

**Experiments and observations on electricity, made in Philadelphia ... To which are added, letters and papers on philosophical subjects / [Ed. by P. Collinson].**

**Contributors**

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E X P E R I M E N T S  
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O N  
E L E C T R I C I T Y,  
M A D E A T  
P H I L A D E L P H I A I N A M E R I C A.

*Benj. Franklin.*









Fig. I.



Fig. II.



Fig. III.

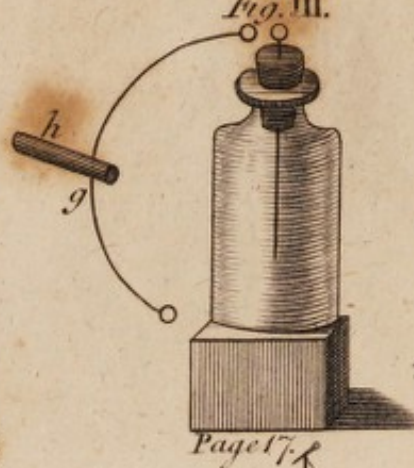


Fig. IV.



Fig. V.



Fig. IX.

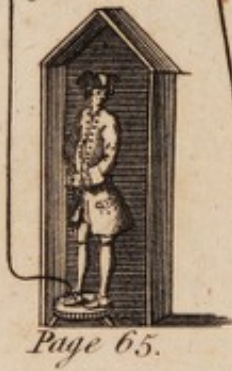


Fig. X.



Fig. VI.

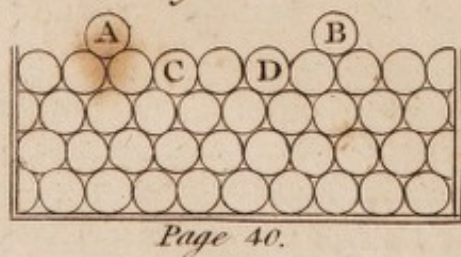


Fig. VIII.

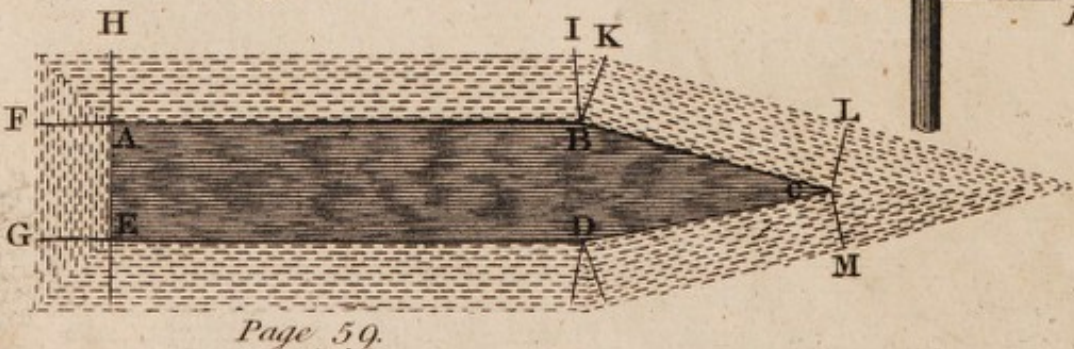
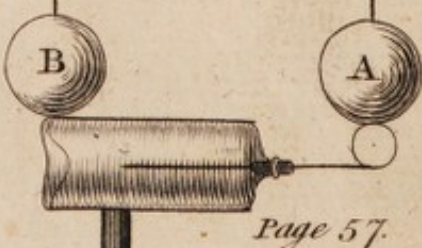


Fig. VII.





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# P R E F A C E

## To the First EDITION.

**I**T 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 written on various topics, 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



*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 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 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*



*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.



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# ADVERTISEMENT.

*Prefixed to the Fourth Edition.*

**A**LL 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 have 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.

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E X T R A C T  
O F  
L E T T E R I.

FROM

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

TO

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

S I R,

*Philadelphia*, March 28, 1747.

Y OUR 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 experi-

\* i. e. of the *Library-Company*, an institution of the Author's, founded 1730. To which company the present was made.

ments 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.

LETTER



# LETTER II.

FROM

BENJ. FRANKLIN, Esq; 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 not possibly 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 filken thread from the cieling, right over the mouth of the bottle, suspend a small cork-ball, about the



bigness of a marble; the 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 destroyed, 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, though 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 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 electrify 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 electrified 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 destroyed. 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

\* 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.

coal



coal from a wood fire; and the light of a 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 diffused 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 disposition and application of which wheels, and the various phenomena resulting,

\* 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 electrified shot, through which rarified air the electric fluid could more readily pass.

I could,



I could, if I had time, fill you a sheet †. The impossibility of electrifying 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 electrified, 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 electrified.

3. If they touch one another after exciting the tube, and drawing the fire as aforesaid, there will be a stronger spark between them than was between either of them and the person on the floor.

† 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.



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 electrified: 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 electrifying, 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 electrified *positively*; *A*, *negatively*. Or rather, *B* is electrified *plus*; *A*, *minus*. And we daily in our experiments  
electrify



electrify bodies *plus* or *minus*, as we think proper.--To electrify *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 ceiling to the gun barrel, thence to the sphere, and so electrified 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 electrified *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 electrify a person twenty or more times running, with a touch of the finger on the wire, thus: He stands on wax. Give him the electrified bottle in his hand. Touch the wire with your finger, and then touch his hand or face; there are sparks every time\*.---We encrease 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 electrified 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, four or five 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 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

\* 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.



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 fully 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 sitting 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.



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# LETTER III.

FROM

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

TO

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

S I R, *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 electrified from a non-electric electrified 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 considerable extent; but the electrical



electrical fire is crowded *into the substance* of the former, the glass confining it\*.

2. At the same time that the wire and top of the bottle, &c. is electrified *positively* or *plus*, the bottom of the bottle is electrified *negatively* or *minus*, in exact proportion: *i. e.* whatever quantity of electrical fire is thrown in at the top, an equal quantity goes out of the bottom†. To understand this, suppose the common quantity of electricity in each part of the bottle, before the operation begins, is equal to 20; and at every stroke of the tube, suppose a quantity equal to 1 is thrown in; then, after the first stroke, the quantity contain'd in the wire and upper part of the bottle will be 21, in the bottom 19. After the second, the upper part will have 22, the lower 18, and so on, till, after 20 strokes 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 spued back through the wire, or flies out in loud cracks through the sides of the bottle.

3. The equilibrium cannot be restored in the bottle by *inward* communication or contact of the parts; but it must be done by a communication formed *without* the

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

† What is said here, and after, of the *top* and *bottom* of the bottle, is true of the *inside* and *outside* surfaces, and should have been so expressed.



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 electrified, 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 electrified, 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 electrified 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 compressed, and in another part a perfect vacuum, we know the equilibrium would be instantly restored *within*. But here we have a bottle

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

containing



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

\* Other circumstances being equal,

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 electrified 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 electrify 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



cessantly from one to the other, 'till the bottle is no longer electrified; 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 electrified 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 electrified: the equilibrium is

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



never destroyed: for 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 supplied from the top\*. Hence a bottle cannot be electrified 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

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



on one, and then touch the wire; that book will be electrified *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 electrified *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 electrified *plus*, it will repel a positively 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 electrified a number of times by repeatedly touching the wire of an electrified 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 electrified *minus*, which will continue after the shock, as would also any *plus* electricity, which 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 electrified: 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,

\* *i. e.* From the *inside* to the *outside*.

† Placing the book on glass or wax is not necessary to produce the appearance; it is only to show that the visible Electricity is not brought up from the common stock in the earth.

and



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 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.

*S. I R,*

1748.

§ 1. **T**HERE 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

\* This was a Discovery of the very ingenious Mr. Kinnerley, and by him communicated to me.



first to the coating of the 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

\* 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.



ball, when it is repelled from one hook, will be as strongly attracted by the other; and play vigorously between them, fetching the electric fluid from the one, and delivering it to the other, 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



10. But suspend two or more phials on the prime conductor, one hanging on the tail of the other; and a wire from the last to the floor, an equal number of turns of the 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 bottles becomes more equal to the charging power, and so repels the fire back again on the globe, sooner in proportion 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 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,



glafs, and drew out the cork and wire which for that purpose had been loofely put in. Then taking the bottle in one hand, and bringing a finger of the other near its mouth, a ftrong fpark came from the water, and the fhock was as violent as if the wire had remained in it, which fhewed that the force did not lie in the wire. Then to find if it refided in the water, being crouded into and condensed in it, as confin'd by the glafs, which had been our former opinion, we electrified the bottle again, and placing it on glafs, drew out the wire and cork as before; then taking up the bottle, we decanted all its water into an empty bottle, which likewise flood on glafs; and taking up that other bottle, we expected, if the force refided in the water, to find a fhock from it; but there was none. We judged then that it muft either be loft in decanting, or remain in the firft bottle. The latter we found to be true; for that bottle on trial gave the fhock, though filled up as it flood with fresh unelectrified water from a tea-pot.---To find, then, whether glafs had this property merely as glafs, or whether the form contributed any thing to it; we took a pane of fafh-glafs, and laying it on the hand, placed a plate of lead on its upper furface; then electrified that plate, and bringing a finger to it, there was a fpark and fhock. We then took two plates of lead of equal dimenfions, but lefs than the glafs by two inches every way, and electrified the glafs between them, by electrifying the uppermoft lead; then feparated the glafs from the lead, in doing which, what little fire might be in the lead was taken out, and the glafs being touched in the



electrified parts with a finger, afforded only very small pricking sparks, but a great number of them might be taken from different places. Then dextrously placing it again between the leaden plates, and completing 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, armed 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



fwering our intention 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, p. 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 glass: then put in the board, and that side is finished. Turn

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

† Contrived by Mr. *Kinnerfley*.



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

\* We have since found it fatal to small animals, though not to large ones. The biggest we have yet killed is a hen. 1756.



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; and not only taking out the fire that had been communi-  
cated



cated 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 on its top, to keep



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  
F surface,



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 oreries.

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 thro' 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. Kinnerley.



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 broken, 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 surpris'd 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 circuit, would not conduct the least perceptible shock, and indeed when one wire was electrified the other hardly shewed any signs of its being in connection with it \*. Even a thoroughly wet packthread 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 surprises 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 *positively*, and the cork

\* 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; the number of interruptions in the line of gold, making, when added together, a space larger perhaps than the striking distance.



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 *Skuykil* \*. 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 †.

\* 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 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 on the top of one of the rods, bending downwards, so as to deliver commodiously the spark upon the surface



A turkey is to be killed for our 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*.

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. *Kinnerfley'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, if sufficient, and other circumstances are equal) is struck and penetrated by it as it passes.

\* An *electrified bumper* is a small thin glass tumbler, nearly 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 breath on the liquor.

April 29,

1749.

LETTER



# LETTER V.

CONTAINING

OBSERVATIONS *and* SUPPOSITIONS,  
towards forming a new HYHOTHESIS, for ex-  
plaining the several Phænomena of THUN-  
DER-GUSTS \*.

S I R,

§. 1. **N**ON-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 ne-

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

gatively:



gatively: for should it 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, 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 made denser, would sink to the earth with its water, and not rise 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 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



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 furrounded 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 furrounding one particle of air comes in contact with the water furrounding another, they coalesce and form a drop, and we have rain.

24. The sun supplies (or seems to supply) common fire to 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 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 cloud; and, in leaving it, flashes brightly and cracks loudly; the particles



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 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 clouds



clouds 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 goodness: 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 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, the other to receive fire; and so it proceeds through both sets, but so quick as to be in a manner instantaneous.



instantaneous. In the cohesion they shake off and drop their water, which represents rain.

35. Thus when sea and land clouds would pass at two great a distance for 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 is rarified by the sun, it rises, the denser northern and southern air pressing into its place.

The



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 though 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 seen first.

