l*), ſmoke, and duſt. It tore up the hearth in ſeveral places *(m)*, and broke ſome pieces of china in the beaufet *(n)*. A copper tea kettle ſtanding in the chimney was beat together, as if ſome great weight had fallen upon it *(o)*; and three holes, each about half an inch diameter, melted through the bottom *(p)*. What ſeems to me moſt ſurpriſing is, that the hearth under the kettle was not hurt, yet the bottom of the kettle was drove inward, as if the lightning proceeded from under it upwards *(q)*, and the cover was thrown to the middle of the floor *(r)*. The fire dogs, an iron loggerhead, an Indian pot, an earthen cup, and a cat, were all in the chimney at the time unhurt, though great part of the hearth was torn up *(ſ)*. My wife's ſiſter, two children, and a Negro wench, were all who happened to be in the houſe at the time: The firſt, and one child, ſat within five feet of the chimney; and were ſo ſtunned, that they never ſaw the lightning nor heard the exploſion; the wench, with the other child in her arms, ſitting at a greater diſtance, was ſenſible of both; though every one was ſo ſtunn'd that they did not recover for ſome time; however it pleaſed God that no farther miſchief enſued. The kitchen, at 90 feet diſtance, was full of Negroes, who were all ſenſible of the ſhock; and ſome of them tell me, that they felt the rod about a minute after, when it was ſo hot that they could not bear it in hand."

REMARKS.

The foregoing very ſenſible and diſtinct account may afford a good deal of inſtruction relating to the nature and effects of lightning, and to the conſtruction and uſe of this inſtrument for averting the miſchiefs of it.—Like other new inſtruments, this appears to have been at firſt in ſome reſpects imperfect; and we find that we are, in this as in others, to expect improvement from experience chiefly: But there ſeems to be nothing in the account, that ſhould diſcourage us in the uſe of it; ſince at the ſame time that its imperfections are diſcovered, the means of removing them are pretty eaſily to be learnt from the circumſtances of the account itſelf; and its utility upon the whole is manifeſt.

One intention of the pointed rod, is, to *prevent* a ſtroke of lightning. (*See pages* 126, 162.) But to have a better chance of obtaining this end, the points ſhould not be too near to the top of the chimney or higheſt part of the building to which they are affixed, but ſhould be extended five or ſix feet above it; otherwiſe their operation in ſilently drawing off the fire (from ſuch fragments of cloud as float in the air between the great body of cloud and the earth) will be prevented. For the experiment with the lock of cotton hanging below the electrified prime conductor, ſhews, that a finger under it, being a blunt body, extends the cotton, drawing its lower part downwards; when a needle with its point preſented to the cotton, makes it fly up again to the prime conductor; and that this effect is ſtrongeſt, when as much of of the needle as possible appears above the end of the finger; grows weaker as the needle is shortened between the finger and thumb; and is reduced to nothing when only a short part below the point appears above the finger. Now it seems the points of Mr. *Maine*'s rod were elevated only (*a*) *six or seven inches above the top of the chimney*; which, considering the bulk of the chimney and the house, was too small an elevation. For the great body of matter near them would hinder their being easily brought into a negative state by the repulsive power of the electrised cloud, in which negative state it is that they attract most strongly and copiously the electric fluid from other bodies, and convey it into the earth.

(*b*) *Nothing of the points, &c. could be found.* This is a common effect. (*See page* 163.) Where the quantity of the electric fluid passing is too great for the conductor thro' which it passes, the metal is either melted, or reduced to smoke and dissipated; but where the conductor is sufficiently large, the fluid passes in it without hurting it. Thus these three wires were destroyed, while the rod to which they were fixed, being of greater substance, remained unhurt; its end only, to which they were joined, being a little melted, some of the melted part of the lower ends of those wires uniting with it, and appearing on it like solder.

(*c*) (*d*) (*e*) As the several parts of the rod were connected only by the ends being bent round into hooks, the contact between hook and hook was much smaller than the

the rod; therefore the current through the metal being confin'd in thoſe narrow paſſages, melted part of the metal, as appeared on examining the inſide of each hook. Where metal is melted by lightning, ſome part of it is generally exploded; and theſe exploſions in the joints appear to have been the cauſe of unhooking them; and, by that violent action, of ſtarting alſo moſt of the ſtaples. We learn from hence, that a rod in one continued piece is preferable to one compoſed of links or parts hooked together.

(f) No part of the chimney was damaged; becauſe the lightning paſſed in the rod. And this inſtance agrees with others in ſhewing, that the ſecond and principal intention of the rods is obtainable, viz. that of *conducting* the lightning. In all the inſtances yet known of the lightning's falling on any houſe guarded by rods, it has pitched down upon the point of the rod; and has not fallen upon any other part of the houſe. Had the lightning fallen on this chimney, unfurniſhed with a rod, it would probably have rent it from top to bottom, as we ſee, by the effects of the lightning on the points and rod, that its quantity was very great; and we know that many chimneys have been ſo demoliſhed. But *no part of this was damaged, only (f) (g) (h) at the foundation, where it was ſhattered and ſeveral bricks torn out.* Here we learn the principal defect in fixing this rod. The lower joint being ſunk but three feet into the earth, did not it ſeems go low enough to come at water, or a large body of earth ſo moiſt as to receive readily

dily from its end the quantity it conducted. The electric fluid therefore thus accumulated near the lower end of the rod, quitted it at the ſurface of the earth, dividing in ſearch of other paſſages. Part of it tore up the ſurface in furrows, and made holes in it: Part entered the bricks of the foundation, which being near the earth are generally moiſt, and, in exploding that moiſture, ſhattered them. (*See page* 415.) Part went through or under the foundation, and got under the hearth, blowing up great part of the bricks *(m) (ſ)*, and producing the other effects *(o) (p) (q) (r)*. The iron dogs, loggerhead and iron pot were not hurt, being of ſufficient ſubſtance, and they probably protected the cat. The copper tea kettle being thin, ſuffered ſome damage. Perhaps, tho' found on a ſound part of the hearth, it might at the time of the ſtroke have ſtood on the part blown up, which will account both for the bruiſing and melting.

That *it ran down the inſide of the chimney (k)* I apprehend muſt be a miſtake. Had it done ſo, I imagine it would have brought ſomething more than ſoot with it; it would probably have ripp'd off the pargetting, and brought down fragments of plaiſter and bricks. The ſhake, from the exploſion on the rod, was ſufficient to ſhake down a good deal of looſe ſoot. Lightning does not uſually enter houſes by the doors, windows, or chimneys, as open paſſages, in the manner that air enters them: Its nature is, to be attracted by ſubſtances, that are conductors of electricity; it penetrates and paſſes *in* them, and, if they are not good conductors, as are nei-

ther

ther wood, brick, ftone nor plaifter, it is apt to rend them in its paffage. It would not eafily pafs thro' the air from a cloud to a building, were it not for the aid afforded it in its paffage by intervening fragments of clouds below the main body, or by the falling rain.

It is faid that *the houfe was filled with its flafh (l)*. Expreffions like this are common in accounts of the effects of lightning, from which we are apt to underftand that the lightning filled the houfe. Our language indeed feems to want a word to exprefs the *light* of lightning as diftinct from the lightning itfelf. When a tree on a hill is ftruck by it, the lightning of that ftroke exifts only in a narrow vein between the cloud and tree, but its light fills a vaft fpace many miles round; and people at the greateft diftance from it are apt to fay, "the lightning came into our rooms through our windows." As it is in itfelf extreamly bright, it cannot, when fo near as to ftrike a houfe, fail illuminating highly every room in it through the windows; and this I fuppofe to have been the cafe at Mr. *Maine*'s; and that, except in and near the hearth, from the caufes abovementioned, it was not in any other part of the houfe; *the flafh* meaning no more than *the light* of the lightning.--- It is for want of confidering this difference, that people fuppofe there is a kind of lightning not attended with thunder. In fact there is probably a loud explofion accompanying every flafh of lightning, and at the fame inftant;--- but as found travels flower than light, we often hear the

found

found ſome ſeconds of time after having ſeen the light; and as ſound does not travel ſo far as light, we ſometimes ſee the light at a diſtance too great to hear the ſound.

(*n*) The *breaking ſome pieces of china in the beaufet*, may nevertheleſs ſeem to indicate that the lightning was there: But as there is no mention of its having hurt any part of the beaufet, or of the walls of the houſe, I ſhould rather aſcribe that effect to the concuſſion of the air, or ſhake of the houſe by the exploſion.

Thus, to me it appears, that the houſe and its inhabitants were ſaved by the rod, though the rod itſelf was unjointed by the ſtroke; and that, if it had been made of one piece, and ſunk deeper in the earth, or had entered the earth at a greater diſtance from the foundation, the mentioned ſmall damages (except the melting of the points) would not have happened.

LETTER XLI.

Saturday, July 3, 1762.

TO try, at the requeſt of a friend, whether amber finely powdered might be melted and run together again by means of the electric fluid, I took a piece of ſmall glaſs tube about 2½ inches long, the bore about $\frac{1}{12}$ of an inch diameter, the glaſs itſelf about the ſame thickneſs; I introduced into this tube ſome powder of amber, and with two pieces of wire nearly fitting the bore, one inſerted at one end, the other at the other, I rammed the powder hard between them in the middle of the tube, where it ſtuck faſt, and was in length about half an inch. Then leaving the wires in the tube, I made them part of the electric circuit, and diſcharged through them three rows of my caſe of bottles. The event was, that the glaſs was broke into very ſmall pieces and thoſe diſperſed with violence in all directions. As I did not expect this, I had not, as in other experiments, laid thick paper over the glaſs to ſave my eyes, ſo ſeveral of the pieces ſtruck my face ſmartly, and one of them cut my lip a little ſo as to make it bleed. I could find no part of the amber; but the table where the tube lay was ſtained very black in ſpots, ſuch as might be made by a thick ſmoke forced on it by a blaſt, and the air was filled with a ſtrong ſmell, ſomewhat like that from burnt gunpowder. Whence I imagined, that the amber was burnt, and had exploded as gunpowder would have done in the ſame circumſtances.

That I might better ſee the effect on the amber, I made the next experiment in a tube formed of a card rolled up and bound ſtrongly with packthread. Its bore was about $\frac{1}{8}$ of an inch diameter. I rammed powder of amber into this as I had done in the other, and as the quantity of amber was greater, I increaſed the quantity of electric fluid, by diſcharging through it at once 5 rows of my bottles. On opening the tube, I found that ſome of the powder

powder had exploded, an impression was made on the tube though it was not burst, and most of the powder remaining was turned black, which I suppose might be by the smoke forced through it from the burnt part: Some of it was hard; but as it powdered again when pressed by the fingers, I suppose that hardness not to arise from melting any parts in it, but merely from my ramming the powder when I charged the tube.

B. F.

LETTER XLII.

To the Rev. Father BECCARIA.

Rev. S I R, *London, July* 13, 1762.

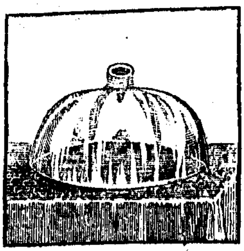
I Once promised myself the pleasure of seeing you at *Turin*, but as that is not now likely to happen, being just about returning to my native country, *America*, I sit down to take leave of you (among others of my *European* friends that I cannot see) by writing.

I thank you for the honourable mention you have so frequently made of me in your letters to Mr. *Collinson* and others, for the generous defence you undertook and executed with ſo much ſucceſs, of my electrical opinions; and for the valuable preſent you have made me of your new work, from which I have received great information and pleaſure. I wiſh I could in return entertain you with any thing new of mine on that ſubject; but I have not lately purſued it. Nor do I know of any one here that is at preſent much engaged in it.

Perhaps, however, it may be agreeable to you, as you live in a muſical country, to have an account of the new inſtrument lately added here to the great number that charming ſcience was before poſſeſſed of:——As it is an inſtrument that ſeems peculiarly adapted to *Italian* muſic, eſpecially that of the ſoft and plaintive kind, I will endeavour to give you ſuch a deſcription of it, and of the manner of conſtructing it, that you, or any of your friends, may be enabled to imitate it, if you incline ſo to do, without being at the expence and trouble of the many experiments I have made in endeavouring to bring it to its preſent perfection.