For the electrical fire is never visible but when in motion, and leaping from body to body, or from particle to particle through the air. When it passes through 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, passes in the wire invisibly ;



sibly; but in passing along a chain, it becomes visible as it leaps from link to link. In passing along leaf gilding it is 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, though 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 it is first in motion, *i. e.* in the most northern part, and the appearance proceeds southward, though 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 screened 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, because the blood and other humours, containing so much water, are more ready conductors.

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 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, though 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 fundry menstrua can) will make the solid become a

\* 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.



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 dropped 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. Transf.* Vol. LI. Part I. And Mr. *Kinnerley* has found that a fine iron wire, melted by Electricity, has had the same effect.

A D D I-



# ADDITIONAL PAPERS.

T O

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

S I R, *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 shew, 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.



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

§. 1. **T**HE 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 through 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 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 sponge to the electrical fluid. And as a sponge would receive no water if the parts of water were not smaller than the pores of the sponge; and even then but slowly, if there were not a mutual attraction between those parts and the parts of the sponge; 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 sponge. 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 electrics *per se*, as glass, &c. attract



tract 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



of the 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 encreases; 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 ceiling. 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



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 electrified 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.



i 6. The atmosphere of electrical particles furrounding an electrified sphere, is not more disposed to leave it, or more easily drawn off from any one part of the sphere than 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 it's 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.



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 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, a degree of strength not sufficient to pull away a handful at once, could



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 occurred to me, and while they first floated in my mind, appeared perfectly satisfactory; but now I have written them, and considered them more closely, 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; it is enough if we know the laws themselves. It is 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. It is 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, formed into a tube, near ten feet long and a foot diameter. It is covered with *Dutch* emboss'd paper, almost totally gilt. This large metallic surface supports a much greater



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 the 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 the beam by a pack-thread from the ceiling, 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 pack-thread. Set the iron  
punch



punch on the end upon the floor, in a 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, though 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 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 ;



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 tried 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 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,



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 Turkey-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 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

See note in page 49.



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 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



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 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,



tance, it cannot leap over them, and so the flock 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 extreme 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 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;



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 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 from points where you would not have them. You  
may



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 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



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 considered, *that we cannot by any means we are yet acquainted with, force the electrical fluid through glass.* I know it is commonly thought that it easily pervades 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 found, but this, that the fluid can pass through the one, and not through the other\*?

29. It is true, there is an experiment that at first sight would be apt to satisfy a slight observer, that the

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



fire thrown into the bottle by the wire, does really pass through the glass. It is this: place the bottle on a glass stand, under the prime conductor; suspend 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 glass stand, at just the same distance from the coating of the bottle, as the bullet is from its wire. Now let the globe be turned, and you see a spark strike from the bullet to the wire of the bottle, and the same instant you see and feel an exactly equal spark striking from the coating on your knuckle, and so on, spark for spark. This looks as if the whole received by the bottle was again discharged from it. And yet the bottle by this means is charged \*! And therefore the fire that thus leaves the bottle, though the same in quantity, cannot be the very same fire that entered at the wire, for if it were, the bottle would remain uncharged.

30. If the fire that so leaves the bottle be not the same that is thrown in through the wire, it must be fire that subsisted in the bottle, (that is, in the glass of the bottle) before the operation began.

31. If so, there must be a great quantity in glass, because a great quantity is thus discharged, even from very thin glass.

32. That this electrical fluid or fire is strongly attracted by glass, we know from the quickness and vio-

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



lence 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 repulency 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 inner 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, and so mix together; yet, though the particles of electric fluid, imbibed by each surface, cannot  
themselves



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 vacuum had been occasioned. For experiments favouring (if I  
may



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.---Glas, 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 friction on the rubb'd part of the globe's

\* 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.



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 wire into the tube,

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



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 a 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 thro' 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 se*, it does not attract it, having already

\* See *Further Experiments*, Sect. 15.



its quantity. So the air never draws off an electric atmosphere from any body, but in proportion to the non-electrics mixed 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. 1<sup>st</sup>, 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 sur-

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face;



face: 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 farther 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, and so go to the prime conductor; for the electric fluid itself cannot come through; and the prime conductor is always supply'd  
from



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 electrified. 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 electrified. 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 through 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 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



already made this paper too long, for which I must crave pardon, not having now time to abridge it. 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.

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 if you draw the phial from the coating by a chain from the coating to the phial, and the phial will charge.

ADDITION

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



## 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 furcharged, be so much precisely as is wanted by the outside surface, it will pass round through the wire fixed to the wax handle, restore the equilibrium in  
the



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 repulsion before the bottle is discharged, they continue so afterwards. If not, they are not electrified by that discharge.

LETTER



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# LETTER VI.

FROM

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

TO

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

S I R,

July 27, 1750.

MR. *W-tf-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 it is 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,

\* See the paper entitled, *Farther Experiments, &c,*



in a spoon held by another 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 basin in his hand, into which the water from the phial is poured, *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 basin, 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 polarity to needles, and reversed it



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, entered 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 blistered when examined by a magnifying glass: the jars I make use of hold seven or eight 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 gun-powder 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 circuit, 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.



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# LETTER VII.

FROM

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

TO

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

*Communicated to Mr. COLLINSON.*

S I R,

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 to you that I have melted brass pins and steel needles, inverted the poles of the magnetic needle, given a magnetism and polarity to needles that  
had



had none, and fired dry gunpowder by the electric spark. I have five bottles that contain eight or nine 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 humourously 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



## QUERIES and ANSWERS referred 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 rubb'd, 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. If so, the  
terms



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, entitled, *Opinions and Conjectures, &c.* 1749.

*Query*, What are the effects of air in electrical experiments?

*Answer*. All I have hitherto observed are these.

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



Moist 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 furrounds, 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 passage 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 filled 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 breathe freely in such an atmosphere, and dry air will blow through it without displacing or driving it away. I question whether the strongest dry North-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

† A cold dry wind of North America.



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 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.

† The experiment here mentioned was thus made. An empty phial was stopped with a cork. Through the cork passed a thick wire, as usual in the Leyden experiment, which wire almost reached the bottom: Through another part of the cork passed one leg of a small glass syphon, the other leg on the outside came down almost to the bottom of the phial. This phial was first held a short time in the hand, which warming, and of course rarifying the air within, drove a small part of it out through the syphon. Then a little red ink in a tea-spoon was applied to the opening of the outer leg of the syphon; so that as the air within cooled, a little of the ink might rise in that leg. When the air within the bottle came to be of the same temperature of that without, the drop of red ink would rest in a certain part of the leg. But the warmth of a finger applied to the phial would cause that drop to descend, as the least outward coolness applied would make it ascend. When it had found its situation, and was at rest, the wire was electrified by a communication from the prime conductor. This was supposed to give an electric atmosphere to the wire within the bottle, which might likewise rarify the included air, and of course depress the drop of ink in the syphon. But no such effect followed.

A bright



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.

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## LETTER VIII.

FROM

Mr. E. KINNERSLEY, at *Boston*,

TO

BENJAMIN FRANKLIN, Esq; at *Philadelphia*.

*Communicated, with the Answers, to Mr. COLLINSON,*

S I R,

*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



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 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 cannot 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.

LETTER



## LETTER IX.

FROM

BENJ. FRANKLIN, Esq; in *Philadelphia*,

TO

Mr. E. KINNERSLEY, at *Boston*.

S I R,

*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 centres, 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.

LETTER



# LETTER X.

FROM

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

TO

Mr. E. KENNERSLEY, at *Boston*.

S I R;

March 16, 1752.

HAVING brought your brimstone globe to work, I tried one of the experiments you proposed, and was agreeably surpris'd 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 charged by the glass globe, attracted a cork ball that had touched the wire of a phial charged by the brimstone globe, and *vice versa*, so that the cork continued to play be-

tween



tween 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 reverse of both happens, when  
you



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 turned; short, small, and hissing when the glass globe is turned.---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

P

positively?



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 Messieurs *de Buffon*, *D'Alibard*, and *de Lor*, a desire of verifying the con-

\* 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.

jectures



jectures of Mr. *Franklin*, upon the analogy of thunder and electricity, they prepared 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. MAZARS.



*A more particular Account of the Circumstances and Success of this extraordinary Experiment, was laid before the Royal Academy of Sciences at Paris, three Days afterwards, in a Memorial by M. D'Alibard, viz.*

## EXTRAIT D'UN MEMOIRE

DE M. D'ALIBARD,

*Lû à l'Académie Royale des Sciences, le 13 Mai, 1752.*

“ . . . . **E**N suivant la route que M. Franklin nous  
 “ à tracée, j'ai obtenu une satisfaction com-  
 “ plette. Voici les préparatifs, le procédé & le succès.  
 “ 1°. J'ai fait faire à Marly-la-ville, située à six  
 “ lieues de Paris au milieu d'une belle plaine dont le  
 “ sol est fort élevé, une verge de fer ronde, d'environ  
 “ un pouce de diametre, longue de 40 pieds, & fort  
 “ pointuë par son extrémité supérieure; pour lui mé-  
 “ nager une pointe plus fine, je l'ai fait armer d'acier  
 “ trempé & ensuite brunir, au défaut de dorure, pour  
 “ la préserver de la rouille; outre cela, cette verge de  
 “ fer est courbée vers son extrémité inférieure en deux  
 “ coudes à angles aigus quoiqu'arrondis; le premier  
 “ coude est éloigné de deux pieds du bout inférieur,  
 “ & le second est en sens contraire à trois pieds du  
 “ premier.

“ 2°. J'ai



“ 2°. J’ai fait planter dans un jardin trois grosses  
“ perches de 28 à 29 pieds, disposées en triangle, &  
“ éloignées les unes des autres d’environ huit pieds ;  
“ deux de ces perches sont contre un mur, & la troi-  
“ sième est au-dedans du jardin. Pour les affermir  
“ toutes ensemble, l’on a cloué sur chacune des entre-  
“ toises à vingt pieds de hauteur ; & comme le grand  
“ vent agitoit encore cette espèce d’édifice, l’on a at-  
“ taché au haut de chaque perche de longs cordages,  
“ qui tenant lieu d’aubans, répondent par le bas à de  
“ bons piquets fortement enfoncés en terre à plus de  
“ 20 pieds des perches.

“ 3°. J’ai fait construire entre les deux perches voi-  
“ sines du mur, & adosser contre ce mur une petite gué-  
“ rite de bois capable de contenir un homme & une  
“ table.

“ 4°. J’ai fait placer au milieu de la guérite une pe-  
“ tite table d’environ un demi-pied de hauteur ; & sur  
“ cette table j’ai fait dresser & affermir un tabouret  
“ électrique. Ce tabouret n’est autre chose qu’une  
“ petite planche quarrée, portée sur trois bouteilles à  
“ vin ; il n’est fait de cette matière que pour suppléer  
“ au défaut d’un gâteau de résine qui me manquoit.

“ 5°. Tout étant ainsi préparé, j’ai fait élever per-  
“ pendiculairement la verge de fer au milieu des trois  
“ perches, & je l’ai affermie en l’attachant à chacune de  
“ ces perches avec de forts cordons de soie par deux en-  
“ droits seulement. Les premiers liens sont au haut  
“ des perches, environ trois pouces au-dessous de leurs  
“ extrémités supérieures ; les seconds vers la moitié de  
“ leur hauteur. Le bout inférieur de la verge de fer est  
“ solidement



“ solidement appuyé sur le milieu du tabouret elec-  
“ trique, où j’ai fait creuser, un trou propre à le re-  
“ cevoir.

“ 6°. Comme il étoit important de garantir de la  
“ pluie le tabouret & les cordons de soie, parce qu’ils  
“ laisseroient passer la matière électrique s’ils étoient  
“ mouillés, j’ai pris les précautions nécessaires pour en  
“ empêcher. C’est dans cette vue que j’ai mis mon ta-  
“ bouret sous la guérite, & que j’avois fait courber  
“ ma verge de fer à angles aigus; afin que l’eau qui  
“ pourroit couler le long de cette verge, ne pût arriver  
“ jusques sur le tabouret. C’est aussi dans le même  
“ dessein que j’ai fait clouer sur le haut & au milieu  
“ de mes perches, à trois pouces au-dessus des cordons  
“ de soie, des especes de boîtes formées de trois petites  
“ planches d’environ 15 pouces de long, qui couvrent  
“ par-dessus & par les côtés une pareille longueur des  
“ cordons de soie, sans leur toucher.

“ Il s’agissoit de faire, dans le tems de l’orage, deux  
“ observations sur cette verge de fer ainsi disposée; l’une  
“ étoit de remarquer à sa pointe une aigrette lumineuse,  
“ semblable à celle que l’on apperçoit à la pointe d’une  
“ aiguille, quand on l’oppose assez près d’un corps  
“ actuellement électrisé; l’autre étoit de tirer de la  
“ verge de fer des étincelles, comme on en tire du  
“ canon de fusil dans les expériences électriques; & a-  
“ fin de se garantir des piquûres de ces étincelles, j’avois  
“ attaché le tenon d’un fil d’archal au cordon d’une  
“ longue fiole pour lui servir de manche. . . .

“ Le Mercredi 10 Mai 1752, entre deux & trois heures  
“ après midi, le nommé Coiffier, ancien dragon, que  
“ j’avois



“ j’avois chargé de faire les observations en mon ab-  
 “ sence, ayant entendu un coup de tonnerre assez fort,  
 “ vole aussitôt à la machine, prend la fiole avec le fil  
 “ d’archal, présente le tenon du fil à la verge, en voit  
 “ sortir une petite étincelle brillante, & en entend le  
 “ pétilllement ; il tire une seconde étincelle plus fort  
 “ que la première & avec plus de bruit ! il appelle ses  
 “ voisins, & envoie chercher M. le Prieur. Celui-ci  
 “ accourt de toutes ses forces ; les paroissiens voyant  
 “ la précipitation de leur Curé, s’imaginent que le  
 “ pauvre Coiffier a été tué du tonnerre ; l’alarme se  
 “ répand dans le village : la grêle qui survient n’em-  
 “ pêche point le troupeau du suivre son pasteur. Cet  
 “ honnête Ecclésiastique arrive près de la machine, &  
 “ voyant qu’il n’y avoit point de danger, met lui-même  
 “ la main à l’œuvre & tire de fortes étincelles. La  
 “ nuée d’orage & de grêle ne fut pas plus d’un quart-  
 “ d’heure à passer au zénith de notre machine, & l’on  
 “ n’entendit que ce seul coup de tonnerre. Sitôt que  
 “ le nuage fut passé, & qu’on ne tira plus d’étincelles,  
 “ de la verge de fer, M. le Prieur de Marly fit partir le  
 “ sieur Coiffier lui-même, pour m’apporter la lettre sui-  
 “ vante, qu’il m’écrivit à la hâte.

*Je vous annonce, Monsieur, ce que vous attendez : l’expé-  
 rience est complète. Aujourd’hui à deux heures 20 minutes  
 après midi, le tonnerre a grondé directement sur Marly ; le  
 coup a été assez fort. L’envie de vous obliger, & la curiosité  
 m’ont tiré de mon fauteuil, où j’étois occupé à lire : je suis  
 allé chez Coiffier, qui déjà m’avoit dépêché un enfant que  
 j’ai rencontré en chemin, pour me prier de venir ; j’ai doublé  
 le*



le pas à travers un torrent de grêle. Arrivé à l'endroit où est placée la tringle coudée, j'ai présenté le fil d'archal, en avançant successivement vers la tringle, à un pouce & demi, ou environ ; il est sorti de la tringle une petite colonne de fer bleuâtre sentant le soufre, qui venoit frapper avec une extrême vivacité le tenon du fil d'archal, & occasionnoit un bruit semblable à celui qu'on feroit en frappant sur la tringle avec une clef. J'ai répété l'expérience au moins six fois dans l'espace d'environ quatre minutes, en présence de plusieurs personnes, & chaque expérience que j'ai faite a duré l'espace d'un pater & d'un ave. J'ai voulu continuer ; l'action du feu s'est ralentie peu à peu ; j'ai approché plus près, & n'ai plus tiré que quelques étincelles, & enfin rien n'ai paru.

Le coup de tonnerre qui a occasionné cet événement, n'a été suivi d'aucun autre ; tout s'est terminé par une abondance de grêle. J'étois si occupé dans le moment de l'expérience de ce que je voyois, qu'ayant été frappé au bras un peu au-dessus du coude, je ne puis dire si c'est en touchant au fil d'archal ou à la tringle : je ne me suis pas plaint du mal que m'avoit fait le coup dans le moment que je l'ai reçu ; mais comme la douleur continuoit, de retour chez moi, j'ai découvert mon bras en présence de Coiffier, & nous avons apperçu une meurtrissure tournante autour du bras, semblable à celle que feroit un coup de fil d'archal, si j'en avois été frappé à nud. En revenant de chez Coiffier, j'ai rencontré M. le Vicaire, M. de Milly, et le Maître d'école, à qui j'ai rapporté ce qui venoit d'arriver ; ils se sont plaints tous les trois qu'ils sentoient une odeur de soufre qui les frappoit davantage à mesure qu'ils s'approchoient de moi : j'ai porté chez moi la même odeur, & mes domestiques s'en sont apperçus sans que je leur aye rien dit.

Voilà



*Voilà, Monsieur, un récit fait à la hâte, mais naïf & vrai que j'atteste, & vous pouvez assurer que je suis prêt à rendre témoignage de cet événement dans toutes les occasions. Coiffier a été le premier qui a fait l'expérience & l'a répétée plusieurs fois ; ce n'est qu'à l'occasion de ce qu'il a vu qu'il m'a envoyé prier de venir. S'il étoit besoin d'autres témoins que de lui & de moi, vous les trouveriez. Coiffier presse pour partir.*

*Je suis avec une respectueuse considération, Monsieur, votre, &c. signé RAULET, Prieur de Marly. 10 Mai 1752.*

“ On voit, par le détail de cette lettre, que le fait  
“ est assez bien constaté pour ne laisser aucun doute à  
“ ce sujet. Le porteur m'a assuré de vive voix qu'il  
“ avoit tiré pendant près d'un quart-d'heure avant  
“ que M. le Prieur arrivât, en présence de cinq ou six  
“ personnes, des étincelles plus fortes & plus bruy-  
“ antes que celles dont il est parlé dans la lettre. Ces  
“ premières personnes arrivant successivement, n'osoient  
“ approcher qu'à 10 ou 12 pas de la machine ; & à  
“ cette distance, malgré le plein soleil, ils voyoient les  
“ étincelles & entendoient le bruit. ....

“ Il résulte de toutes les expériences & observations  
“ que j'ai rapportées dans ce mémoire, & surtout de  
“ la dernière expérience faite à Marly-la-ville, que la  
“ matière du tonnerre est incontestablement la même  
“ que celle de l'électricité. L'idée qu'en a eu M.  
“ Franklin cesse d'être une conjecture : la voilà de-  
“ venue une réalité, & j'ose croire que plus on appro-  
“ fondira tout ce qu'il a publié sur l'électricité, plus  
“ on reconnoîtra combien la Physique lui est redevable  
“ pour cette partie.”



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. *Trans.* 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, tho' 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



which served to 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  
 “ opportunity of trying Mr. *Franklin's* experiment of  
 “ extracting 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  
 “ soldered 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 approach-  
 “ ing it a second time, I received the spark at the dis-  
 “ tance 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 electrified at all. The rain continued  
 “ during the thunder, but was considerably abated at  
 “ the time of making the experiment.” Thus far  
 Mr. *Canton*.



Mr. *Wilfon* 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*,

TO

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

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



rising a foot or more 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 lightning completely demonstrated.

B. FRANKLIN.

LET-



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# LETTER XII.

FROM

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

TO

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

S I R,

*Philadelphia, September, 1753.*

I N my former paper on this subject, written 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, produced 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, though 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



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 spark was obtained, another could not be got for some time after; at other times



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 electrified *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, through the prime conductor: When charged, 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 electrified *positively*, the ball being attracted and repelled by one, must be also repelled by the other. If the one *positively*, and the other *negatively*; then the ball would be attracted and repelled 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 though I had given orders in my family, that if the bells rang when I was from home, they should catch some of the light-

had I

R

ning



ning 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 heightened 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 electrified *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. *Kinnerley*, since printed in *London*) that the glass globe electrifies *positively*, I concluded that the clouds are *always* electrified *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 electrified *positively*, though several that passed 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 electrified *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 charged 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 discharged.

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 charged phial to the rod, instead of the usual stream that I expected from the wire to the rod, there was no spark; not even when I brought the wire and the rod to touch; yet the bells continued ringing vigorously, which proved to me, that the rod was then *positively* electrified, as well as the wire of the phial, and equally so; and, consequently, that the particular cloud then over the rod, was in the same positive state. This was near the end of the gust.

But this was a single experiment, which, however, destroys my first too general conclusion, and reduces me to this: *That the clouds of a thunder-gust are most commonly in a negative state of electricity, but sometimes in a positive state.*

The latter I believe is rare; for though I soon after the last experiment, set out on a journey to *Boston*, and was from home most part of the summer, which prevented my making farther trials and observations; yet Mr. *Kinnersley* returning from the islands just as I left home, pursued the experiments during my absence, and informs me that he always found the clouds in the *negative* state.

So that, for the most part, in thunder-strokes, *it is the earth that strikes into the clouds, and not the clouds that strike into the earth.*

Those who are versed in electric experiments; will easily conceive, that the effects and appearances must be nearly the same in either case; the same explosion, and the same flash between one cloud and another, and between the clouds and mountains, &c. the same  
rending



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 though 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



nor in the 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 compared 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 condensed by being squeezed 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 condensed, 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



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 produced 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 electrified positively, I conjecture that, in that case, such cloud, after having received what was, in its rare state, only its *natural quantity*, became compressed by the driving winds, or some other means,



means, so that part of what it had absorbed was forced out, and formed an electric atmosphere 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 ceiling, 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 ceiling, and let it gradually sink into the can again.---From the ceiling, 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 flowed 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 diminished 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 gathered 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 absorb 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



factory to myself: I thought, 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 through 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 through, so as to prevent an explosion; but when the fluid comes in a quantity too great to be confined by such cohesion, it explodes, and rends or fuses the body that endeavoured to confine it. If it be wood, brick, stone, or the like, the splinters will fly off on that side where there is least resistance. And thus, when a hole is struck through pasteboard by the electrified jar, if the surfaces of the pasteboard are not confined or compressed, there will be a bur raised all round the hole on both sides the



pasteboard; but if one side be confined, so that the bur cannot be raised on that side, it will be all raised 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, though 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 incomplete, dis-united, 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 complete 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



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, discharge a very great quantity of electricity, which nevertheless will be all conducted round the corner of a book, by the fine filleting 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 of 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 jars, which is a quantity, I imagine, far beyond what was ever contained 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, though 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



it become incapable of conducting another. And considering the extreme rapidity with which the electric fluid moves without exploding, when it has a free passage, or compleat metal communication, I should 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 discharged 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



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 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 electrified 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.

B. F.

L E T



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## LETTER XIII.

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

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

S I R, *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, though 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 fortnight after that, he made another observation of the same kind; and last

*Monday*



*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 used. 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 repelled, 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.

B. FRANKLIN,

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# R E M A R K S

On the Abbé NOLLET's  
LETTERS on ELECTRICITY,  
TO  
BENJ. FRANKLIN, *Esq;* of *Philadelphia*.

B Y  
Mr. DAVID COLDEN, of *New York*.