You have doubtleſs heard the ſweet tone that is drawn from a drinking glaſs, by paſſing a wet finger round its brim. One Mr. *Puckeridge*, a gentleman from *Ireland*, was the firſt who thought of playing tunes, formed of theſe tones. He collected a number of glaſſes of different ſizes, fixed them near each other on a table, and tuned them by putting into them water, more or leſs, as each note required. The tones were brought out by paſſing his fingers round their

their brims.---He was unfortunately burnt here, with his instrument, in a fire which consumed the house he lived in. Mr. *E. Delaval*, a most ingenious member of our Royal Society, made one in imitation of it, with a better choice and form of glasses, which was the first I saw or heard. Being charmed with the sweetness of its tones, and the music he produced from it, I wished only to see the glasses disposed in a more convenient form, and brought together in a narrower compass, so as to admit of a greater number of tones, and all within reach of hand to a person sitting before the instrument, which I accomplished, after various intermediate trials, and less commodious forms, both of glasses and construction, in the following manner.



The glasses are blown as near as possible in the form of hemispheres, having each an open neck or socket in the middle. The thickness of the glass near the brim about a tenth of an inch, or hardly quite so much, but thicker as it comes nearer the neck, which in the largest glasses is about an inch deep, and an inch and half wide within, these dimensions lessening as the glasses themselves diminish in size, except that the neck of the smallest ought not to be shorter than half an inch. — The largest glass is nine inches diameter, and the smallest three inches. Between these there are twenty-three different sizes, differing from each other a quarter of an inch in diameter. — To make a a ſingle inſtrument there ſhould be at leaſt ſix glaſſes blown of each ſize; and out of this number one may probably pick 37 glaſſes, (which are ſufficient for 3 octaves with all the ſemitones) that will be each either the note one wants or a little ſharper than that note, and all fitting ſo well into each other as to taper pretty regularly from the largeſt to the ſmalleſt. It is true there are not 37 ſizes, but it often happens that two of the ſame ſize differ a note or half note in tone, by reaſon of a difference in thickneſs, and theſe may be placed one in the other without ſenſibly hurting the regularity of the taper form.

The glaſſes being choſen and every one marked with a diamond the note you intend it for, they are to be tuned by diminiſhing the thickneſs of thoſe that are too ſharp. This is done by grinding them round from the neck towards the brim, the breadth of one or two inches as may be required; often trying the glaſs by a well tuned harpſichord, comparing the tone drawn from the glaſs by your finger, with the note you want, as ſounded by that ſtring of the harpſichord. When you come near the matter, be careful to wipe the glaſs clean and dry before each trial, becauſe the tone is ſomething flatter when the glaſs is wet, than it will be when dry;—and grinding a very little between each trial, you will thereby tune to great exactneſs. The more care is neceſſary in this, becauſe if you go below your required tone, there is no ſharpening it again but

but by grinding somewhat off the brim, which will afterwards require polishing, and thus encrease the trouble.

The glasses being thus tuned, you are to be provided with a case for them, and a spindle on which they are to be fixed. My case is about three feet long, eleven inches every way wide within at the biggest end, and five inches at the smallest end; for it tapers all the way, to adapt it better to the conical figure of the set of glasses. This case opens in the middle of its height, and the upper part turns up by hinges fixed behind. The spindle which is of hard iron, lies horizontally from end to end of the box within, exactly in the middle, and is made to turn on brass gudgeons at each end. It is round, an inch diameter at the thickest end, and tapering to a quarter of an inch at the smallest. — A square shank comes from its thickest end through the box, on which shank a wheel is fixed by a screw. This wheel serves as a fly to make the motion equable, when the spindle, with the glasses, is turned by the foot like a spinning wheel. My wheel is of mahogany, 18 inches diameter, and pretty thick, so as to conceal near its circumference about 25 lb of lead. — An ivory pin is fixed in the face of this wheel and about 4 inches from the axis. Over the neck of this pin is put the loop of the string that comes up from the moveable step to give it motion. The case stands on a neat frame with four legs.

To fix the glasses on the spindle, a cork is first to be fitted in each neck pretty tight, and projecting a little without

without the neck, that the neck of one may not touch the inside of another when put together, for that would make a jarring. — These corks are to be perforated with holes of different diameters, so as to suit that part of the spindle on which they are to be fixed. When a glass is put on, by holding it stiffly between both hands, while another turns the spindle, it may be gradually brought to its place. But care must be taken that the hole be not too small, lest in forcing it up the neck should split; nor too large, lest the glass not being firmly fixed, should turn or move on the spindle, so as to touch and jar against its neighbouring glass. The glasses thus are placed one in another, the largest on the biggest end of the spindle which is to the left hand; the neck of this glass is towards the wheel, and the next goes into it in the same position, only about an inch of its brim appearing beyond the brim of the first; thus proceeding, every glass when fixed shows about an inch of its brim, (or three quarters of an inch, or half an inch, as they grow smaller) beyond the brim of the glass that contains it; and it is from these exposed parts of each glass that the tone is drawn, by laying a finger upon one of them as the spindle and glasses turn round.

My largest glass is G a little below the reach of a common voice, and my highest G, including three compleat octaves. — To distinguish the glasses the more readily to the eye, I have painted the apparent parts of the glasses within side, every semitone white, and the other notes of the

the octave with the ſeven priſmatic colours, *viz.* C, red; D, orange; E, yellow; F, green; G, blue; A, Indigo; B, purple; and C, red again; — ſo that glaſſes of the ſame colour (the white excepted) are always octaves to each other.

This inſtrument is played upon, by ſitting before the middle of the ſet of glaſſes as before the keys of a harpſichord, turning them with the foot, and wetting them now and then with a ſpunge and clean water. The fingers ſhould be firſt a little ſoaked in water and quite free from all greaſineſs; a little fine chalk upon them is ſometimes uſeful, to make them catch the glaſs and bring out the tone more readily. Both hands are uſed, by which means different parts are played together.---Obſerve, that the tones are beſt drawn out when the glaſſes turn *from* the ends of the fingers, not when they turn *to* them.

The advantages of this inſtrument are, that its tones are incomparably ſweet beyond thoſe of any other; that they may be ſwelled and ſoftened at pleaſure by ſtronger or weaker preſſures of the finger, and continued to any length; and that the inſtrument, being once well tuned, never again wants tuning.

In honour of your muſical language, I have borrowed from it the name of this inſtrument, calling it the *Armonica*.

With great eſteem and reſpect, I am, &c.

LETTER XLIII.

From Professor WINTHROP, to B. F.

SIR, *Cambridge, N. E. Sept.* 29, 1762.

THERE is an obſervation relating to electricity in the atmoſphere, which ſeemed new to me, though perhaps it will not to you: However, I will venture to mention it. I have ſome points on the top of my houſe, and the wire where it paſſes within-ſide the houſe is furniſhed with bells, according to your method, to give notice of the paſſage of the electric fluid. In ſummer, theſe bells generally ring at the approach of a thunder cloud; but ceaſe ſoon after it begins to rain. In winter, they ſometimes, though not very often, ring while it is ſnowing; but never, that I remember, when it rains. But what was unexpected to me was, that, though the bells had not rung while it was ſnowing, yet the next day, after it had done ſnowing, and the weather was cleared up; while the ſnow was driven about by a high wind at W. or N.W. the bells rung for ſeveral hours (though with little intermiſſions) as briſkly as ever I knew them, and I drew conſiderable ſparks from the wire. This phænomenon I never obſerved but twice; *viz.* on the 31ſt of *January*, 1760, and the 3d of *March*, 1762.

I am, Sir, &c.

LET-

LETTER XLIV.

TO a FRIEND.

Dear SIR, *July* 20, 1762.

I Have peruſed your paper on ſound, and would freely mention to you, as you deſire it, every thing that appeared to me to need correction :—But nothing of that kind occurs to me, unleſs it be, where you ſpeak of the air as "the *beſt* medium for conveying ſound." Perhaps this is, ſpeaking rather too poſitively, if there be, as I think there are, ſome other mediums that will convey it farther and more readily. – It is a well-known experiment, that the ſcratching of a pin at one end of a long piece of timber, may be heard by an ear applied near the other end, though it could not be heard at the ſame diſtance through the air. —— And two ſtones being ſtruck ſmartly together under water, the ſtroke may be heard at a greater diſtance by an ear alſo placed under water in the ſame river, than it can be heard through the air. I think I have heard it near a mile; how much farther it may be heard, I know not; but ſuppoſe a great deal farther, becauſe the ſound did not ſeem faint, as if at a diſtance, like diſtant ſounds through air, but ſmart and ſtrong, and as if preſent juſt at the ear.——I wiſh you would repeat theſe experiments now you are upon the ſubject, and add your own obſervations. —— And if you were to repeat, with your naturally exact attention and obſervation, the common experiment of the bell in the exhauſted receiver, poſſibly ſomething new may occur to you, in conſidering,

1. Whether the experiment is not ambiguous; *i. e.* whether the gradual exhauſting of the air, as it creates an increaſing difference of preſſure on the outſide, may not occaſion in the glaſs a difficulty of vibrating, that renders it leſs fit to communicate to the air without, the vibrations that ſtrike it from within; and the diminution of the ſound ariſe from this cauſe, rather than from the diminution of the air?

2. Whether as the particles of air themſelves are at a diſtance from each other, there muſt not be ſome medium between them, proper for conveying ſound, ſince otherwiſe it would ſtop at the firſt particle?

3. Whether the great difference we experience in hearing ſounds at a diſtance, when the wind blows towards us from the ſonorous body, or towards that from us, can be well accounted for by adding to or ſubſtracting from the ſwiftneſs of ſound, the degree of ſwiftneſs that is in the wind at the time? The latter is ſo ſmall in proportion, that it ſeems as if it could ſcarce produce any ſenſible effect, and yet the difference is very great. Does not this give ſome hint, as if there might be a ſubtile fluid, the conductor of ſound, which moves at different times in different directions over the ſurface of the earth, and whoſe

whose motion may perhaps be much swifter than that of the air in our strongest winds; and that in passing through air, it may communicate that motion to the air which we call wind, though a motion in no degree so swift as its own?

4. It is somewhere related, that a pistol fired on the top of an exceeding high mountain, made a noise like thunder in the valleys below. Perhaps this fact is not exactly related: but if it is, would not one imagine from it, that the rarer the air, the greater sound might be produced in it from the same cause?

5. Those balls of fire which are sometimes seen passing over a country, computed by philosophers to be often 30 miles high at least, sometimes burst at that height; the air must be exceeding rare there, and yet the explosion produces a sound that is heard at that distance, and for 70 miles round on the surface of the earth, so violent too as to shake buildings, and give an apprehension of an earthquake. Does not this look as if a rare atmosphere, almost a vacuum, was no bad conductor of sound?

I have not made up my own mind on these points, and only mention them for your consideration, knowing that every subject is the better for your handling it.

With the greatest esteem, I am, &c. B. F.

LET-

LETTER XLV.

To Dr. P. in London.

SIR, *Philadelphia, Dec.* 1, 1762.

DURING our paſſage to Madeira, the weather being warm, and the cabbin windows conſtantly open for the benefit of the air, the candles at night flared and run very much, which was an inconvenience. At Madeira we got oil to burn, and with a common glaſs tumbler or beaker, ſlung in wire, and ſuſpended to the cieling of the cabbin, and a little wire hoop for the wick, furniſh'd with corks to float on the oil, I made an Italian lamp, that gave us very good light all over the table.—The glaſs at bottom contained water to about one third of its height; another third was taken up with oil; the reſt was left empty that the ſides of the glaſs might protect the flame from the wind. There is nothing remarkable in all this; but what follows is particular. At ſupper, looking on the lamp, I remarked that tho' the ſurface of the oil was perfectly tranquil, and duly preſerved its poſition and diſtance with regard to the brim of the glaſs, the water under the oil was in great commotion, riſing and falling in irregular waves, which continued

continued during the whole evening. The lamp was kept burning as a watch light all night, till the oil was ſpent, and the water only remain'd. In the morning I obſerved, that though the motion of the ſhip continued the ſame, the water was now quiet, and its ſurface as tranquil as that of the oil had been the evening before. At night again, when oil was put upon it, the water reſum'd its irregular motions, riſing in high waves almoſt to the ſurface of the oil, but without diſturbing the ſmooth level of that ſurface. And this was repeated every day during the voyage.

Since my arrival in America, I have repeated the experiment frequently thus. I have put a pack-thread round a tumbler, with ſtrings of the ſame, from each ſide, meeting above it in a knot at about a foot diſtance from the top of the tumbler. Then putting in as much water as would fill about one third part of the tumbler, I lifted it up by the knot, and ſwung it to and fro in the air; when the the water appeared to keep its place in the tumbler as ſteadily as if it had been ice.—But pouring gently in upon the water about as much oil, and then again ſwinging it in the air as before, the tranquility before poſſeſſed by the water, was transferred to the ſurface of the oil, and the water under it was agitated with the ſame commotions as at ſea.