S I R, Coldenham, in N. York, Dec. 4, 1753.  
**I**N considering the Abbé *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 Abbé'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



of the magical picture, and writes thus of it. "When  
 " you electrify a pane of glass coated on both sides  
 " with metal, it is evident that whatever is placed on  
 " the side opposite to that which receives the electri-  
 " city 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 electri-  
 cal virtue to any thing in contact with it, till it is  
 entirely discharged of its electrical fire. To which the  
 Abbé thus objects: "Tell me, (says he) I pray you,  
 " how much time is necessary for this pretended dis-  
 " charge? I can assure you, that after having main-  
 " tained the electrification for hours, this surface which  
 " ought, as it seems to me, to be entirely discharged  
 " of its electrical matter, considering either 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  
 " rather better electrified thereby, and more proper to  
 " produce all the effects of an actual electric body."  
 p. 68.

The Abbé 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 elec-  
 tricity 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-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, charged with electricity, have. Some of the effects of an electric body, which I suppose the Abbé 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



they endeavour to attract the 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 electrify 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 Abbé says, *p.* 69. “that he can electrify 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 turned, 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 electrified. After the electric matter was spewed out as  
above

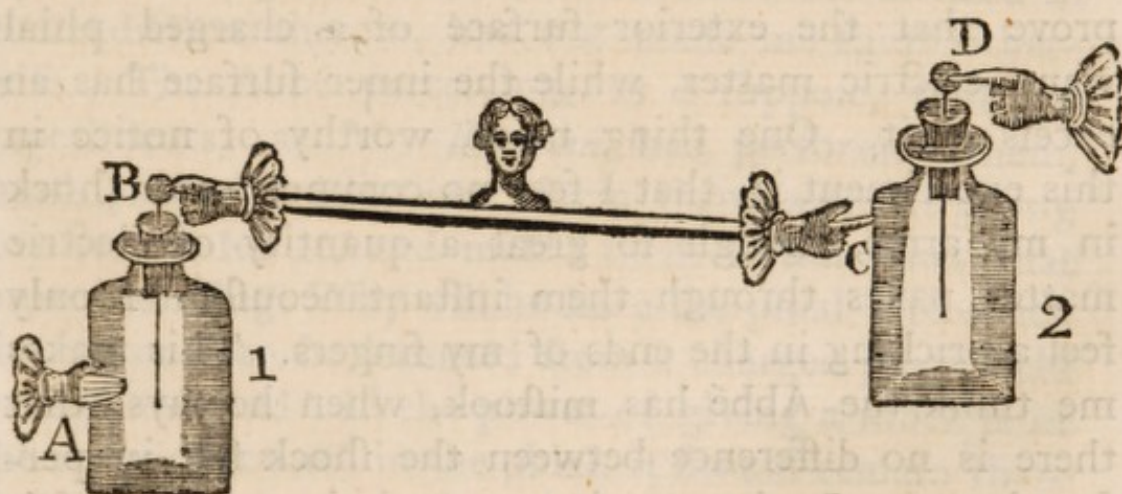


above from the hook of a phial prepared for the *Leyden* experiment, I hung another phial, in like manner prepared, to a hook fixed in the coating of the first, and held this other phial in my hand; now if there was any electric matter transmitted through the glass of the first phial, the second one 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 discharged. If the Abbé's opinion be right, that the exterior surface, communicating with  
the



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 stirred 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 me what the hook



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, though so great a quantity of electric matter passes through them instantaneously: I only feel a pricking in the ends of my fingers. This makes me think the Abbé 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 communication, by my arms, from the hook to the coating of the same phial; because when it was taken  
into



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 entered.

The next thing I meet with is in the Abbé'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 Abbé has asserted in contradiction thereto, has not made me think otherwise. The Abbé perceiving, as I suppose, that the experiments, as Mr. *Franklin* had performed 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 contained 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 Abbé



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 electrified; 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 electrified 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 contained 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 that pleases to try. This experiment fully evidences, that the water in the phial contains no more electric matter than it would do in an open basin, and has not any of that great quantity which produces  
the



the shock, and is only retained by the glass. If after the spark is drawn from the conductor, you touch the coating of the phial (which all this while is supposed to hang in the air, free from any non-electric body) the threads on the conductor will instantly start up, and shew that the conductor is electrified. It receives this electrification from the inner surface of the phial, which, when the outer surface 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 excess. It is diverting to see how the threads will rise and fall by touching the coating and conductor of the phial alternately. May it not be that the difference between the charged side of the glass, and the outer or emptied side, being lessened by touching the hook or the conductor; the outer side can receive from the hand which touched it, and by its receiving, the inner side cannot retain so much; and for that reason so much as it cannot contain electrifies the water, or filings and conductor: For it seems to be a rule, that the one side must be emptied in the same proportion that the other is filled: Though this from experiment appears evident, yet it is still a mystery not to be accounted for.

I am in many places of the Abbé'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 observed them to do. The Abbé, 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 Abbé 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 ten o'clock at night till eight 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 raised 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 Abbé has asserted the contrary of this, p. 101, in order to account for the playing of a cork-ball between the 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 Abbé says, p. 103, "that a piece of metal leaf  
" hung to a silk thread and electrified, will be repelled  
" by the bottom of a charged phial held by its hook  
" in the air:" This I find constantly otherwise, it is  
with



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 Abbé, *p.* 108, says, “that it is not impossible, “as Mr. *Franklin* says it is, to charge a phial while “there is a communication formed between its coating and its hook.” I have always found it impossible to charge such a phial so as to give 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 Abbé, 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 it sifts through the glass, and charges the exterior surface, according to the Abbé’s system!

The Abbé’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 concealed 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 Abbé has done.



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# 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.

#### EXPERIMENT I.

**F**ROM 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



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.

## E X P E R I M E N T 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 conjectured 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; though some, perhaps is continually entering and passing through 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 through the threads into  
the



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

## E X P E R I M E N T      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 charged phial, the balls will be affected in the same manner at the approach of excited wax, or the wire of the phial.

## E X P E R I M E N T      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



The bringing the excited glass to the end, or edge of the tin-tube, in the third experiment, is supposed 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 diminished; 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 contained in cork; their repulsion will be quite destroyed. But if the tube be brought nearer; the fluid without, 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 imbibed 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

X

increased,



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.



affected. By the approach of the excited 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 edge 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.

#### E X P E R I M E N T 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 rendered 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, notwithstanding, continue



to repel each other; though not at so 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 touched 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 considered 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 off through  
the



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

## E X P E R I M E N T 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 discovered, by a charged phial, whether the electrical fire be drawn out of the apparatus by a negative cloud, or forced  
into



into it by a 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 electrified for ten minutes, or a quarter of an hour, after the clouds have passed the zenith; and sometimes till they appear more than halfway 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 entirely 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



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?





## A P P E N D I X.

**A**S 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 killed 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 was followed by a violent quick trembling in the trunk, which went off gradually



dually 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, though 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, though 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.



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

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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. **E**LECTRIC 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 approach-  
ing 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 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 you 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 withdrawn, it takes with it its whole atmosphere



mosphere, and leaves the prime-conductor electrified 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 electrified 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 electrified 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.

EXPE-



EXPERIMENT III.

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

Part of their natural quantity is thereby driven out of them into the prime-conductor, and they become negatively electrified, 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 electrified, 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 electrified, as well as the lock of cotton so situated.

*Extract*



*Extract of a Letter concerning Electricity, from Mr. B. Franklin, to Mons. D'Alibard, at Paris, inclosed in a Letter to Mr. Peter Collinson, F. R. S.*

*Philadelphia, June 29, 1755.*

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

YOU desire my opinion of *Pere Beccaria's Italian* book \*. I have read it with much pleasure, and think it one of the best pieces on the subject that I have seen in any language. Yet as to the article of water-spouts, I am not at present of his sentiments; though I must own with you, that he has handled it very ingeniously. Mr. Collinson has my opinion of whirlwinds and water-spouts at large, written some time since. I know not whether they will be published; if not, I will get them transcribed for your perusal. It does not appear to me that *Pere Beccaria* doubts of the *absolute impermeability of glass* in the sense I meant it; for the instances he gives of holes made through glass by the electric stroke, are such as we have all experienced, and only shew that the electric fluid could not pass without making a hole. In the same manner we say, glass is

\* This work is written conformable to Mr. *Franklin's* theory, upon artificial and natural Electricity, which compose the two parts of it. It was printed in *Italian* at *Turin*, in 4<sup>to</sup>. 1753; between the two parts is a letter to the *Abbé Nollet*, in defence of Mr. *Franklin's* system. *J. Bevis.*

impermeable



impermeable to water, and yet a stream from a fire-engine will force through the strongest panes of a window. As to the effect of points in drawing the electric matter from clouds, and thereby securing buildings, &c. which, you say, he seems to doubt, I must own I only think he 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 of 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

\* Notwithstanding this complaint of the author's, repeated in four editions of his papers, he continues to be misunderstood and misrepresented in this particular, as if he considered it as the *sole* use of pointed rods, that they might *draw off* the lightning from the clouds and, *prevent* a stroke; and the very instances adduced by him to show that the rods had protected houses, by *conducting* the lightning *when there was a stroke*, proved by its having melted the points, are said to be instances of conductors being UNSUCCESSFUL, — in a pamphlet entitled, *Observations upon Lightning, &c.* just published, 1773.



*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 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 kneedle. 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 gimblet-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



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 afore-mentioned 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 towards 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.

LETTER



# LETTER XIII.

T O

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

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

**I**N my last, *via Virginia*, I promised to send you per next ship, a small philosophical packet: But now having got the materials (old letters and rough drafts) before me, I fear you will find it a great one. Nevertheless, 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 Letter 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 his part of 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*



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

S I R,

*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,



South, the effect entirely ceases. We made at *Faneuil* Hall, where Mr. K——'s apparatus is, several experiments to give some small needles the magnetic virtue; previously examining, by putting them on 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

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

density:



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 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 accumulated 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 phænomenon 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



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 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 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 extremely subtil and active, would be immediately communicated, either to those lower parts



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, 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



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L E T T E R XIV.

F R O M

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

S I R,

*Philadelphia, Jan. 24, 1752.*

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

I AM glad to learn, by your favour of the 21<sup>st</sup> past, that Mr. *Kinnerfley's* lectures have been acceptable to the gentlemen of *Boston*, and are likely 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



glass globe, on a non-electric cushion ; and that, notwithstanding the surprising activity and swiftness of that fluid, and the non-electric communication between all parts of the cushion and the earth, yet quantities would be snatched up by the revolving surface of the globe, thrown on the prime-conductor, and dissipated in air. How this was done, and why that subtle 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 spheres, 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



found that salt in the water of an electric phial, does not lessen the shock; 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



the rest broke into small pieces, from half an inch long, to half a quarter of an inch. 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. FRANKLIN.

B b LETTER



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# LETTER XV.

FROM

J. B. Esq. at *Boston*,

TO

BENJAMIN FRANKLIN, Esq; at *Philadelphia*.

S I R,

*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. *Kinnerfley* 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 polar 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 needle varies from a North and South



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 thro' 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 been continued to



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 *Newbery*, p. 170.] The steeple, when repaired, was guarded by an iron conductor, or rod, extending from the foot of the vane-spindle down to the outside of the building, into the earth.—The news-papers 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*



*Physical and Meteorological Observations, Con-  
jectures, and Suppositions ; by B. F.*

Read at the Royal  
Society, June 3,  
1756.

**T**HE 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 repulency 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, though laid at rest at the bottom of a vessel of water, will dissolve therein, and its parts  
move



move every way, till equally diffused in the water; therefore there is a 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 saltiness 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



in dry seasons: Showers wash it from the air, 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 adhere to, by which they  
might



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

The water commonly diffused 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 dissolved 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 breathed, 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 particle of air loaded with adhering water, or 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 (confined 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 pushed 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 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



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-four hours, 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 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



He that falls, 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.

\* See a paper on this subject, by the late ingenious Mr. *Hadley*, in the *Philosophical Transactions*, wherein this hypothesis for explaining the trade-winds first appeared.



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,



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

\* In *Pensylvania*.

downwards;



downwards ; if ascending, greatest below, and lessening upwards ; like a speaking trumpet, standing its big end on the ground.

When the air descends with violence in some places, it may rise 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 sides, the whirling air, as an arch, preventing ; it must then press in at the open ends.

The greatest pressure inwards must be at the lower end, the greatest weight of the surrounding atmosphere being there. The air entering, rises within, and carries up dust, leaves, and even heavier bodies that happen in its way, as the eddy, or whirl, passes over land.

If it passes over water, the weight of the surrounding atmosphere 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 still farther from the center or axis of the trump, as the pressure lessens ; and at last, as the trump widens, is broken into small particles, and so united with air as to be supported by it, and become black clouds at the top of the trump.

Thus these eddies may be whirlwinds at land, water-spouts at sea. A body of water so raised, may be suddenly let fall, when the motion, &c. has not strength to support it, or the whirling arch is broken so as to admit the air ; falling in the sea, it is harmless, unless  
ships



ships happen under it. But if in the progressive motion of the whirl, it has moved from the sea, over the land, and there breaks, sudden, violent, and mischievous torrents are the consequence.

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## LETTER XVI.

From Dr. \* \* \* of *Boston*,

T O

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

S I R,

*Boston, Aug. 5, 1751.*

THIS comes to you on account of Dr. *Douglas*: 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  $11 \frac{1}{2}$ , but had we been experienced in this way, at the first coming of the distemper, probably the proportion had been but one 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.

LETTER



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L E T T E R XVII.

FROM

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

To Doctor ——— of *Boston*.

S I R,

*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 twenty-two 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 four. 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. *Douglas* 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 doute 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 will 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 Philosophy can be founded. And, I assure you, nothing can 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.

OBSERVATIONS



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

1. **T**ABLES of the proportion of marriages to births, of deaths to births, of marriages to the number 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 height; those who cannot get land, must labour for others that have it; when labourers are plenty,  
their



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

7. Hence



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 *Pensylvania*, 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 interfering 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 cannot interfere; which will increase, in a short time, even beyond her power of supplying,



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 will engross as many offices, and exact as much



much tribute or profit on the labour of the conquered, as will maintain them in their new establishment; and this diminishing the subsistence of the natives, discourages their marriages, and so gradually diminishes them, while the foreigners increase. 2. Loss of territory. Thus the *Britons* being driven into *Wales*, and crowded together in a barren country, insufficient to support such great numbers, diminished, till the people bore a proportion to the produce, while the *Saxons* increased on their abandoned lands, till the island became full of *English*. And, were the *English* now driven into *Wales* by some foreign nation, there would, in a few years, be no more *Englishmen* in *Britain*, than there are now people in *Wales*. 3. Loss of trade. Manufactures exported, draw subsistence from foreign countries for numbers; who are thereby enabled to marry and raise 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 soon be deprived of so many people. 4. Loss of food. Suppose a nation has a fishery, which not only employs great numbers, but makes the food and subsistence of the people cheaper: if another nation becomes master of the seas, and prevents the fishery, the people will diminish in proportion as the loss of employ, and dearth of provision makes it more difficult to subsist a family. 5. Bad government and insecure property. People not only leave such a country, and settling abroad incorporate with other nations, lose 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 trades, arts, or manufactures, or new improvements in husbandry, may be properly called



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 diminish the families that indulge in it, who are few. The greater the common fashionable



expence of any rank of people, the more cautious they are of marriage. Therefore luxury should never be suffered to become common.

19. The great increase of offspring in particular families, is not always owing to greater fecundity of nature, but sometimes to examples of industry in the heads, and industrious education; by which the children are enabled to provide better for themselves, and their marrying early is encouraged from the prospect of good subsistence.

20. If there be a sect, therefore, in our nation, that regard frugality and industry as religious duties, and educate their children therein, more than others commonly do; such sect must consequently increase more by natural generation, than any other sect in *Britain*.

21. The importation of foreigners into a country that has as many inhabitants as the present employments and provisions for subsistence will bear, will be in the end no increase of people, unless the newcomers have more industry and frugality than the natives, and then they will provide more subsistence, and increase in the country; but they will gradually eat the natives out. — Nor is it necessary to bring in foreigners to fill up any occasional vacancy; for such vacancy (if the laws are good, § 14, 16) will soon be filled by natural generation. Who can now find the vacancy made in *Sweden*, *France*, or other warlike nations, by the plague of heroism 40 years ago; in *France*, by the expulsion of the Protestants; in



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 of *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



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 tenfold in numbers and strength.

\* \* \* \* \*

\* In 1751.

† A water insect, well-known to Naturalists.



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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 vorticem*; 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 abovesaid.

Descending



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 rarefaction 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, tho' the relators seemed to think they took up water.

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## LETTER XIX.

From Dr. ———, of *Boston*,

T O

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

S I R,

*Boston, October 23, 1752.*

Read at the Royal  
Society, June 24,  
1756.

I N 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

F f

doubt

† Water-spouts.



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

Some



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 it otherwise would go; and by the last making the top of the bush appear to bend outwards (*i. e.*) the cloud of spray is forced 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 reversed ; 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 immersed in the bush, a little blackened 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 thro' 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 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



pose 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, (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



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 a-cross 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 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* N<sup>o</sup>. 11, which is curious for its inequalities, and, in particular, the approach to breaking, which, if it would  
not



not be too tedious, I would have observed 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.

LETTER



## L E T T E R      X X.

F R O M

BENJ. FRANKLIN, *Esq*; of *Philadelphia*.To Doctor ——— of *Boston*.

S I R,

*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 hence a sure advantage results;  
for



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 enquiries, 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 rise in such vacuum to that height, or to a 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  
“ phænomenon may be solved by suction (improperly  
“ so called) or rather pulsion, as in the application of  
“ a cupping 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.

Whirlwinds have, generally, a progressive, as well as a circular motion; so had what is called the spout, at *Topsam*—(See the account of it in the *Transactions*)—which also appears, by its effects described, to have  
been



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 *Acurement-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 whaler 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 phenomena 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 concentrating 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 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 the tiles, shingles, or thatch, force a boat down into the water, or a piece of timber into the earth,



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, 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 abridged 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



by its continually lightning or emitting flashes on all sides, pushing along with a surprising 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 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



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 mile 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 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.

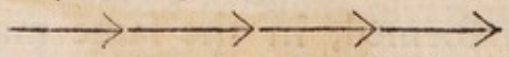
And,



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 approaches the region of the clouds, till it ends in a point, as at P in Fig. II. Plate 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 *a a a a* and *b b b b* 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



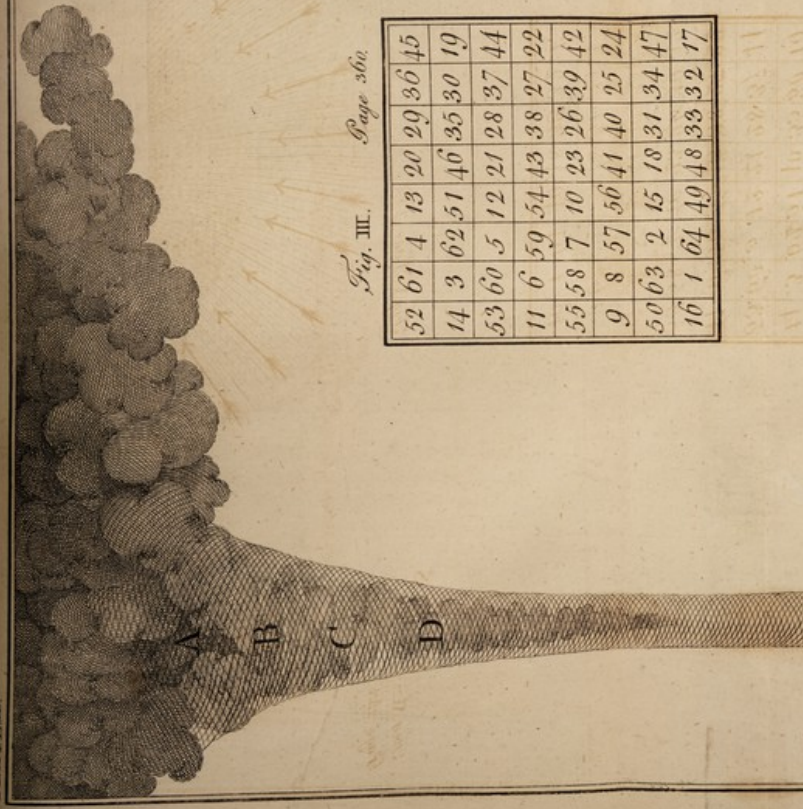


Fig. III. Page 360.

|    |    |    |    |    |    |    |    |
|----|----|----|----|----|----|----|----|
| 52 | 61 | 4  | 13 | 20 | 29 | 36 | 45 |
| 14 | 3  | 62 | 51 | 40 | 35 | 30 | 19 |
| 53 | 60 | 5  | 12 | 21 | 28 | 37 | 44 |
| 11 | 6  | 59 | 54 | 43 | 38 | 27 | 22 |
| 55 | 58 | 7  | 10 | 23 | 26 | 39 | 42 |
| 9  | 8  | 57 | 56 | 41 | 40 | 25 | 24 |
| 50 | 63 | 2  | 15 | 18 | 31 | 34 | 47 |
| 16 | 1  | 64 | 49 | 48 | 33 | 32 | 47 |

S P S

Fig. II. Page 233.

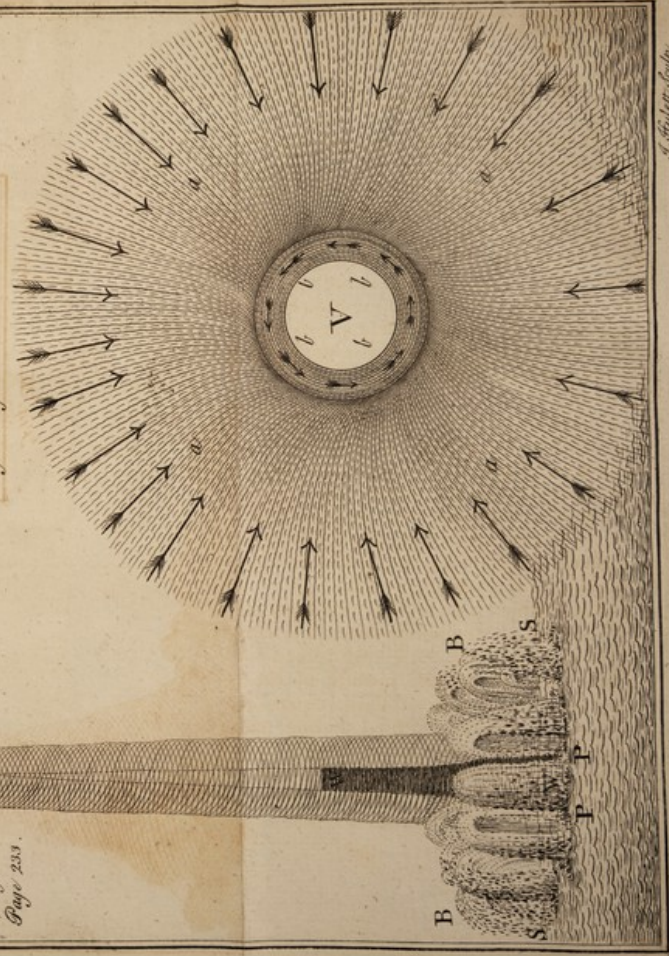


Fig. I. Page 232.







It is this whirling body of air between *a a a a* and *b b b b* 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 *a a a a* and *b b b b* 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 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  
H h
filled



filled so much of it. S S S S, the spiral whirl of air furrounding 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*, furrounding 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 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



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.