I have ſhewn this experiment to a number of ingenious perſons. Thoſe who are but ſlightly acquainted with the principles of hydroſtatics, &c. are apt to fancy immediately that they underſtand it, and readily attempt to explain

it;

it; but their explanations have been different; and *to me* not very intelligible.—Others more deeply ſkill'd in thoſe principles, ſeem to wonder at it, and promiſe to conſider it. And I think it is worth conſidering: For a new appearance, if it cannot be explain'd by our old principles, may afford us new ones, of uſe perhaps in explaining ſome other obſcure parts of natural knowledge.

I am, &c. B. F.

LETTER XLVI.

From Mr. A. S. to B. F.

I Have juſt recollected that in one of our great ſtorms of lightning, I ſaw an appearance, which I never obſerved before, nor ever heard deſcribed. I am perſuaded that I ſaw *the* flaſh which ſtruck St. Bride's ſteeple. Sitting at my window, and looking to the north, I ſaw what appeared to me a ſolid ſtreight rod of fire, moving at a very ſharp angle with the horizon. It appeared to my eye as about two inches diameter, and had nothing of the zig-zag lightning motion. I inſtantly told a perſon ſitting with me, that ſome

ſome place muſt be ſtruck at that inſtant. I was ſo much ſurprized at the vivid diſtinct appearance of the fire, that I did not hear the clap of thunder, which ſtunned every one beſides. Conſidering how low it moved, I could not have thought it had gone ſo far, having St. Martin's, the New Church, and St. Clement's ſteeples in its way. It ſtruck the ſteeple a good way from the top, and the firſt impreſſion it made in the ſide is in the ſame direction I ſaw it move in. It was ſucceeded by two flaſhes, almoſt united, moving in a pointed direction. There were two diſtinct houſes ſtruck in Eſſex ſtreet. I ſhould have thought the rod would have fallen in Covent Garden, it was ſo low. Perhaps the appearance is frequent, though never before ſeen by

Yours, A. S.

LETTER XLVII.

To Mr. P. F. Newport.

——— You may acquaint the gentleman that deſired you to enquire my opinion of the beſt method of ſecuring a powder magazine from lightning, that I think they can-

 not

not do better than to erect a mast not far from it, which may reach 15 or 20 feet above the top of it, with a thick iron rod in one piece fastened to it, pointed at the highest end, and reaching down through the earth till it comes to water. Iron is a cheap metal; but if it were dearer, as this is a publick thing, the expence is insignificant; therefore I would have the rod at least an inch thick, to allow for its gradually wasting by rust; it will last as long as the mast, and may be renewed with it. The sharp point for five or six inches should be gilt.

But there is another circumstance of importance to the strength, goodness and usefulness of the powder, which does not seem to have been enough attended to: I mean the keeping it perfectly dry. For want of a method of doing this, much is spoilt in damp magazines, and much so damaged as to become of little value.—If instead of barrels it were kept in cases of bottles well cork'd; or in large tin canisters, with small covers shutting close by means of oil'd paper between, or covering the joining on the canister; or if in barrels, then the barrels lined with thin sheet lead; no moisture in either of these methods could possibly enter the powder, since glass and metals are both impervious to water.

By the latter of these means you see tea is brought dry and crisp from China to Europe, and thence to America, tho' it comes all the way by sea in the damp hold of a ship.

And

And by this method, grain, meal, &c. if well dry'd before 'tis put up, may be kept for ages sound and good.

There is another thing very proper to line small barrels with; it is what they call tin-foil, or leaf-tin, being tin mill'd between rollers till it becomes as thin as paper, and more pliant, at the same time that its texture is extreamly close. It may be apply'd to the wood with common paste, made with boiling water thicken'd with flour; and, so laid on, will lie very close and stick well: But I should prefer a hard sticky varnish for that purpose, made of linseed oil much boil'd. The heads might be lined separately, the tin wrapping a little round their edges. The barrel while the lining is laid on, should have the end hoops slack, so that the staves standing at a little distance from each other, may admit the head into its groove. The tin-foil should be plyed into the groove. Then one head being put in, and that end hoop'd tight, the barrel would be fit to receive the powder, and when the other head is put in and the hoops drove up, the powder would be safe from moisture even if the barrel were kept under water. This tin-foil but about 18 pence sterling a pound, and is so extreamly thin, that I imagine a pound of it would line three or four powder barrels.

I am &c. B. F.

LETTER XLVIII.

To Miſs *S———n*, at *Wanſtead.*

Craven-ſtreet, May 17, 1760.

I Send my dear good girl the books I mentioned to her laſt night. I beg her to accept them as a ſmall mark of my eſteem and friendſhip. They are written in the familiar eaſy manner for which the French are ſo remarkable, and afford a good deal of philoſophic and practical knowledge, unembarras'd with the dry mathematics uſed by more exact reaſoners, but which is apt to diſcourage young beginners.—I would adviſe you to read with a pen in your hand, and enter in a little book ſhort hints of what you find that is curious, or that may be uſeful; for this will be the beſt method of imprinting ſuch particulars in your memory, where they will be ready, either for practice on ſome future occaſion, if they are matters of utility; or at leaſt to adorn and improve your converſarion, if they are rather points of curioſity.—And, as many of the terms of ſcience are ſuch as you cannot have met with in your common reading, and may therefore be unacquainted with, I think it would be well for you to have a good dictionary at hand, to conſult immediately when you meet with a word you

you do not comprehend the precise meaning of. This may at first seem troublesome and interrupting; but 'tis a trouble that will daily diminish, as you will daily find less and less occasion for your Dictionary as you become more acquainted with the terms; and in the mean time you will read with more satisfaction because with more understanding.—When any point occurs in which you would be glad to have farther information than your book affords you, I beg you would not in the least apprehend that I should think it a trouble to receive and answer your questions. It will be a pleasure, and no trouble. For though I may not be able, out of my own little stock of knowledge to afford you what you require, I can easily direct you to the books where it may most readily be found. Adieu, and believe me ever, my dear friend,

Yours affectionately,
B. FRANKLIN.

LETTER XLIX.

To the same.

Craven-street, June 11, 1760.

'TIS a very sensible question you ask, how the air can affect the barometer, when its opening appears covered with wood?—If indeed it was so closely covered as to admit of no communication of the outward air to the ſurface of the mercury, the change of weight in the air could not poſſibly affect it. But the leaſt crevice is ſufficient for the purpoſe; a pinhole will do the buſineſs. And if you could look behind the frame to which your barometer is fixed, you would certainly find ſome ſmall opening.

There are indeed ſome barometers in which the body of mercury at the lower end is contained in a cloſe leather bag, and ſo the air cannot come into immediate contact with the mercury; yet the ſame effect is produced. For the leather being flexible, when the bag is preſſed by any additional weight of air, it contracts, and the mercury is forced up into the tube;—when the air becomes lighter, and its preſſure leſs, the weight of the mercury prevails, and it deſcends again into the bag.

Your obſervation on what you have lately read concerning inſects, is very juſt and ſolid. Superficial minds are apt to deſpiſe thoſe who make that part of the creation their ſtudy, as mere triflers; but certainly the world has been much obliged to them. Under the care and management of man, the labours of the little Silkworm afford employment and ſubſiſtence to thouſands of families, and become an immenſe article of commerce. The Bee, too, yields us its delicious honey, and its wax uſeful to a multitude of purpoſes. Another inſect, it is ſaid, produces the Cochineal, from whence we have our rich ſcarlet dye.
The

The usefulness of the Cantharides or Spanish flies, in medicine, is known to all, and thousands owe their lives to that knowledge. By human industry and observation, other properties of other insects may possibly be hereafter discovered, and of equal utility. A thorough acquaintance with the nature of these little creatures, may also enable mankind to prevent the increase of such as are noxious or secure us against the mischiefs they occasion. These things doubtless your books make mention of: I can only add a particular late instance which I had from a Swedish gentleman of good credit.—In the green timber intended for ship-building at the king's yards in that country, a kind of worms were found, which every year became more numerous and more pernicious, so that the ships were greatly damaged before they came into use. The king sent Linnæus, the great naturalist, from Stockholm, to enquire into the affair, and see if the mischief was capable of any remedy. He found, on examination, that the worm was produced from a small egg, deposited in the little roughnesses on the surface of the wood, by a particular kind of fly or beetle; from whence the worm, as soon as it was hatch'd, began to eat into the substance of the wood, and after some time came out again a fly of the parent kind, and so the species increas'd. The season in which the fly laid its eggs, Linnæus knew to be about a fortnight (I think) in the month of May, and at no other time in the year. He therefore advis'd, that some days before that

season

ſeaſon, all the green timber ſhould be thrown into the water, and kept under water till the ſeaſon was over. Which being done by the king's order, the flies miſſing their uſual neſts, could not increaſe; and the ſpecies was either deſtroyed or went elſewhere; and the wood was effectually preſerved, for after the firſt year, it became too dry and hard for their purpoſe.

There is, however, a prudent moderation to be uſed in ſtudies of this kind. The knowledge of nature may be ornamental, and it may be uſeful; but if to attain an eminence in that, we neglect the knowledge and practice of eſſential duties, we deſerve reprehenſion. For there is no rank in natural knowledge of equal dignity and importance with that of being a good parent, a good child, a good huſband, or wife, a good neighbour or friend, a good ſubject or citizen, that is, in ſhort, a good chriſtian. Nicholas Gimcrack, therefore, who neglected the care of his family, to purſue butterflies, was a juſt object of ridicule, and we muſt give him up as fair game to the ſatyriſt.

Adieu, my dear friend, and believe me ever

Yours affectionately,

B. FRANKLIN.

LETTER L.

To the same.

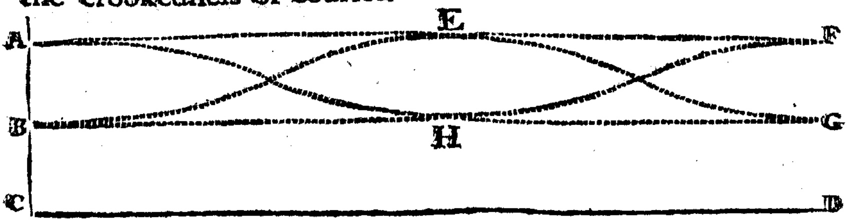
My dear Friend, *London, Sept. 13, 1760.*

I HAVE your agreeable letter from *Bristol*, which I take this first leisure hour to answer, having for sometime been much engaged in business.

Your first question, *What is the reason the water at this place, though cold at the spring, becomes warm by pumping?* It will be most prudent in me to forbear attempting to answer, till, by a more circumstantial account, you assure me of the fact. I own I should expect that operation to warm, not so much the water pumped, as the person pumping.—The rubbing of dry solids together, has been long observed to produce heat; but the like effect has never yet, that I have heard, been produced by the mere agitation of fluids, or friction of fluids with solids. Water in a bottle shook for hours by a mill hopper, it is said, discovered no sensible addition of heat. The production of animal heat by exercise, is therefore to be accounted for in another manner, which I may hereafter endeavour to make you acquainted with.

This prudence of not attempting to give reasons before one is sure of facts, I learnt from one of your sex, who, as *Selden* tells us, being in company with some gentlemen that were viewing, and considering something which they called a Chinese shoe, and disputing earnestly about the manner of wearing it, and how it could possibly be put on; put in her word, and said modestly, *Gentlemen, are you sure it is a shoe?—Should not that be settled first?*

But I shall now endeavour to explain what I said to you about the tide in rivers, and to that end shall make a figure, which though not very like a river, may serve to convey my meaning.—Suppose a canal 140 miles long, communicating at one end with the sea, and filled therefore with sea water. I chuse a canal at first, rather than a river, to throw out of consideration the effects produced by the streams of fresh water from the land, the inequality in breadth, and the crookedness of courses.



Let A, C, be the head of the canal; C, D, the bottom of it; D, F, the open mouth of it next the sea. Let the strait prick'd line, B, G, represent low water mark the whole length of the canal, A, F, high water mark:—Now if a person standing at E, and observing at the time of high water there, that the canal is quite full at that place up to the

the line E, ſhould conclude that the canal is equally full to the ſame height from end to end, and therefore there was as much more water come into the canal ſince it was down at low water mark, as would be included in the oblong ſpace A, B, G, F, he would be greatly miſtaken. For the tide is *a Wave*, and the top of the wave, which makes high water, as well as every other lower part, is progreſſive; and it is high water ſucceſſively, but not at the ſame time, in all the ſeveral points between G, F, and A, B.—And in ſuch a length as I have mentioned it is low water at F, G, and alſo at A, B, at or near the ſame time with its being high water at E; ſo that the ſurface of the water in the canal, during that ſituation, is properly repreſented by the curve pricked line B, E, G. And on the other hand, when it is low water at E, H, it is high water both at F, G, and at A, B, at or near the ſame time; and the ſurface would then be deſcribed by the inverted curve line, A, H, F.