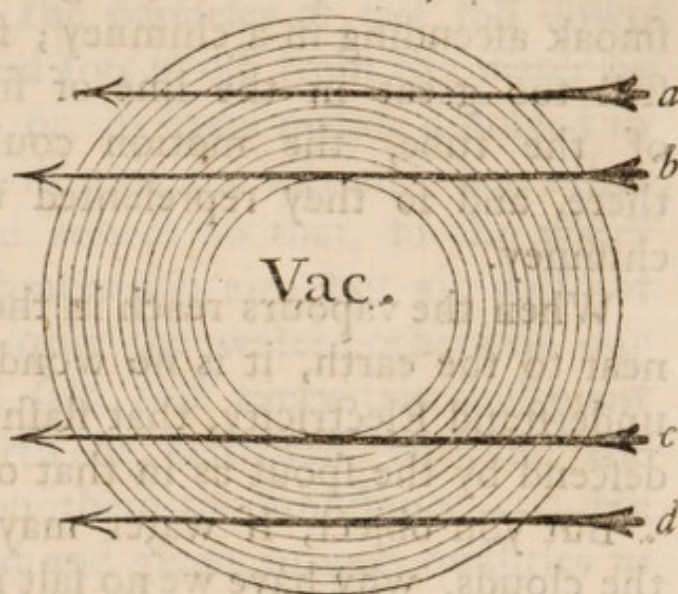
Dr. *Stuart* says, "It was observable of all the spouts he saw, but more perceptible of the great one; that, towards the end, it began to appear like a hollow canal, only black in the borders, but white in the middle; and though at first it was altogether black and opaque, yet, now, one could very distinctly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

And Dr. *Mather*, describing a whirlwind, says, "a thick dark small cloud arose, with a pillar of light in it, of about eight or ten feet diameter, and passed along the ground in a tract not wider than a street, horribly 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



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 direction of the arrows, and suppose opaque particles to be equally mixed 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





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 of *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, 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,



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 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 think I formerly read in *Dampier*, or some other voyager, that a spout, in its progressive motion, went over a ship becalmed, on the coast of *Guinea*, and first threw her down on one side, carrying away her fore-mast, then suddenly whipped 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 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



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.

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## LETTER XXI.

From Doctor *M—r*,

T O

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

S I R, *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 i

I had



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 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 wharf 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, tho' 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

\* I suppose shingles, staves, timber, and other lumber, might be lying in quantities on the wharf, for sale, as brought from the Northern colonies. B. F.

from



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 thro' 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.



## LETTER XXII.

FROM

Doctor ———, of *Boston*,

TO

BENJAMIN FRANKLIN, Esq; of *Philadelphia*.

S I R,

*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 thro' a non-electric medium. Electric fire in our globe is always in action, sometimes ascending, descending, or passing



passing from region to region. 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*



---

*A subsequent PAPER from the same.*

Read at the Royal  
Society, July 8,  
1756,

**S**POUTS 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 *Straits 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 *Straits of Malacca*, 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. However, I believe it impossible  
to



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 *Straits 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. Agreeable 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 *Straits 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 degree, the spout emerges from the cloud,



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 condensation. Or more plainly thus: The descending vapour which

K k

forms



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 phenomena 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 to; it being then pretty cool for the season and climate; and this is worth remark, because cool air is weighty, and will not ascend; besides, when the air grows cool, it shews that the upper region descends,  
and



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 seawater 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 traditional 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 supposable causes, not above one foot, as may appear more plainly by and by. Supposing 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



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 reduced 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



conceive how a condensative power should instantaneously take place of a rarifying 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 cannot 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 does not appear that the above-mentioned different degrees of heat and cold concur in any region where spouts usually happen, nor, indeed, in any other.



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*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, with 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 very rarified air, may be lighter than a  
quantity



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 do not 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. Do not we see the air overloaded, and casting forth water plentifully when there is no suffocation?



Then 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 supplied by air from Northern and Southern  
" latitudes, which coming from parts where the air  
" and earth had less motion, and not suddenly ac-  
" quiring the quicker motion of the equatorial earth,  
" appears an East wind blowing Westward; the earth  
" moving from West the 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.



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*Observations in Answer to the foregoing; by B. F.*

Read at the Royal  
Society, Nov. 4.  
1756.

1st. **T**HE 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 crushed 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 immersed 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 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



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 season, 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 *Philippine 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*



*Observations on the METEOROLOGICAL PAPER ;  
sent by a Gentleman in New-York to B. F.*

Read at the Royal  
Society, Nov. 4,  
1756.

**T**HAT 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 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



ceive 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 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



sion 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 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



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 overfet, or foundered at sea suddenly. I have heard of vessels being overfet when it was perfectly calm, the instant before the stream of wind struck them, and immediately after they were overfet; 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 remarkable intestine motion, like a violent fermentation, is very observable in the cloud from whence the spout issues. No salt water, I am persuaded, was ever observed to fall from the clouds, which must certainly have happened if sea-water had been raised by a spout.



## ANSWER to the foregoing Observations, by B. F.

Read at the Royal  
Society, Nov. 4,  
1756.

I Agree with you, that it seems absurd to suppose that a body can act where it is not. I have no idea of bodies at a distance attracting or repelling one another without the assistance of some medium, though I know not what that medium is, or how it operates. When I speak of attraction or repulsion, I make use of those words for want of others more proper, and intend only to express *effects* which I see, and not *causes* of which I am ignorant. When I press a blown bladder between my knees, and find I cannot bring its sides together, but my knees feel a springy matter, pushing them back to a greater distance, or repelling them, I conclude that the air it contains is the cause. And when I operate on the air, and find I cannot by pressure force its particles into contact, but they still spring back against the pressure, I conceive there must be some medium between its particles that prevents their closing, tho' I cannot tell what it is.---And if I were 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



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; 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



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**



## LETTER XXIII.

FROM

BENJ. FRANKLIN, *Esq*; of *Philadelphia*.To C. C. *Esq*; at *New-York*.

S I R,

*Philadelphia, April 23, 1752.*

Read at the Royal  
Society, Nov. II,  
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 educed 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, thro' 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 ætherial fire. Perhaps the *Auroræ Boreales* are currents of this fluid in its own region,  
above



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 such a waste of matter; and the planets, instead of drawing nearer to him,



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 subtile 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 colour, some white, blue, purple, red; the strongest, white; weak ones red. Thus different de-



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 diminish 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



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 publicly 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.



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*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 seawater 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



it 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*, sometime 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



of the wind still lasted, and took the ship on the star-board-bow with such violence, that it snapt off the bolt-sprit 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 bolt-sprit 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 foretop when the foremast broke, and one on the bolt-sprit, 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 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 descending from any dark cloud, and, therefore, the more strange to me, I never having seen the



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. p. 223.

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*Account of another SPOUT. From the same.*

"**W**E 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. p. 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.



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*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-illuminated. 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 with my finger, it was renewed. I then carried the cloth to the light,

O o

but



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 fins, 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 be more numerous in some places than others; and, therefore, that the appearance  
above-



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.

*Further* 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 chymistry, thinks that heat is propagated by the vibration of a subtle elastic fluid, dispersed through the



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 pleasure to come.

The



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 direction in which the cloud moves and precedes it, yet wind issues from



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.



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*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. 16,  
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*,



*Schuylkill*, 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 *Sasquehanna* 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 *Sasquehanna*, or *Potomack*, in that the electric fire would have a circuit of some thousands of miles to go down *Ohio* to *Mississippi*, 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.



*Answer to the foregoing ; by B. F.*

Read at the Royal  
Society, Dec. 23,  
1756.

**S**UPPOSE 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



the 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 extreme 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 *Susquehanna*, 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.



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*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.*

**I**N 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 convenience 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)



bled) will of course grow scarcer and dearer, any new proposal for saving the 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 condensed 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 not quite full of air, tie the neck tight, and lay it before



before a fire as near as may be without scorching the bladder; 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 (*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 inconveniencies 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 purposed 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 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 chimneys 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 funne 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 defluations, 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, pleurifies, &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

\* 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 necessarily 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 openings 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 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



mean time, very little is done by these chimneys towards warming the room; for the air round the fireplace, 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.

“ 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 *Antonino Parcio*, 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 sævit mensibus Dec. Jan. Feb. ex cæli, aërisque inclementia facta est, quod non habeant hypocausta* [stove-rooms] *et quod non solliciti 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 absumpti; 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 through 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 antient 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 fuel) is wasted, and contributes nothing towards warming the room.

3. To remedy this, the *Sieur Gauger* gives, in his book entitled, *La Meehanique 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 hearths, 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 chimneys, discouraged the propagation of the invention; so that there are, I suppose, very few such chimneys 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 conveniences 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 fuel 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 ceiling, 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 spoken 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



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 screwed 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 unchanged 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,



it, render it disagreeable, hurtful to 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



rods O P; a long air-hole, *aa*, through which the fresh outward air passes up 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



when the plates are put together, and the joints luted, there is no communication between the air-box and the smoke. In the winding passages of this box, fresh air is warmed 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 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, *dd*, 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 put this machine to work,

R r

I. A



1. A false back of four inch (or, in shallow small chimnies, 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 through 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:



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.

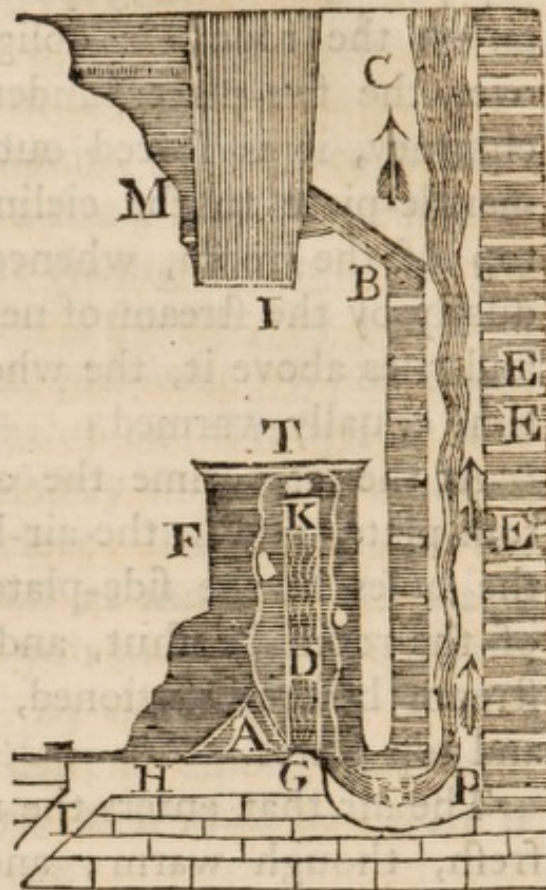
*K* The hole in the side-plate, through which the warmed air is discharged out of the air-box into the room.

*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 shew 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 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 mantle-piece to the cieling, and spreads all over the top of the room, whence being crouded down gradually by the stream of newly-warmed 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



against the back of the chimney when open, it closes the vacancy behind the false back, and shoots the foot, that falls in sweeping, 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 closed or opened at pleasure. When open, there will be a strong draught of air through 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 the fire in the morning



ing is very advantageous, as it immediately, by its sudden blaze, heats the plates, and warms the room, (which with bad wood slowly kindling would not be done so soon) and at the same time, by the length of its flame, turning in the passages, consumes and cleanses away the soot that such bad smoaky wood had produced therein the preceding day, and so keeps them always free and clean.---When you have laid a little back log, and placed your billets on small dogs, as in common chimneys, and put some fire to them, then slide down your shutter as low as the dogs, and the opening being by that means contracted, the air rushes in briskly, and presently blows up the flames. When the fire is sufficiently kindled, slide it up again \*. In some of these fire-places there is a little six-inch square trap-door of thin wrought iron or brass, covering a hole of like dimensions 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 springing sides fixed to it perpendicularly [*See the Plate, Fig. 4.*] the air rushes in from the hollow under the bottom plate, and blows the fire. Where this is used, the shutter serves only to close the fire at nights. The more forward you can make your fire on the

\* The shutter is slid up and down in this manner, only in those fire-places which are so made as that the distance between the top of the arched opening, and the bottom plate, is the same as the distance between it and the top plate. Where the arch is higher, as it is in the draught annexed, (which is agreeable to the last improvements) the shutter is set by, and applied occasionally ; because if it were made deep enough to close the whole opening when slid down, it would hide part of it when up.