In this view of the caſe, you will eaſily ſee, that there muſt be very little more water in the canal at what we call high water, than there is at low water, thoſe terms not relating to the whole canal at the ſame time, but ſucceſſively to its parts. And if you ſuppoſe the canal ſix times as long, the caſe would not vary as to the quantity of water at different times of the tide; there would only be ſix waves in the canal at the ſame time, inſtead of one, and the hollows in the water would be equal to the hills.

That this is not mere theory, but conformable to fact, we know by our long rivers in *America*. The *Delaware*, on which *Philadelphia* ftands, is in this particular fimilar to the canal I have fuppofed of one wave: For when it is high water at the *Capes* or mouth of the river, it is alfo high water at *Philadelphia*, which ftands about 140 miles from the fea; and there is at the fame time a low water in the middle between the two high waters; where, when it comes to be high water, it is at the fame time low water at the *Capes* and at *Philadelphia*. And the longer rivers have, fome a wave and half, fome two, three, or four waves, according to their length.—In the fhorter rivers of this ifland, one may fee the fame thing in part: for inftance, it is high water at *Gravefend* an hour before it is high water at *London Bridge*; and 20 miles below *Gravefend* an hour before it is high water at *Gravefend*. Therefore at the time of high water at *Gravefend* the top of the wave is there, and the water is then not fo high by fome feet where the top of the wave was an hour before, or where it will be an hour after, as it is juft then at *Gravefend*.

Now we are not to fuppofe, that becaufe the fwell or top of the wave runs at the rate of 20 miles an hour, that therefore the current or water itfelf of which the wave is compofed, runs at that rate. Far from it. To conceive this motion of a wave, make a fmall experiment or two. Faften one end of a cord in a window near the top of a houfe, and let the other end come down to the ground; take this end in

your

your hand, and you may, by a ſudden motion occaſion a wave in the cord that will run quite up to the window; but though the wave is progreſſive from your hand to the window, the parts of the rope do not proceed with the wave, but remain where they were, except only that kind of motion that produces the wave.—So if you throw a ſtone into a pond of water when the ſurface is ſtill and ſmooth, you will ſee a circular wave proceed from the ſtone as its center, quite to the ſides of the pond; but the water does not proceed with the wave, it only riſes and falls to form it in the different parts of its courſe; and the waves that follow the firſt, all make uſe of the ſame water with their predeceſſors.

But a wave in water is not indeed in all circumſtances exactly like that in a cord; for water being a fluid, and gravitating to the earth, it naturally runs from a higher place to a lower; therefore the parts of the wave in water do actually run a little both ways from its top towards its lower ſides, which the parts of the wave in the cord cannot do. Thus when it is high and ſtanding water at *Graveſend*, the water 20 miles below has been running ebb, or towards the ſea for an hour, or ever ſince it was high water there; but the water at *London Bridge* will run flood, or from the ſea yet another hour, till it is high water or the top of the wave arrives at that bridge, and then it will have run ebb an hour at *Graveſend*, &c. &c. Now this motion of the water, occaſioned only by its gravity, or tendency to run from a higher

place

place to a lower, is by no means ſo ſwift as the motion of the wave. It ſcarce exceeds perhaps two miles in an hour. If it went as the wave does twenty miles an hour, no ſhips could ride at anchor in ſuch a ſtream, nor boats row againſt it.

In common ſpeech, indeed, this current of the water both ways from the top of the wave is called *the tide*; thus we ſay, *the tide runs ſtrong*, *the tide runs at the rate of one, two, or three miles an hour*, &c. and when we are at a part of the river behind the top of the wave, and find the water lower than high-water mark, and running towards the ſea we ſay, *the tide runs ebb*; and when we are before the top of the wave, and find the water higher than low-water mark, and running from the ſea, we ſay, the *tide runs flood*; but theſe expreſſions are only locally proper; for a tide ſtrictly ſpeaking, is *one whole wave*, including all its parts higher and lower, and theſe waves ſucceed one another about twice in 24 hours.

This motion of the water occaſioned by its gravity, will explain to you why the water near the mouths of rivers may be ſalter at high water than at low. Some of the ſalt water, as the tide wave enters the river, runs from its top and fore ſide, and mixes with the freſh, and alſo puſhes it back up the river.

Suppoſing that the water commonly runs during the flood at the rate of two miles in an hour, and that the flood runs five hours, you ſee that it can bring at moſt into our canal only

only a quantity of water equal to the ſpace included in the breadth of the canal, ten miles of its length, and the depth between low and high-water mark; which is but a fourteenth part of what would be neceſſary to fill all the ſpace between low and high-water mark, for 140 miles, the whole length of the canal.

And indeed ſuch a quantity of water as would fill that whole ſpace, to run in and out every tide, muſt create ſo outrageous a current as would do infinite damage to the ſhores, ſhipping, &c. and make the navigation of a river almoſt impracticable.

I have made this letter longer than I intended, and therefore reſerve for another what I have further to ſay on the ſubject of tides and rivers. I ſhall now only add, that I have not been exact in the numbers, becauſe I would avoid perplexing you with minute calculations, my deſign at preſent being chiefly to give you diſtinct and clear ideas of the firſt principles.

After writing ſix folio pages of philoſophy to a young girl, is it neceſſary to finiſh ſuch a letter with a compliment?—Is not ſuch a letter of itſelf a compliment?—Does it not ſay, ſhe has a mind thirſty after knowledge, and capable of receiving it; and that the moſt agreeable things one can write to her are thoſe that tend to the improvement of her underſtanding?—It does indeed ſay all this, but then it is ſtill no compliment; it is no more than plain honeſt truth, which is not the character of a compliment. So if I

would

would finiſh my letter in the mode, I ſhould yet add ſome thing that means nothing, and is *merely* civil and polite.— But being naturally aukward at every circumſtance of ceremony, I ſhall not attempt it. I had rather conclude abruptly with what pleaſes me more than any compliment can pleaſe you, that I am allowed to ſubſcribe myſelf

Your affectionate friend,

B. FRANKLIN.

LETTER LI.

To the ſame.

My dear Friend, *Craven-ſtr. Monday March* 30, 1761.

SUPPOSING the fact, that the water of the well at *Briſtol* is warmer after ſometime pumping, I think your manner of accounting for that increaſed warmth very ingenious and probable. It did not occur to me, and therefore I doubted of the fact.

You are, I think, quite right in your opinion, that the riſing of the tides in rivers is not owing to the immediate influence

influence of the moon on the rivers. It is rather a ſubſequent effect of the influence of the moon on the ſea, and does not make its appearance in ſome rivers till the moon has long paſs'd by. I have not expreſs'd myſelf clearly if you have underſtood me to mean otherwiſe. You know I have mentioned it as a fact, that there are in ſome rivers ſeveral tides all exiſting at the ſame time; that is, two, three, or more, high-waters, and as many low-waters, in different parts of the ſame river, which cannot poſſibly be all effects of the moon's immediate action on that river; but they may be ſubſequent effects of her action on the ſea.

In the encloſed paper you will find my ſentiments on ſeveral points relating to the air, and the evaporation of water. It is Mr. Collinſon's copy, who took it from one I ſent thro' his hands to a correſpondent in *France* ſome years ſince; I have, as he deſired me, corrected the miſtakes he made in tranſcribing, and muſt return it to him; but if you think it worth while, you may take a copy of it: I would have ſaved you any trouble of that kind, but had not time.

Some day in the next or the following week, I purpoſe to have the pleaſure of ſeeing you at *Wanſtead*; I ſhall accompany your good mama thither, and ſtay till the next morning, if it may be done without incommoding your family too much.—We may then diſcourſe any points in that paper that do not ſeem clear to you; and taking a walk to lord *Tilney*'s ponds, make a few experiments there to explain the nature of the tides more fully. In the mean time, believe me to be, with the higheſt eſteem and regard, your ſincerely affectionate friend,

B. FRANKLIN.

LETTER LII.

To the ſame.

Cravenſtr. Aug. 10, 1761.

WE are to ſet out this week for *Holland*, where we may poſſibly ſpend a month, but purpoſe to be at home again before the coronation. I could not go without taking leave of you by a line at leaſt, when I am ſo many letters in your debt.

In yours of *May* 19, which I have before me, you ſpeak of the eaſe with which ſalt water may be made freſh by diſtillation, ſuppoſing it to be, as I had ſaid, that in evaporation the air would take up water but not the ſalt that was mixed

mixed with it. It is true that diſtilled ſea water will not be ſalt, but there are other diſagreeable qualities that riſe with the water in diſtillation; which indeed ſeveral beſides Dr. *Hales* have endeavoured by ſome means to prevent; but as yet their methods have not been brought much into uſe.

I have a ſingular opinion on this ſubject, which I will venture to communicate to you, though I doubt you will rank it among my whims.—It is certain that the ſkin has *imbibing* as well as *diſcharging* pores; witneſs the effects of a bliſtering plaiſter, &c. I have read that a man hired by a phyſician to ſtand by way of experiment in the open air naked during a moiſt night, weighed near three pounds heavier in the morning. I have often obſerved myſelf, that however thirſty I may have been before going into the water to ſwim, I am never long ſo in the water. Theſe imbibing pores, however, are very fine, perhaps fine enough in filtring to ſeparate ſalt from water; for though I have ſoaked (by ſwimming, when a boy) ſeveral hours in the day for ſeveral days ſucceſſively in ſalt-water, I never found my blood and juices ſalted by that means, ſo as to make me thirſty or feel a ſalt taſte in my mouth: And it is remarkable that the fleſh of ſea fiſh, though bred in ſalt water is not ſalt.—Hence I imagine, that if people at ſea, diſtreſſed by thirſt when their freſh water is unfortunately ſpent, would make bathing-tubs of their empty water caſks, and filling them with ſea water, ſit in them an hour or two each day, they might be greatly relieved. Perhaps keeping their cloaths conſtantly wet might have an almoſt equal effect; and this without danger of catching cold. Men do not catch cold by wet cloaths at ſea. Damp but not wet linen may poſſibly give colds; but no one catches cold by bathing, and no cloaths can be wetter than water itſelf. Why damp cloaths ſhould then occaſion colds, is a curious queſtion, the diſcuſſion of which I reſerve for a future letter, or ſome future converſation.

Adieu, my little philoſopher. Preſent my reſpectful compliments to the good ladies your aunts, and to miſs Pitt; and believe me ever

Your affectionate friend,
and humble Servant,
B. FRANKLIN.

LETTER LIII.

To the ſame.

London, March 22, 1762.

I MUST retract the charge of idleneſs in your ſtudies, when I find you have gone thro' the doubly difficult taſk of reading ſo big a book, on an abſtruſe ſubject and in a foreign language.

In

In answer to your question concerning the *Leiden* phial; —The hand that holds the bottle receives and conducts away the electric fluid that is driven out of the outside by the repulsive power of that which is forced into the inside of the bottle. As long as that power remains in the same situation, it must prevent the return of what it had expelled; though the hand would readily supply the quantity if it could be received.

Your affectionate Friend,
B. FRANKLIN.

LETTER LIV.

To the same.

Craven-street, Saturday Evening, past 10.

THE question you ask me is a very sensible one, and I shall be glad if I can give you a satisfactory answer. There are two ways of contracting a chimney; one, by contracting the opening *before* the fire; the other, by contracting the funnel *above* the fire. If the funnel above the fire is left open in its full dimensions, and the opening before

fore the fire is contracted; then the coals, I imagine, will burn faster, becauſe more air is directed through the fire, and in a ſtronger ſtream; that air which before paſſed over it, and on each ſide of it, now paſſing *thro'* it. This is ſeen in narrow ſtove chimneys, when a ſacheverell or blower is uſed, which ſtill more contracts the narrow opening.—But if the funnel only *above* the fire is contracted, then, as a leſs ſtream of air is paſſing up the chimney, leſs muſt paſs through the fire, and conſequently it ſhould ſeem that the conſuming of the coals would rather be checked than augmented by ſuch contraction. And this will alſo be the caſe, when both the opening *before* the fire, and the funnel *above* the fire are contracted, provided the funnel above the fire is more contracted in proportion than the opening before the fire.——So you ſee I think you had the beſt of the argument; and as you notwithſtanding gave it up in complaiſance to the company, I think you had alſo the beſt of the diſpute. There are few, though convinced, that know how to give up, even an error, they have been once engaged in maintaining; there is therefore the more merit in dropping a conteſt where one thinks one's ſelf right; 'tis at leaſt reſpectful to thoſe we converſe with. And indeed all our knowledge is ſo imperfect, and we are from a thouſand cauſes ſo perpetually ſubject to miſtake and error, that poſitiveneſs can ſcarce ever become even the moſt knowing; and modeſty in advancing any opinion, however plain and true we may ſuppoſe it, is always

always decent, and generally more likely to procure assent. *Pope*'s Rule

To speak, though sure, with seeming diffidence,

is therefore a good one; and if I had ever seen in your conversation the least deviation from it, I should earnestly recommend it to your observation.

I am, &c.

B. FRANKLIN.

LETTER LV.

To Mr. O. N.

Dear SIR,

I Cannot be of opinion with you that 'tis too late in life for you to learn to swim. The river near the bottom of your garden affords you a most convenient place for the purpose. And as your new employment requires your being often on the water, of which you have such a dread, I think you would do well to make the trial; nothing being so likely to remove those apprehensions as the consciousness of an ability to swim to the shore, in case of an accident,

accident, or of ſupporting yourſelf in the water till a boat could come to take you up.

I do not know how far corks or bladders may be uſeful in learning to ſwim, having never ſeen much trial of them. Poſſibly they may be of ſervice in ſupporting the body while you are learning what is called the 'ſtroke,' or that manner of drawing in and ſtriking out the hands and feet that is neceſſary to produce progreſſive motion. But you will be no ſwimmer till you can place ſome confidence in the power of the water to ſupport you; I would therefore adviſe the acquiring that confidence in the firſt place; eſpecially as I have known ſeveral who by a little of the practice neceſſary for that purpoſe, have inſenſibly acquired the ſtroke, taught as it were by nature.

The practice I mean is this. Chuſing a place where the water deepens gradually, walk coolly into it till it is up to your breaſt, then turn round, your face to the ſhore, and throw an egg into the water between you and the ſhore. It will ſink to the bottom, and be eaſily ſeen there, as your water is clear. It muſt lie in water ſo deep as that you cannot reach it to take it up but by diving for it. To encourage yourſelf in undertaking to do this, reflect that your progreſs will be from deeper to ſhallower water, and that at any time you may by bringing your legs under you and ſtanding on the bottom, raiſe your head far above the water. Then plunge under it with your eyes open, throwing yourſelf towards the egg, and endeavouring by the action of your hands and feet againſt the water to get forward till within reach of it. In this attempt you will find, that the water buoys you up againſt your inclination; that it is not ſo eaſy a thing to ſink as you imagined; that you cannot, but by active force, get down to the egg. Thus you feel the power of the water to ſupport you, and learn to confide in that power; while your endeavours to overcome it and to reach the egg, teach you the manner of acting on the water with your feet and hands, which action is afterwards uſed in ſwimming to ſupport your head higher above water, or to go forward through it.

I would the more earneſtly preſs you to the trial of this method, becauſe, though I think I ſatisfyed you that your body is lighter than water, and that you might float in it a long time with your mouth free for breathing, if you would put yourſelf in a proper poſture, and would be ſtill and forbear ſtruggling; yet till you have obtained this experimental confidence in the water, I cannot depend on your having the neceſſary preſence of mind to recollect that poſture and the directions I gave you relating to it. The ſurprize may put all out of your mind. For though we value ourſelves on being reaſonable knowing creatures, reaſon and knowledge ſeem on ſuch occaſions to be of little uſe to us; and the brutes to whom we allow ſcarce a glimmering of either, appear to have the advantage of us.

I will, however, take this opportunity of repeating thoſe particulars to you, which I mentioned in our laſt converſation, as by perufing them at your leifure, you may poffibly imprint them fo in your memory as on occafion to be of fome ufe to you.

1. That though the legs, arms and head, of a human body, being folid parts, are fpecifically fomething heavier than frefh water, yet the trunk, particularly the upper part from its hollownefs, is fo much lighter than water, as that the whole of the body taken together is too light to fink wholly under water, but fome part will remain above, untill the lungs become filled with water, which happens from drawing water into them inftead of air, when a perfon in the fright attempts breathing while the mouth and noftrils are under water.

2. That the legs and arms are fpecifically lighter than falt-water, and will be fupported by it, fo that a human body would not fink in falt-water, though the lungs were filled as above, but from the greater fpecific gravity of the head.

3. That therefore a perfon throwing himfelf on his back in falt-water, and extending his arms, may eafily lie fo as to keep his mouth and noftrils free for breathing; and by a fmall motion of his hands may prevent turning, if he fhould perceive any tendency to it.

4. That in frefh water, if a man throws himfelf on his back, near the furface, he cannot long continue in that fituation but by proper action of his hands on the water. If he ufes no fuch action, the legs and lower part of the body will

will gradually ſink till he comes into an upright poſition, in which he will continue ſuſpended, the hollow of the breaſt keeping the head uppermoſt.

5. But if in this erect poſition, the head is kept upright above the ſhoulders, as when we ſtand on the ground, the immerſion will, by the weight of that part of the head that is out of water, reach above the mouth and noſtrils, perhaps a little above the eyes, ſo that a man cannot long remain ſuſpended in water with his head in that poſition.

6. The body continuing ſuſpended as before, and upright; if the head be leaned quite back, ſo that the face looks upwards, all the back part of the head being then under water, and its weight conſequently in a great meaſure ſupported by it, the face will remain above water quite free for breathing, will riſe an inch higher every inſpiration, and ſink as much every expiration, but never ſo low as that the water may come over the mouth.

7. If therefore a perſon unacquainted with ſwimming, and falling accidentally into the water, could have preſence of mind ſufficient to avoid ſtruggling and plunging, and to let the body take this natural poſition, he might continue long ſafe from drowning till perhaps help would come. For as to the cloathes, their additional weight while immerſed is very inconſiderable, the water ſupporting it; though when he comes out of the water, he would find them very heavy indeed.

But, as I ſaid before, I would not adviſe you or any one to depend on having this preſence of mind on ſuch an occaſion, but learn fairly to ſwim; as I wiſh all men were taught to do in their youth; they would, on many occurrences, be the ſafer for having that ſkill, and on many more the happier, as freer from painful apprehenſions of danger, to ſay nothing of the enjoyment in ſo delightful and wholeſome an exerciſe. Soldiers particularly ſhould, methinks, all be taught to ſwim; it might be of frequent uſe either in ſurpriſing an enemy, or ſaving themſelves. And if I had now boys to educate, I ſhould prefer thoſe ſchools (other things being equal) where an opportunity was afforded for acquiring ſo advantageous an art, which once learnt is never forgotten.

I am, Sir, &c.

B. F.

LETTER LVI.

To Miſs S-------n, at *Wanſtead.*

My Dear Friend, *Sept.* 20, 1761.

IT is, as you obſerved in our late converſation, a very general opinion, that *all rivers run into the ſea,* or depoſite their waters there. 'Tis a kind of audacity to call ſuch general opinions in queſtion, and may ſubject one to cenſure. But we muſt hazard ſomething in what we think the cauſe of truth: And if we propoſe our objections modeſtly, we ſhall, tho' miſtaken, deſerve a cenſure leſs ſevere, than when we are both miſtaken and inſolent.

That ſome rivers run into the ſea is beyond a doubt: Such, for inſtance, are the *Amazones,* and I think the *Oronoko* and the *Miſſiſipi.* The proof is, that their waters are freſh quite to the ſea, and out to ſome diſtance from the land. Our queſtion is, whether the freſh waters of thoſe rivers whoſe beds are filled with ſalt water to a conſiderable diſtance up from the ſea (as the *Thames,* the *Delaware,* and the rivers that communicate with *Cheſapeak-bay* in *Virginia*) do ever arrive at the ſea? And as I ſuſpect they

they do not, I am now to acquaint you with my reaſons; or, if they are not allowed to be reaſons, my conceptions at leaſt, of this matter.

The common ſupply of rivers is from ſprings, which draw their origin from rain that has ſoaked into the earth. The union of a number of ſprings forms a river. The waters as they run, expoſed to the ſun, air and wind, are continually evaporating. Hence in travelling one may often ſee where a river runs, by a long blueiſh miſt over it, tho' we are at ſuch a diſtance as not to ſee the river itſelf. The quantity of this evaporation is greater or leſs, in proportion to the ſurface expoſed by the ſame quantity of water to thoſe cauſes of evaporation. While the river runs in a narrow confined channel in the upper hilly country, only a ſmall ſurface is expoſed; a greater as the river widens. Now if a river ends in a lake, as ſome do, whereby its waters are ſpread ſo wide as that the evaporation is equal to the ſum of all its ſprings, that lake will never over-flow:—And if inſtead of ending in a lake, it was drawn into greater length as a river, ſo as to expoſe a ſurface equal in the whole to that lake, the evaporation would be equal, and ſuch river would end as a canal; when the ignorant might ſuppoſe, as they actually do in ſuch caſes, that the river loſes itſelf by running under ground, whereas in truth it has run up into the air.

Now, many rivers that are open to the ſea, widen much before they arrive at it, not merely by the additional waters they

they receive, but by having their course stopt by the opposing flood-tide; by being turned back twice in twenty-four hours, and by finding broader beds in the low flat countries to dilate themselves in; hence the evaporation of the fresh water is proportionably increased; so that in some rivers it may equal the springs of supply. In such cases, the salt water comes up the river, and meets the fresh in that part where, if there were a wall or bank of earth across from side to side, the river would form a lake, fuller indeed at some times than at others, according to the seasons, but whose evaporation would, one time with another, be equal to its supply.

When the communication between the two kinds of water is open, this supposed wall of separation may be conceived as a moveable one, which is not only pushed some miles higher up the river by every flood tide from the sea, and carried down again as far by every tide of ebb, but which has even this space of vibration removed nearer to the sea in wet seasons, when the springs and brooks in the upper country are augmented by the falling rains, so as to swell the river, and farther from the sea in dry seasons.

Within a few miles above and below this moveable line of separation, the different waters mix a little, partly by their motion to and fro, and partly from the greater specific gravity of the salt water, which inclines it to run under the fresh, while the fresh water, being lighter, runs over the salt.

Caſt your eye on the map of *North-America*, and obſerve the bay of *Cheſapeak* in *Virginia*, mentioned above; you will ſee, communicating with it by their mouths, the great rivers *Saſquehanah*, *Potowmack*, *Rappahanock*, *York*, and *James*, beſides a number of ſmaller ſtreams, each as big as the *Thames*. It has been propoſed by philoſophical writers, that to compute how much water any river diſcharges into the ſea, in a given time, we ſhould meaſure its depth and ſwiftneſs at any part above the tide; as, for the *Thames*, at *Kingſton* or *Windſor*. But can one imagine, that if all the water of thoſe vaſt rivers went to the ſea, it would not firſt have puſhed the ſalt water out of that narrow-mouthed bay, and filled it with freſh?—The *Saſquehanah* alone would ſeem to be ſufficient for this, if it were not for the loſs by evaporation. And yet that bay is ſalt quite up to *Annapolis*.

As to our other ſubject, the different degrees of heat imbibed from the ſun's rays by cloths of different colours, ſince I cannot find the notes of my experiment to ſend you, I muſt give it as well as I can from memory.

But firſt let me mention an experiment you may eaſily make yourſelf. Walk but a quarter of an hour in your garden when the ſun ſhines, with a part of your dreſs white, and a part black; then apply your hand to them alternately, and you will find a very great difference in their warmth. The black will be quite hot to the touch, the white ſtill cool.

Another.

Another. Try to fire paper with a burning glaſs. If it is white, you will not eaſily burn it;—but if you bring the focus to a black ſpot, or upon letters, written or printed, the paper will immediately be on fire under the letters.

Thus fullers and dyers find black cloths, of equal thickneſs with white ones, and hung out equally wet, dry in the ſun much ſooner than the white, being more readily heated by the ſun's rays. It is the ſame before a fire; the heat of which ſooner penetrates black ſtockings than white ones, and ſo is apt ſooner to burn a man's ſhins. Alſo beer much ſooner warms in a black mug ſet before the fire, than in a white one, or in a bright ſilver tankard.

My experiment was this. I took a number of little ſquare pieces of broad cloth from a taylor's pattern card, of various colours. There were black, deep blue, lighter blue, green, purple, red, yellow, white, and other colours, or ſhades of colours. I laid them all out upon the ſnow in a bright ſun-ſhiny morning. In a few hours (I cannot now be exact as to the time) the black being warm'd moſt by the ſun, was ſunk ſo low as to be below the ſtroke of the ſun's rays; the dark blue almoſt as low, the lighter blue not quite ſo much as the dark, the other colours leſs as they were lighter; and the quite white remained on the ſurface of the ſnow, not having entered it at all.