Fig. 3

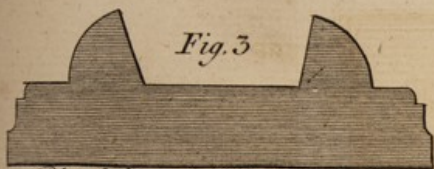
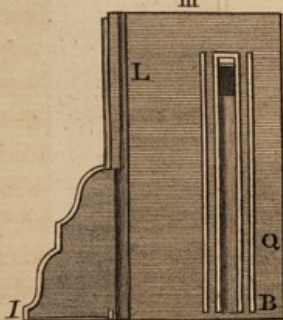


Plate & Joynr of the proper Size

vii



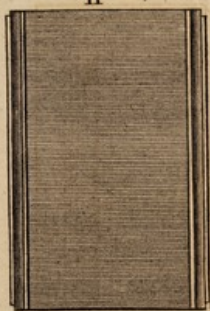
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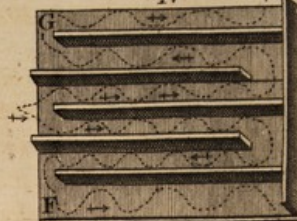
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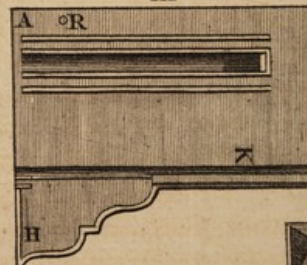
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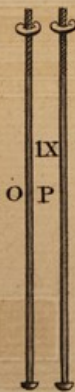
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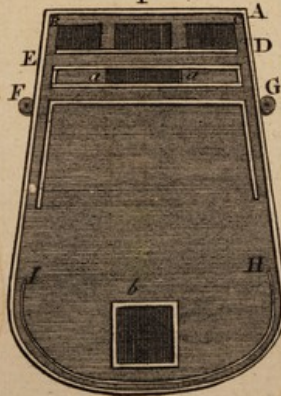
Fig. 2



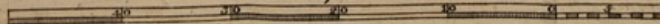
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P

i



A Scale of Inches









the hearth-plate, not to be incommoded by the smoke, the sooner and more will the room be warmed. At night, when you go 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 rekindling. 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 checked 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 extreme cold weather it often is)



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 draught, and let your fire burn with the moderation you desire: And at the same time both the fire-place and the room will be the warmer, because less cold air will enter and pass through them.---And if the chimney should happen to take fire, (which indeed there is very little danger of, if the preceding direction be observed in making fires, and it be well swept once a year; for, much less wood being burnt, less soot is proportionably made; and the fuel being soon blown into flame by the shutter (or the trap-door bellows) there is consequently less smoke from the fuel to make soot; then, though the funnel should be foul, yet the sparks have such a crooked up and down round about way to go, that they are out before they get at it. I say, if ever it should be on fire, a turn of the register shuts all close, and prevents any air going into the chimney, and so the fire may easily be stifled and mastered.

*The Advantages of this FIRE-PLACE.*

Its advantages above the common fire-places are,

1. That your whole room is equally warmed, so that people need not crowd so close round the fire, but may sit near the window, and have the benefit of the light for reading, writing, needle-work, &c. They may sit with comfort in any part of the room, which is a very considerable advantage in a large family, where  
there



there must often be two fires kept, because all cannot conveniently come at one.

2. If you sit near the fire, you have not that cold draught of uncomfortable air nipping your back and heels, as when 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, pleurifies, 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 *Pensylvania*, that very few children of the *Germans* died of that distemper in proportion to those of the *English*; which was ascribed,

\* My Lord *Molesworth*, in his account of *Denmark*, says, "That few  
" or none of the people there are troubled with coughs, catarrhs, con-  
" sumptions, or such like diseases of the lungs; so that in the midst of win-  
" ter in the churches, which are very much frequented, there is no noise  
" to interrupt the attention due to the preacher. I am persuaded (says  
" he) their *warm stoves* contribute to their freedom from these kind of  
" maladies." p. 91.



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.

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, crowded 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,

\* 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  
their



which is a considerable advantage where wood is dear.

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.

their former fires; some (according to the different circumstances of their rooms and chimnies) having been used to make very large, others 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.



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.

14. And lastly, the fire is so secured at night, that not one spark can fly out into the room to do damage.

With all these conveniencies, you do not lose the pleasing sight nor use of the fire, as in the *Dutch* stoves, but may boil the tea-kettle, warm the flat-irons, heat heaters, keep warm a dish of victuals, by setting it on the top, &c.

#### OBJECTIONS *answered.*

There are some objections commonly made by people that are unacquainted with these fire-places, which it may not be amiss to endeavour to remove, as they arise from prejudices which might otherwise obstruct, in some degree, the general use of this beneficial machine. We frequently hear it said, *They are of the nature of Dutch stoves; stoves have an unpleasant smell; stoves are unwholesome; and, warm rooms make people tender, and apt to catch cold.*---As to the first, that they are of the nature of *Dutch* stoves, the description of those stoves, in the beginning of this paper, compared with that of these machines, shows that there is a most material difference, and that these have vastly the advantage,  
if



if it were only in the single article of the admission and circulation of the fresh air. But it must be allowed there may have been some cause to complain of the offensive smell of iron stoves. This smell, however, never proceeded from the iron itself, which, in its nature, whether hot or cold, is one of the sweetest of metals, but from the general uncleanly manner of using those stoves. If they are kept clean, they are as sweet as an 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 unmanly 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



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 warmed rooms make people tender, and apt to catch cold, is a mistake as great as it is (among the English) general.*



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 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 asser-

tion,



tion, 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 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

\* Mr. Boyle, in his experiments and observations upon cold, *Shaw's Abridgement*, Vol. I. p. 684, says, "It is 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 being told by a person of unquestionable credit, that it was a common 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 tradition is likewise abundantly confirmed by *Olearius*."—"It is 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."



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 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:

T t

Make



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) 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



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 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.

\* See pag. 302, where the trap-door is described that ought to be in this closing.



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 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 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



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 through the passage under the bottom plate, and up through the air-box, by which means it will not come cold to your 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 carelessly 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 plaistered and white-washed, and the hearth reddened, the whole will make a pretty appearance.

Before



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.

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*Soon after the foregoing piece was published, some persons in England, in imitation of Mr. Franklin's invention, made what they call Pennsylvania 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.*

LETTER



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LETTER XXIV.

FROM

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

TO

Dr. L——, at *Charles Town, South Carolina*.

S I R,

*Philadelphía, 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 in *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

\* See p. 162, for the paper here mentioned.

well



well as myself, without success. At 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 thro' *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 *Havannah*, 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!



ject ! But he was, as you observe, a very singular character. I was sorry the tubes did not get to the *Havannah* 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 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. *Kinnerfley* 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 proposed to make, the reasons for making them, and the observations



observations that arose upon them, from which minutes my letters were afterwards drawn. 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. 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. Rending bodies it passes through. 9. Destroying animals. 10. Melting metals. 11. Firing inflammable substances. 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 electrified itself, either positively or negatively) I should perceive and determine it by the separation of the balls, and by finding whether they were positive or negative; but no alteration was made at all, nor could I perceive that the steam was itself electrified, though I have still some suspicion that the steam was not fully examined, and I think the experiment should be repeated. Whether the first state of electrified clouds is positive or negative, if I could find the cause of that, I should be at no loss about the other, for either is easily deduced from the other, as one state is easily produced by the other. A strongly positive cloud may drive out of a neighbouring cloud much of its natural quantity of the electric fluid, and, passing by it, leave it in a negative state. In the same way, a strongly negative cloud may occasion a neighbouring cloud to draw into itself from others, an additional quantity, and, passing by it, leave it in a positive state. How these effects may be produced, you will easily conceive, on perusing and considering the experiments in the enclosed paper: And from them too it appears probable, that every change from positive to negative, and from negative to positive, that, during a thunder gust, we see in the cork-balls annexed to the apparatus, is not owing to the presence of clouds in the same state, but often to the absence of positive or negative clouds, that, having just passed, leave the rod in the opposite state.

The



The knocking down of the six men was performed with two of my large jarrs not fully charged. I laid one end of 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



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

Mr. F. has since thought, that, possibly, the mutual repulsion of the inner opposite sides of the electrified 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.

readily



readily conceive that others may possess it : They think of inventions as of miracles ; there might be such formerly, but they are ceased. With these, every one who offers a new invention is deemed 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 inventor of any thing. They are confirmed, too, in these sentiments, by the 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 disgraced you less ; for men have not so contemptible an idea of him that robs for gold on the highway, as of him  
that



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, 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.

LETTER



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LETTER XXV.

FROM

R. J. *Esq*; of *London*,

TO

BENJ. FRANKLIN, *Esq*; at *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 quantity of subsistence,

See pag. 205.

X x

whether



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



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 in-



sensibly, 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 encreasing 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*, and 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*, where their expences are more  
confined



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 mere 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 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.



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 surprising; both the rent and the value of the inheritance



ritance of land depend on them greatly more than on nature, and 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, tho' 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 public conduct, founded, enlarged,  
and



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 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 enterprising



surprising manners, and restless dispositions of the inhabitants of *Canada* have enabled a handful of men to harass 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, than 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 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 public 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* 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



livings are seldom given without consideration, even by sincere Christians, and for consideration not seldom to very unworthy persons. The rudeness of antient 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 disunited, 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 thro' disunion, than see them under a general administration less equitable than that concerted at *Albany*.

I take it, the inhabitants of *Pensylvania* 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 antient 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



servable that none of the *English* colonies became any way considerable, till the necessary manners were born and grew up in the country, 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*.

S I R,

*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



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 entertained, but not yet reduced into any form.



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 more

sub in quid hinc quod natus quidam  
sensibile



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,



proportion, keeps the parts of the flesh 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 has 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,



fossils, as sulphur, sea-coal, &c. contain a great deal of solid fire: That some bodies are almost wholly solid fire; and that, 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 nearly 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 through their substance to the air. Hence what is called *warmth* in wool, and its preference, on that account, to linen; 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,



feathers, and the filk by which the filk-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;



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 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



## L E T T E R XXVII.

F R O M

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

T O

PETER COLLINSON, *Esq.* at London.

S I R,

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



their time in things that were merely *difficiles nugæ*, incapable of any useful application. He answered, that many of the arithmetical or mathematical questions, publicly proposed and answered in *England*, were equally trifling and useless. Perhaps the considering and answering such questions, I replied, may not be altogether useless, if it produces by practice an habitual readiness and exactness in mathematical disquisitions, which readiness may, on many occasions, be of real use. In the same way, says he, may the making of these squares be of use. I then confessed to him, that in my younger days, having once some leisure, (which I still think I might have employed more usefully) I had amused myself in making these kind of magic squares, and, at length, had acquired such a knack at it, that I could fill the cells of any magic square of reasonable size, with a series of numbers as fast as I could write them, disposed in such a manner as that the sums of every row, horizontal, perpendicular, or diagonal, should be equal; but not being satisfied with these, which I looked on as common and easy things, I had imposed on myself more difficult tasks, and succeeded in making other magic squares, with a variety of properties, and much more curious. He then shewed me several in the same book, of an uncommon and more curious kind; but as I thought none of them equal to some I remembered to have made, he desired me to let him see them; and accordingly, the next time I visited him, I carried him a square of 8, which I found among my old papers, and which



which I will now give you, with an account of its properties. (See Plate IV.)