What ſignifies philoſophy that does not apply to ſome uſe?—May we not learn from hence, that black clothes are not ſo fit to wear in a hot ſunny climate or ſeaſon, as white ones; becauſe in ſuch clothes the body is more heated by the ſun when we walk abroad, and are at the ſame time heated by the exerciſe, which double heat is apt to bring on putrid dangerous fevers? That ſoldiers and ſeamen who muſt march and labour in the ſun, ſhould in the *Eaſt* or *Weſt-Indies* have an uniform of white? That ſummer hats for men or women, ſhould be white, as repelling that heat which gives head-achs to many, and to ſome the fatal ſtroke that the French call the *Coup de Soleil?* That the ladies Summer hats, however, ſhould be lined with black, as not reverberating on their faces thoſe rays which are reflected upwards from the earth or water? That the putting a white cap of paper or linen *within* the crown of a black hat, as ſome do, will not keep out the heat, tho' it would if plac'd *without.* That fruit walls being blacked may receive ſo much heat from the ſun in the day-time, as to continue warm in ſome degree thro' the night, and thereby preſerve the fruit from froſts, or forward its growth?—with ſundry other particulars of leſs or greater importance, that will occur from time to time to attentive minds?—

I am,

Yours affectionately,

B. FRANKLIN.

LETTER LVII.

Extract of a Letter to Lord *K.* at *Edinburgh*, *June* 2, 1765.

* * * In my paſſage to America I read your excellent work, the *Elements of Criticiſm*, in which I found great entertainment. I only wiſhed you had examined more fully the ſubject of muſick, and demonſtrated that the pleaſure artiſts feel in hearing much of that compoſed in the modern taſte, is not the natural pleaſure ariſing from melody or harmony of ſounds, but of the ſame kind with the pleaſure we feel on ſeeing the ſurprizing feats of tumblers and rope-dancers, who execute difficult things. For my part I take this to be really the caſe, and ſuppoſe it the reaſon why thoſe who are unpractiſed in muſick, and therefore unacquainted with thoſe difficulties, have little or no pleaſure in hearing this muſick. Many pieces of it are mere compoſitions of tricks. I have ſometimes at a concert, attended by a common audience, placed myſelf ſo as to ſee all their faces, and obſerved no ſigns of pleaſure in them during the performance of a great part

that was admired by the performers themſelves; while a plain old Scotch tune, which they diſdained, and could ſcarcely be prevailed on to play, gave manifeſt and general delight. Give me leave on this occaſion to extend a little the ſenſe of your poſition, That "Melody and Harmony are ſeparately agreable, and in union delightful," and to give it as my opinion that the reaſon why the Scotch tunes have lived ſo long, and will probably live for ever (if they eſcape being ſtifled in modern affected ornament) is merely this, that they are really compoſitions of melody and harmony united, or rather that their melody is harmony. I mean the ſimple tunes ſung by a ſingle voice. As this will appear parodoxical, I muſt explain my meaning. In common acceptation, indeed, only an agreable *ſucceſſion* of ſounds is called *melody*, and only the *co-exiſtence* of agreable ſounds, *harmony*. But ſince the memory is capable of retaining for ſome moments a perfect idea of the pitch of a paſt ſound, ſo as to compare with it the pitch of a ſucceeding ſound, and judge truly of their agreement or diſagreement, there may and does ariſe from thence a ſenſe of harmony between the preſent and paſt ſounds, equally pleaſing with that between two preſent ſounds. Now the conſtruction of the old Scotch tunes is this, that almoſt every ſucceeding emphatical note, is a third, a fifth, an octave, or in ſhort ſome note that is in concord with the preceding note. Thirds are chiefly uſed, which are very pleaſing concords. I uſe the word *emphatical* to diſtinguiſh

thoſe

thoſe notes which have a ſtreſs laid on them in ſinging the tune, from the lighter connecting notes, that ſerve merely, like grammar articles in common ſpeech, to tack the whole together.

That we have a moſt perfect idea of a ſound juſt paſt, I might appeal to all acquainted with muſick, who know how eaſy it is to repeat a ſound in the ſame pitch with one juſt heard. In tuning an inſtrument, a good ear can as eaſily determine that two ſtrings are in uniſon by ſounding them ſeparately, as by ſounding them together; their diſagreement is alſo as eaſily, I believe I may ſay more eaſily and better diſtinguiſhed when ſounded ſeparately; for when ſounded together, tho' you know by the beating that one is higher than the other, you cannot tell which it is. I have aſcribed to memory the ability of comparing the pitch of a preſent tone with that of one paſt. But if there ſhould be, as poſſibly there may be, ſomething in the ear ſimilar to what we find in the eye, that ability would not be entirely owing to memory. Poſſibly the vibrations given to the auditory nerves by a particular ſound may actually continue ſome time after the cauſe of thoſe vibrations is paſt, and the agreement or diſagreement of a ſubſequent ſound become by compariſon with them more diſcernible. For the impreſſion made on the viſual nerves by a luminous object will continue for twenty or thirty ſeconds. Sitting in a room look earneſtly at the middle of a window a little while when the day is bright, and then ſhut your eyes; the

the figure of the window will ſtill remain in the eye, and ſo diſtinct that you may count the panes. A remarkable circumſtance attending this experiment, is, that the impreſſion of forms is better retained than that of colours; for after the eyes are ſhut, when you firſt diſcern the image of the window, the panes appear dark, and the croſs bars of the ſaſhes, with the window frames and walls, appear white or bright; but if you ſtill add to the darkneſs in the eyes by covering them with your hand, the reverſe inſtantly takes place, the panes appear luminous and the croſs bars dark. And by removing the hand they are again reverſed. This I know not how to account for.—Nor for the following; that after looking long thro' green ſpectacles, the white paper of a book will on firſt taking them off appear to have a bluſh of red; and after long looking thro' red glaſſes, a greeniſh caſt; this ſeems to intimate a relation between green and red not yet explained. Farther, when we conſider by whom theſe ancient tunes were compoſed, and how they were firſt performed, we ſhall ſee that ſuch harmonical ſucceſſions of ſounds was natural and even neceſſary in their conſtruction. They were compoſed by the minſtrels of thoſe days to be played on the harp accompanied by the voice. The harp was ſtrung with wire, which gives a ſound of long continuance, and had no contrivance like that in the modern harpſichord, by which the ſound of the preceding could be ſtopt, the moment a ſucceeding note

note began. To avoid actual discord, it was therefore necessary that the succeeding emphatic note should be a chord with the preceding, as their sounds must exist at the same time. Hence arose that beauty in those tunes that has so long pleased, and will please for ever, tho' men scarce know why. That they were originally composed for the harp, and of the most simple kind, I mean a harp without any half notes but those in the natural scale, and with no more than two octaves of strings, from C to C, I conjecture from another circumstance, which is, that not one of those tunes really ancient, has a single artificial half note in it, and that in tunes where it was most convenient for the voice to use the middle notes of the harp, and place the key in F, there the B, which if used should be a B flat, is always omitted, by passing over it with a third. The connoisseurs in modern music will say, I have no taste, but I cannot help adding, that I believe our ancestors, in hearing a good song, distinctly articulated, sung to one of those tunes, and accompanied by the harp, felt more real pleasure than is communicated by the generality of modern operas, exclusive of that arising from the scenery and dancing. Most tunes of late composition, not having this natural harmony united with their melody, have recourse to the artificial harmony of a bass, and other accompanying parts*. This support, in my opinion, the old tunes do not need,

* The celebrated *Rousseau* in his *Dictionaire de Musique*, printed 1768, appears to have similar sentiments of our modern *Harmony*, viz.

" M. Rameau

need, and are rather confused than aided by it. Whoever has heard *James Oswald* play them on his violoncello, will be less inclined to dispute this with me. I have more than once seen tears of pleasure in the eyes of his auditors; and yet, I think, even *his* playing those tunes would please more, if he gave them less modern ornament.

I am, &c.

B. F.

" M. Rameau prétend que les dessus d'une certaine simplicité suggèrent naturellement leur basse, & qu'un homme ayant l'oreille juste & non exercée, entonnera naturellement cette basse. C'est-là un préjugé de musicien, démenti par toute expérience. Non seulement celui qui n'aura jamais entendu ni basse ni *harmonie*, ne trouvera, de lui-même, ni cette *harmonie* ni cette basse; mais elles lui déplairont si on les lui fait entendre, & il aimera beaucoup mieux le simple unisson.

Quand on songe que, de tous les peuples de la terre, qui tous ont une musique & un chant, les Européens sont les seuls qui aient une *harmonie* des accords, & qui trouvent ce mélange agréable; quand on songe que le monde a duré tant de siècles, sans que, de toutes les nations qui ont cultivé les beaux arts, aucune ait connu cette *harmonie*; qu'aucun animal, qu'aucun oiseau, qu'aucun être dans la nature ne produit d'autre accord que l'unisson, ni d'autre musique que la mélodie; que les langues orientales, si sonores, si musicales; que les oreilles Grecques, si délicates, si sensibles, exercées avec tant d'art, n'ont jamais guidé ces peuples voluptueux & passionnés vers notre *harmonie*; que, sans elle, leur musique avoit des effets si prodigieux: qu'avec elle la nôtre en a de si foibles; qu'enfin il étoit réservé à des peuples du Nord, dont les organes durs & grossiers sont plus touchés de l'éclat & du bruit des voix, que de la douceur des accens, & de la mélodie des inflexions, de faire cette grande découverte, & de la donner pour principe à toutes les règles de l'art; quand, dis-je, on fait attention à tout cela, il est bien difficile de ne pas soupçonner que toute notre *harmonie* n'est qu'une invention gothique & barbare, dont nous ne nous fussions jamais avisés, si nous eussions été plus sensibles aux véritables beautés de l'art, & à la musique vraiment naturelle."

LETTER LVI.

To Mr. P. F. Newport, *New England*.

Dear Brother,

* * * * "I like your ballad, and think it well adapted for your purpose of discountenancing expensive foppery, and encouraging industry and frugality. If you can get it generally sung in your country, it may probably have a good deal of the effect you hope and expect from it. But as you aimed at making it general, I wonder you chose so uncommon a measure in poetry, that none of the tunes in common use will suit it. Had you fitted it to an old one, well known, it must have spread much faster than I doubt it will do from the best new tune we can get compos'd for it. I think too, that if you had given it to some country girl in the heart of the *Massachusets*, who has never heard any other than psalm tunes, or *Chevy Chace*, the *Children in the Wood*, the *Spanish Lady*, and such old simple ditties, but has naturally a good ear, she might more probably have made a pleasing popular tune for you, than any of our masters here, and more proper for your purpose, which

would beſt be anſwered, if every word could as it is ſung be underſtood by all that hear it, and if the emphaſis you intend for particular words could be given by the ſinger as well as by the reader; much of the force and impreſſion of the ſong depending on thoſe circumſtances. I will however get it as well done for you as I can.

Do not imagine that I mean to depreciate the ſkill of our compoſers of muſic here; they are admirable at pleaſing *practiſed* ears, and know how to delight *one another*; but, in compoſing for ſongs, the reigning taſte ſeems to be quite out of nature, or rather the reverſe of nature, and yet like a torrent, hurries them all away with it; one or two perhaps only excepted.

You, in the ſpirit of ſome ancient legiſlators, would influence the manners of your country by the united powers of poetry and muſic. By what I can learn of *their* ſongs, the muſic was ſimple, conformed itſelf to the uſual pronunciation of words, as to meaſure, cadence or emphaſis, *&c.* never diſguiſed and confounded the language by making a long ſyllable ſhort, or a ſhort one long when ſung; their ſinging was only a more pleaſing, becauſe a melodious manner of ſpeaking; it was capable of all the graces of proſe oratory, while it added the pleaſure of harmony. A modern ſong, on the contrary, neglects all the proprieties and beauties of common ſpeech, and in their place introduces its *defects* and *abſurdities* as ſo many graces. I am afraid you will hardly take my word for this, and

and therefore I must endeabour to support it by proof. Here is the first song I lay my hand on. It happens to be a composition of one of our greatest masters, the ever famous *Handel.* It is not one of his juvenile performances, before his taste could be improved and formed: It appeared when his reputation was at the highest, is greatly admired by all his admirers, and is really excellent in its kind. It is called, *The additional* FAVOURITE *Song in* Judas Maccabeus. Now I reckon among the defects and improprieties of common speech, the following, viz.

1. *Wrong placing the accent or emphasis*, by laying it on words of no importance, or on wrong syllables.

2. *Drawling*; or extending the sound of words or syllables beyond their natural length.

3. *Stuttering*; or making many syllables of one.

4. *Unintelligibleness*; the result of the three foregoing united.

5. *Tautology*; and

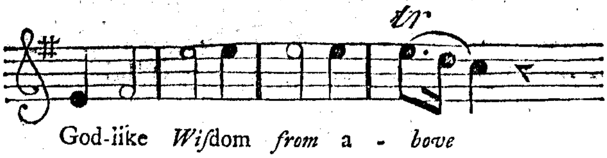
6. *Screaming*, without cause.

For the *wrong placing of the accent*, *or emphasis*, see it on the word *their* instead of being on the word *vain*.

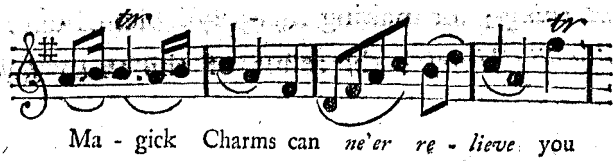
And on the word *from*, and the wrong ſyllable *like*.

For the *Drawling*, ſee the laſt ſyllable of the word *wounded*.

And in the ſyllable *wiſ*, and the word *from*, and ſyllable *bove*



For the *Stuttering*, ſee the words *ne'er relieve*, in



Here are four ſyllables made of one, and eight of three; but this is moderate. I have ſeen in another ſong that I cannot now find, ſeventeen ſyllables made of three, and ſixteen of one; the latter I remember was the word *charms*; viz. *Cha, a, a, a, a, a, a, a, a, a, a, a, a, a, a, arms.* Stammering with a witneſs!

For the *Unintelligibleneſs*; give this whole ſong to any

taught

taught ſinger, and let her ſing it to any company that have never heard it; you ſhall find they will not underſtand three words in ten. It is therefore that at the oratorio's and operas one ſees with books in their hands all thoſe who deſire to underſtand what they hear ſung by even our beſt performers.

For the *Tautology*; you have, *with their vain myſterious art*, twice repeated; *Magic charms can ne'er relieve you*, three times. *Nor can heal the wounded heart*, three times. *Godlike wiſdom from above*, twice; and, *this alone can ne'er deceive you*, two or three times. But this is reaſonable when compared with *the Monſter Polypheme, the Monſter Polypheme*, a hundred times over and over, in his admired *Acis and Galatea*.

As to the *ſcreaming*; perhaps I cannot find a fair inſtance in this ſong; but whoever has frequented our operas will remember many. And yet here methinks the words *no* and *e'er*, when ſung to theſe notes, have a little of the air of *ſcreaming*, and would actually be ſcream'd by ſome ſingers.

I ſend you incloſed the ſong with its muſic at length. Read the words without the repetitions. Obſerve how few

few they are, and what a ſhower of notes attend them. You will then perhaps be inclined to think with me, that though the words might be the principal part of an ancient ſong, they are of ſmall importance in a modern one; they are in ſhort only *a pretence for ſinging.*

I am, as ever,

Your affectionate brother,

B. F.

P. S. I might have mentioned *Inarticulation* among the defects in common ſpeech that are aſſumed as beauties in modern ſinging. But as that ſeems more the fault of the ſinger than of the compoſer, I omitted it in what related merely to the compoſition. The fine ſinger in the preſent mode, ſtifles all the hard conſonants, and poliſhes away all the rougher parts of words that ſerve to diſtinguiſh them one from another; ſo that you hear nothing but an admirable pipe, and underſtand no more of the ſong, than you would from its tune played on any other inſtrument. If ever it was the ambition of muſicians to make inſtruments that ſhould imitate the human voice, that ambition ſeems now reverſed, the voice aiming to be like an inſtrument. Thus wigs were firſt made to imitate a good natural head of hair;—but when they became faſhionable, though in unnatural forms, we have ſeen natural hair dreſſed to look like wigs.

LETTER LIX.

Of LIGHTNING, *and the Method (now used in* America) *of securing Buildings and Persons from its mischievous Effects.*

EXPERIMENTS made in electricity first gave philosophers a suspicion that the matter of lightning was the same with the electric matter. Experiments afterwards made on lightning obtained from the clouds by pointed rods, received into bottles, and subjected to every trial, have since proved this suspicion to be perfectly well founded; and that whatever properties we find in electricity, are also the properties of lightning.

This matter of lightning, or of electricity, is an extream subtile fluid, penetrating other bodies, and subsisting in them, equally diffused.

When by any operation of art or nature, there happens to be a greater proportion of this fluid in one body than in another, the body which has most, will communicate to that which has least, till the proportion becomes equal; provided the distance between them be not too great; or, if it is too great, till there be proper conductors to convey it from one to the other.

If

If the communication be through the air without any conductor, a bright light is feen between the bodies, and a found is heard. In our fmall experiments we call this light and found the electric fpark and fnap; but in the great operations of nature, the light is what we call *lightning*, and the found (produced at the fame time, tho' generally arriving later at our ears than the light does to our eyes) is, with its echoes, called *thunder*.

If the communication of this fluid is by a conductor, it may be without either light or found, the fubtle fluid paffing in the fubftance of the conductor.

If the conductor be good and of fufficient bignefs, the fluid paffes through it without hurting it. If otherwife, it is damaged or deftroyed.

All metals, and water, are good conductors.—Other bodies may become conductors by having fome quantity of water in them, as wood, and other materials ufed in building, but not having much water in them, they are not good conductors, and therefore are often damaged in the operation.

Glafs, wax, filk, wool, hair, feathers, and even wood, perfectly dry are non-conductors: that is, they refift inftead of facilitating the paffage of this fuble fluid.

When this fluid has an opportunity of paffing through two conductors, one good, and fufficient, as of metal, the other not fo good, it paffes in the beft, and will follow it in any direction.

The

The diſtance at which a body charged with this fluid will diſcharge itſelf ſuddenly, ſtriking through the air into another body that is not charged, or not ſo highly charg'd, is different according to the quantity of the fluid, the dimenſions and form of the bodies themſelves, and the ſtate of the air between them.—This diſtance, whatever it happens to be between any two bodies, is called their *ſtriking diſtance*, as till they come within that diſtance of each other, no ſtroke will be made.

The clouds have often more of this fluid in proportion than the earth; in which caſe as ſoon as they come near enough (that is, within the ſtriking diſtance) or meet with a conductor, the fluid quits them and ſtrikes into the earth. A cloud fully charged with this fluid, if ſo high as to be beyond the ſtriking diſtance from the earth, paſſes quietly without making noiſe or giving light; unleſs it meets with other clouds that have leſs.

Tall trees, and lofty buildings, as the towers and ſpires of churches, become ſometimes conductors between the clouds and the earth; but not being good ones, that is, not conveying the fluid freely, they are often damaged.

Buildings that have their roofs covered with lead, or other metal, and ſpouts of metal continued from the roof into the ground to carry off the water, are never hurt by lightning, as whenever it falls on ſuch a building, it paſſes in the metals and not in the walls.

When other buildings happen to be within the ſtriking diſtance from ſuch clouds, the fluid paſſes in the walls whether of wood, brick or ſtone, quitting the walls only when it can find better conductors near them, as metal rods, bolts, and hinges of windows or doors, gilding on wainſcot, or frames of pictures; the ſilvering on the backs of looking-glaſſes; the wires for bells; and the bodies of animals, as containing watry fluids. And in paſſing thro' the houſe it follows the direction of theſe conductors, taking as many in it's way as can aſſiſt it in its paſſage, whether in a ſtrait or crooked line, leaping from one to the other, if not far diſtant from each other, only rending the wall in the ſpaces where theſe partial good conductors are too diſtant from each other.

An iron rod being placed on the outſide of a building, from the higheſt part continued down into the moiſt earth, in any direction ſtrait or crooked, following the form of the roof or other parts of the building, will receive the lightning at its upper end, attracting it ſo as to prevent its ſtriking any other part; and, affording it a good conveyance into the earth, will prevent its damaging any part of the building.

A ſmall quantity of metal is found able to conduct a great quantity of this fluid. A wire no bigger than a gooſe quill, has been known to conduct (with ſafety to the building as far as the wire was continued) a quantity of lightning that did prodigious damage both above and

below

below it; and probably larger rods are not necessary, tho' it is common in America, to make them of half an inch, some of three quarters, or an inch diameter.

The rod may be fastened to the wall, chimney, &c. with staples of iron.—The lightning will not leave the rod (a good conductor) to pass into the wall (a bad conductor), through those staples.—It would rather, if any were in the wall, pass out of it into the rod to get more readily by that conductor into the earth.

If the building be very large and extensive, two or more rods may be placed at different parts, for greater security.

Small ragged parts of clouds suspended in the air between the great body of clouds and the earth (like leaf gold in electrical experiments), often serve as partial conductors for the lightning, which proceeds from one of them to another, and by their help comes within the striking distance to the earth or a building. It therefore strikes through those conductors a building that would otherwise be out of the striking distance.

Long sharp points communicating with the earth, and presented to such parts of clouds, drawing silently from them the fluid they are charged with, they are then attracted to the cloud, and may leave the distance so great as to be beyond the reach of striking.

It is therefore that we elevate the upper end of the rod six or eight feet above the highest part of the building, tapering it gradually to a fine ſharp point, which is gilt to prevent its ruſting.

Thus the pointed rod either prevents a ſtroke from the cloud, or, if a ſtroke is made, conducts it to the earth with ſafety to the building.

The lower end of the rod ſhould enter the earth ſo deep as to come at the moiſt part, perhaps two or three feet; and if bent when under the ſurface ſo as to go in a horizontal line ſix or eight feet from the wall, and then bent again downwards three or four feet, it will prevent damage to any of the ſtones of the foundation.

A perſon apprehenſive of danger from lightning, happening during the time of thunder to be in a houſe not ſo ſecured, will do well to avoid ſitting near the chimney, near a looking glaſs, or any gilt pictures or wainſcot; the ſafeſt place is in the middle of the room, (ſo it be not under a metal luſtre ſuſpended by a chain) ſitting in one chair and laying the feet up in another. It is ſtill ſafer to bring two or three mattraſſes or beds into the middle of the room, and folding them up double, place the chair upon them; for they not being ſo good conductors as the walls, the lightning will not chuſe an interrupted courſe through the air of the room and the bedding, when it can go thro' a continued better conductor the wall. But where it can be had, a hamock or ſwinging bed, ſuſpended by ſilk cords equally diſtant from the walls on every ſide, and from the cieling and floor above and below, affords the ſafeſt ſituation

ſituation a perſon can have in any room whatever; and what indeed may be deemed quite free from danger of any ſtroke by lightning.

Paris, Sept. 1767. B. F.

LETTER LX.

Extract of a Letter from J. W. *Eſq; Profeſſor of Natural Philoſophy at* Cambridge, *in* New England. *Jan.* 6, 1768.

" * * * I have read in the Philoſophical Tranſactions
" the account of the effects of lightning on St. Bride's
" ſteeple. 'Tis amazing to me, that after the full demon-
" ſtration you had given, of the identity of lightning and of
" electricity, and the power of metalline conductors, they
" ſhould ever think of repairing that ſteeple without ſuch
" conductors. How aſtoniſhing is the force of prejudice
" even in an age of ſo much knowledge and free en-
" quiry!"

ANSWER

ANSWER to the above.

* * * It is perhaps not ſo extraordinary that unlearned men, ſuch as commonly compoſe our church veſtries, ſhould not yet be acquainted with, and ſenſible of the benefits of metal conductors in averting the ſtroke of lightning; and preſerving our houſes from its violent effects, or that they ſhould be ſtill prejudiced againſt the uſe of ſuch conductors, when we ſee how long even philoſophers, men of extenſive ſcience and great ingenuity, can hold out againſt the evidence of new knowledge, that does not ſquare with their preconceptions; and how long men can retain a practice that is conformable to their prejudices, and expect a benefit from ſuch practice, though conſtant experience ſhows its inutility. A late piece of the Abbé *Nollet*, printed laſt year in the memoirs of the French Academy of ſciences, affords ſtrong inſtances of this: For though the very relations he gives of the effects of lightning in ſeveral churches and other buildings, ſhow clearly that it was conducted from one part to another by wires, gildings, and other pieces of metal that were *within*, or connected with the building, yet in the ſame paper he objects to the providing metalline conductors *without* the building, as uſeleſs or dangerous*. He cautions people not to ring the church bells

* Notre curioſité pourroit peut-être s'applaudir des recherches qu'elle nous a fait faire ſur la nature du tonnerre, & ſur la mécaniſme de ſes principaux effets, mais ce néſt point ce qu'il y a de plus important; il vaudroit bien mieux que

bells during a thunder-ſtorm, leſt the lightning, in its way to the earth, ſhould be conducted down to them by the bell ropes*, which are but bad conductors; and yet is againſt fixing metal rods on the outſide of the ſteeple, which are known to be much better conductors, and which it would certainly chuſe to paſs in, rather than in dry hemp. And though for a thouſand years paſt bells have been ſolemnly conſecrated by the Romiſh church†, in expectation that

que nous puiſſions trouver quelque moyen de nous en garantir: on y a penſé; on s'eſt même flatté d'avoir fait cette grande découverte; mais malheureuſement douze années d'épreuves & un peu de réflexion, nous apprennent qu'il ne faut pas compter ſur les promeſſes qu'on nous a faites. Je l'ai dit, il y a long temps, and avec regret, toutes ces pointes de fer qu'on dreſſe en l'air, ſoit comme *électroſcopes*, ſoit comme préſervatifs, *** ſont plus propre à nous attirer le feu du tonnerre qu' à nous en préſerver; *** & je perſiſte à dire que le projet d'épuiſer une nuée orageuſe du feu dont elle eſt chargée, n'eſt pas celui d'un phyſicien.—***. *Memoire ſur les Effets du Tonnerre.*

* Les cloches, en vertu de leur bénédiction, doivent écarter les orages & nous preſerver des coups de foudre; mais l'egliſe permet à la prudence humaine le choix des momens où il convient d'uſer de ce préſervatif. Je ne ſais ſi le ſon, conſidéré phyſiquement, eſt capable ou non de faire crever une nuée & de cauſer l'épanchement de ſon feu vers les objets terreſtres, mais il eſt certain & prouvé par l'expérience, que la tonnerre peut tomber ſur un clocher, ſoit que l'on y ſonne ou que l'on n'y ſonne point; & ſi cela arrive dans le premier cas, les ſonneurs ſont en grand danger, parcequ'ils tiennent des cordes par leſquelles la commotion de la foudre peut ſe communiquer juſqu'à eux: il eſt donc plus ſage de laiſſer les cloches en repos quand l'orage eſt arrivé au-deſſus de l'égliſe. Ibid.

† Suivant le rituel de Paris, lorſqu'on benit des cloches, on recite les oraiſons ſuivantes:

Benedic

that the ſound of ſuch bleſſed bells would drive away thoſe ſtorms, and ſecure our buildings from the ſtroke of lightning; and during ſo long a period, it has not been found by experience, that places within the reach of ſuch bleſſed ſound, are ſafer than others where it is never heard; but that on the contrary, the lightning ſeems to ſtrike ſteeples of choice, and that at the very time the bells are ringing†; yet ſtill they continue to bleſs the new bells, and jangle the old ones whenever it thunders.—One would think it was now time to try ſome other trick;—and ours is recommended (whatever this able philoſopher may have been told to the contrary) by more than twelve years experience, wherein, among the great number of houſes furniſhed with iron rods in North America, not one ſo guarded has been materially hurt with lightning, and ſeve-

Benedic, Domine . . . . . quotieſcumque ſonuerit, procul recedat virtus inſidiantium, umbra phantaſmatis, incurſio turbinum, percuſſio fulminum, læſio tonitruum, calamitas tempeſtatum, omniſque ſpiritus procellarum, &c.

Deus, qui per beatum Moïſen, &c. . . . . . . procul pellentur inſidiæ inimici, fragor grandinum, procella turbinum, impetus tempeſtatum, temperentur infeſta tonitrua, &c.

Omnipotens ſempiterne Deus, &c. . . . . . . ut ante ſonitum ejus effugentur ignita jacula inimici, percuſſio fulminum, impetus lapidum, læſio tempeſtatum, &c.

† En 1718. M. Deſlandes fit ſavoir à l'Academie Royale des ſciences, que la nuit du 14 ou 15 d'Avril de la même année, le tonnerre étoit tombé ſur vingtquatre égliſes, depuis Landernau juſqu'à Saint Pol-de-Léon en Bretagne; que ces égliſes étoient précíſément celles où l'on ſonnoit, & que la foudre avoit épargné celles ou l'on ne ſonnoit pas: que dans celle de Gouiſnon, qui fut entièrement ruinée, le tonnerre tua deux perſonnes de quatre qui ſonnoient, &c. *Hiſt. de l'Ac. R. des Sci.* 1719.

ral

ral have been evidently preserved by their means; while a number of houses, churches, barns, ships, &c. in different places, unprovided with rods, have been struck and greatly damaged, demolished or burnt. Probably the vestries of our English churches are not generally well acquainted with these facts; otherwise, since as good protestants they have no faith in the blessing of bells, they would be less excusable in not providing this other security for their respective churches, and for the good people that may happen to be assembled in them during a tempest, especially as those buildings, from their greater height, are more exposed to the stroke of lightning than our common dwellings.

I have nothing new in the philosophical way to communicate to you, except what follows. When I was last year in *Germany*, I met with a singular kind of glass, being a tube about eight inches long, half an inch in diameter, with a hollow ball of near an inch diameter at one end, and one of an inch and half at the other, hermetically sealed, and half filled with water.—If one end is held in hand, and the other a little elevated above the level, a constant succession of large bubbles proceeds from the end in the hand to the other end, making an appearance that puzzled me much, 'till I found that the space not filled with water was also free from air, and either filled with a subtile invisible vapour continually rising from the water, and extreamly rarifiable by the least heat at one end, and condenſable again by the leaſt coolneſs at the other; or it is the very fluid of fire itſelf, which parting from the hand pervades the glaſs, and by its expanſive force depreſſes the water till it can paſs between it and the glaſs, and eſcape to the other end, where it gets thro' the glaſs again into the air. I am rather inclined to the firſt opinion, but doubtful between the two. An ingenious artiſt here,–Mr. *Nairne*, mathematical inſtrument-maker, has made a number of them from mine, and improved them, for his are much more ſenſible than thoſe I brought from Germany.—I bor'd a very ſmall hole through the wainſcot in the ſeat of my window, through which a little cold air conſtantly entered, while the air in the room was kept warmer by fires daily made in it, being winter time. I plac'd one of his glaſſes, with the elevated end againſt this hole; and the bubbles from the other end, which was in a warmer ſituation, were continually paſſing day and night, to the no ſmall ſurprize of even philoſophical ſpectators. Each bubble diſcharged, is larger than that from which it proceeds, and yet that is not diminiſhed; and by adding itſelf to the bubble at the other end, that bubble is not increaſed, which ſeems very paradoxical.—When the balls at each end are made large, and the connecting tube very ſmall and bent at right angles, ſo that the balls, inſtead of being at the ends, are brought on the ſide of the tube, and the tube is held ſo as that the balls are above it, the water will be depreſſed in that which is held in the hand,

hand, and rise in the other as a jet or fountain; when it is all in the other, it begins to boil, as it were, by the vapour passing up through it; and the instant it begins to boil, a sudden coldness is felt in the ball held; a curious experiment, this, first observed and shewn me by Mr. *Nairne*. There is something in it similar to the old observation, I think mentioned by *Aristotle*, that the bottom of a boiling pot is not warm; and perhaps it may help to explain that fact;—if indeed it be a fact.——When the water stands at an equal height in both these balls, and all at rest; if you wet one of the balls by means of a feather dipt in spirit, though that spirit is of the same temperament as to heat and cold, with the water in the glasses, yet the cold occasioned by the evaporation of the spirit from the wetted ball, will so condense the vapour over the water contained in that ball, as that the water of the other ball will be pressed up into it, followed by a succession of bubbles, 'till the spirit is all dried away. Perhaps the observations on these little instruments may suggest and be applied to some beneficial uses. It has been thought that water reduced to vapour by heat, was rarified only fourteen thousand times, and on this principle our engines for raising water by fire are said to be constructed: But if the vapour so much rarified from water, is capable of being itself still farther rarified to a boundless degree by the application of heat to the vessels or parts of vessels containing the vapour (as at first it is applied to those containing the water) perhaps a a much greater power may be obtained, with little additional expence. Poſſibly too, the power of eaſily moving water from one end to the other of a moveable beam (ſuſpended in the middle like a ſcale beam) by a ſmall degree of heat, may be applied advantageouſly to ſome other mechanical purpoſes. * * *

I am, &c. B. F.

LETTER LXI.

To Sir *John Pringle*, Bart.

SIR, *Craven-ſtreet, May* 10, 1768.

YOU may remember that when we were travelling together in *Holland*, you remarked that the track-ſchuyt in one of the ſtages went ſlower than uſual, and enquired of the boatman, what might be the reaſon; who anſwered, that it had been a dry ſeaſon, and the water in the canal was low. On being again aſked if it was ſo low as that the boat touch'd the muddy bottom; he ſaid, no, not ſo low as that, but ſo low as to make it harder for the horſe to draw the boat. We neither of us at firſt could conceive that if there was water enough for the boat to ſwim

ſwim clear of the bottom, its being deeper would make any difference; but as the man affirmed it ſeriouſly as a thing well known among them; and as the punctuality required in their ſtages, was likely to make ſuch difference, if any there were, more readily obſerved by them, than by other watermen who did not paſs ſo regularly and conſtantly backwards and forwards in the ſame track; I began to apprehend there might be ſomething in it, and attempted to account for it from this conſideration, that the boat in proceeding along the canal, muſt in every boat's length of her courſe, move out of her way a body of water, equal in bulk to the room her bottom took up in the water; that the water ſo moved, muſt paſs on each ſide of her and under her bottom to get behind her; that if the paſſage under her bottom was ſtraitened by the ſhallows, more of that water muſt paſs by her ſides, and with a ſwifter motion, which would retard her, as moving the contrary way; or that the water becoming lower behind the boat than before, ſhe was preſſed back by the weight of its difference in height, and her motion retarded by having that weight conſtantly to overcome. But as it is often loſt time to attempt accounting for uncertain facts, I determined to make an experiment of this when I ſhould have convenient time and opportunity.

After our return to *England*, as often as I happened to be on the *Thames*, I enquired of our watermen whether they were ſenſible of any difference in rowing over ſhallow or deep water. I found them all agreeing in the fact, that there was a very great difference, but they differed widely in expreffing the quantity of the difference; fome fuppofing it was equal to a mile in fix, others to a mile in three, &c. As I did not recollect to have met with any mention of this matter in our philofophical books, and conceiving that if the difference fhould really be great, it might be an object of confideration in the many projects now on foot for digging new navigable canals in this ifland, I lately put my defign of making the experiment in execution, in the following manner.

I provided a trough of plained boards fourteen feet long, fix inches wide and fix inches deep, in the clear, filled with water within half an inch of the edge, to reprefent a canal. I had a loofe board of nearly the fame length and breadth, that being put into the water might be funk to any depth, and fixed by little wedges where I would chufe to have it ftay, in order to make different depths of water, leaving the furface at the fame height with regard to the fides of the trough. I had a little boat in form of a lighter or boat of burthen, fix inches long, two inches and a quarter wide, and one inch and a quarter deep. When fwimming, it drew one inch water. To give motion to the boat, I fixed one end of a long filk thread to its bow, juft even with the water's edge, the other end paffed over a well-made brafs pully, of about an inch diameter, turning freely on a fmall axis; and a fhilling was the weight. Then placing the boat at at one end of the trough, the weight would draw it through the water to the other.

Not having a watch that ſhows ſeconds, in order to meaſure the time taken up by the boat in paſſing from end to end, I counted as faſt as I could count to ten repeatedly, keeping an account of the number of tens on my fingers. And as much as poſſible to correct any little inequalities in my counting, I repeated the experiment a number of times at each depth of water, that I might take the medium.— And the following are the reſults.

	Water $1\frac{1}{2}$ inches deep.	2 inches.	$4\frac{1}{2}$ inches.
1ſt exp.	100	94	79
2	104	93	78
3	104	91	77
4	106	87	79
5	100	88	79
6	99	86	80
7	100	90	79
8	100	88	81
	813	717	632
	Medium 101	Medium 89	Medium 79

I made many other experiments, but the above are thoſe in which I was moſt exact; and they ſerve ſufficiently to ſhow that the difference is conſiderable. Between the deepeſt and ſhalloweſt it appears to be ſomewhat more than

than one fifth. So that ſuppoſing large canals and boats and depths of water to bear the ſame proportions, and that four men or horſes would draw a boat in deep water four leagues in four hours, it would require five to draw the ſame boat in the ſame time as far in ſhallow water; or four would require five hours.

Whether this difference is of conſequence enough to juſtify a greater expence in deepening canals, is a matter of calculation, which our ingenious engineers in that way will readily determine.

I am, &c. B. F.

THE END.

INDEX.

D

E

Electric

Fire,

G

Spots,