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



# 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 | 211 | 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  |







curious ones, which, at some other time, I will explain to you.

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 thro' 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  
"astonishing 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 not, however, end with squares, but composed also a magic 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 make 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.

*S I R,*

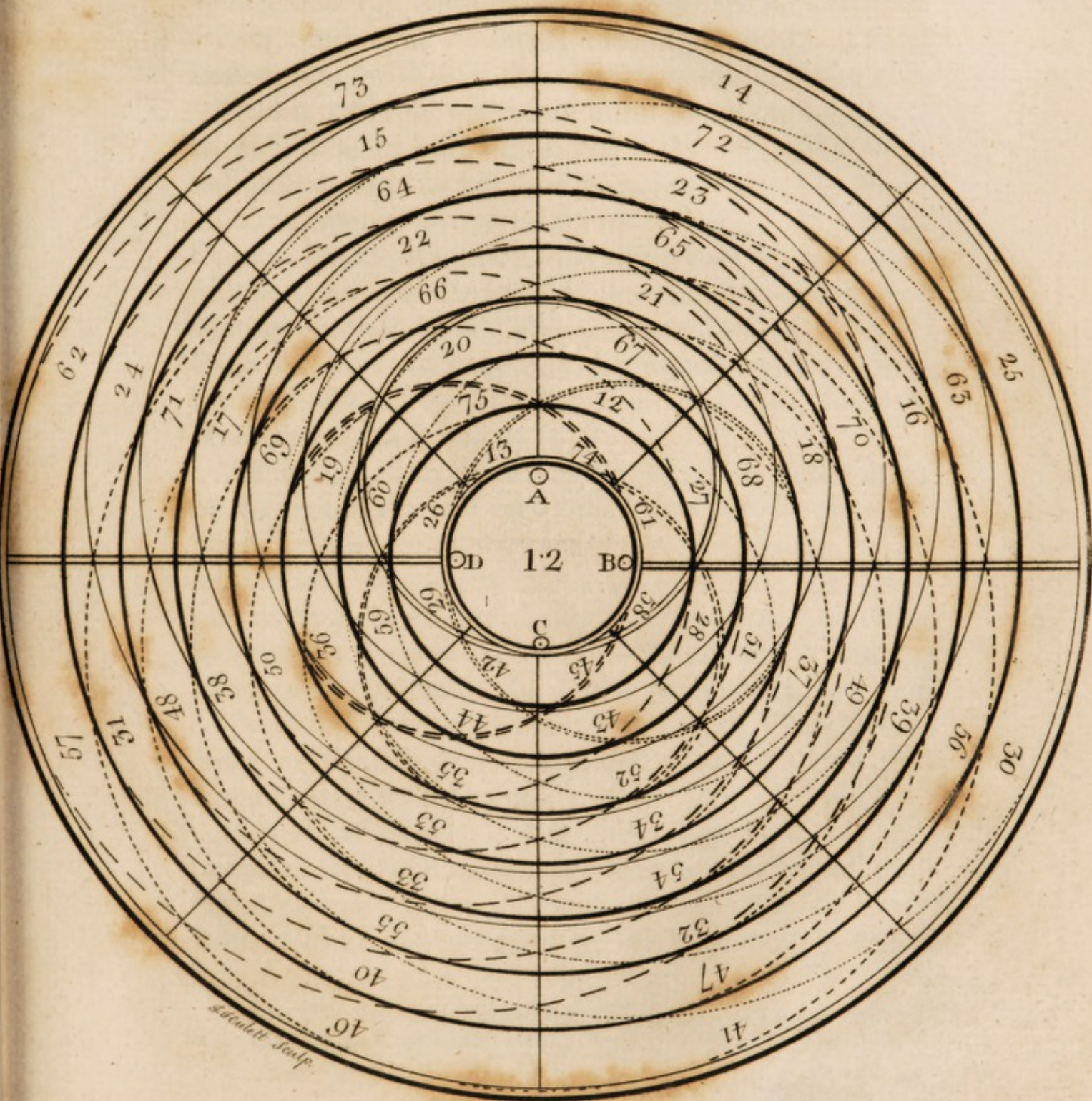
**I** AM glad the perusal of the magical squares afforded you any amusement. I now send you the magical circle. (*See Plate V.*)

Its properties, besides those mentioned in my former, are these.

Half the number in any radial row, added with half the central number, make 180, equal to the number of degrees in a semi-circle.

Also





*Scutell Sculp.*







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 coloured 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 the 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

\* In the plate they are distinguished by dashed or dotted lines, as different as the engraver could well make them.



to break the required number 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. *Tasler*, 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



vale below us, a small whirlwind beginning in the road, and shewing itself by the dust it raised and contained. It appeared in 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 no 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 whirl-wind, 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 passing whirl continue entire, after leaving the trunks and bodies of  
large

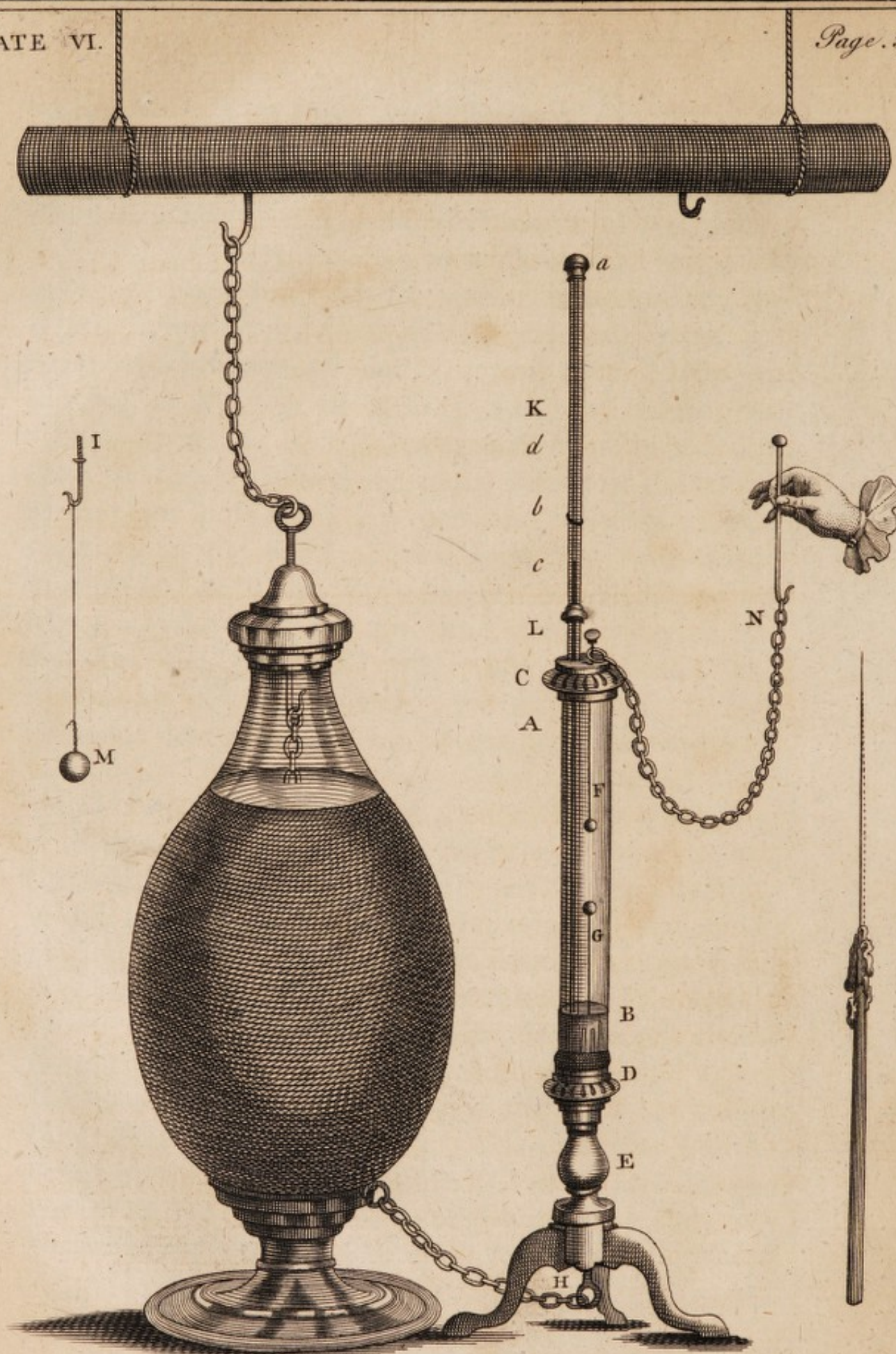


large trees which it had enveloped, I no longer wondered that my whip had no effect on it in its smaller state. I accompanied it about three quarters of a mile, till some limbs of dead trees, broken off by the whirl, flying about, and falling near me, made me more apprehensive of danger; and then I stopped, 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, tho' 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











till we had gone near three miles from the place where we first saw the whirlwind begin. Upon my asking Col. *Taske* 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.

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L E T T E R    X X X.

T O

JOHN PRINGLE, M. D. and F. R. S.

S I R,

*Craven-street, Dec. 21, 1757.*

I N 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 electrified, which I did



did for them at their request. My method was, to place the patient first in a 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 fix-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



rary advantage might arise from the exercise in the 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,*

S I R,

*Your most obedient Servant,*

B. F.



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## LETTER XXXI.

To the same.

S I R,

Craven-street, Jan. 6, 1758.

I Return to 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*.



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## 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

B b b 2

the



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 thro' 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 through 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 side of the camel, or carriage, but in the free air; whereby, as the cloths gradually grow dryer, the water contained



tained in the flasks is made cool. They have likewise a kind of earthen pots, unglazed, 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,



was, that my body never grew so hot as the air that surrounded it, or the inanimate bodies immersed 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 sun-shine, 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 honey, and a considerable portion 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

\* *Pensylvania* is 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.



the alledged 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 vapour as it can well contain, receives no more, and the evaporation is therefore checked 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,



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.

LETTER



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## LETTER XXXIII.

T O

J. B. Esq; *at Boston, 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 funnel being stopt by the rest of the plate, the passage of warm air out of the room up the chimney, is obstructed and re-

C c c

tarded,



tarded, 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 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



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, and 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 drop from the wetted cloths. I think, too, that this property of chimneys might, by means of smoke-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 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,



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 cooled 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 *Beaconhill*, 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



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.

LETTER



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L E T T E R      XXXIV.

To Dr. H. at *London*.

S I R,

*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



other advantage in the form. If any of 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 charged 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 at 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 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 one, 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, (attached 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.

LETTER



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## LETTER XXXV.

*To Mr. P. F. in Newport.*

S I R,

*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 saltiness 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 present height, by some internal mighty force, such as we still feel some remains of, when whole continents are moved by earthquakes. In either case it may be supposed that large hollows, or valleys among hills, might be left filled with sea-water, which evaporating, and the fluid part drying away in a course of years, would leave the salt covering the bottom; and that salt 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 sea, if at any time the sea leaks into any of them, the fluid parts of the water must evaporate from that heat, and pass on through some volcano, while the salt remains, and by degrees, and continual accretion, becomes a great mass. Thus the cavern may at length be filled, and the volcano connected with it cease burning, as many it is said have done; and future miners penetrating such cavern, find what we call a salt mine. --- This is a fancy I had on visiting the salt-mines at *Northwich*, with my son. I send you a piece of the rock-salt which he brought up with him out of the mine. \* \* \* \* \*

*I am, Sir, &c.*

B. F.

LETTER



## LETTER XXXVI.

TO MR. ALEXANDER SMALL, *London.*

Dear Sir,

May 12, 1760.

A Greeable 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 before 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



which came on about seven, 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 begun 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 stopped 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; the water next to that first water  
moves



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.  
LETTER



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## LETTER XXXVII.

From Mr. KINNERSLEY.

S I R,

*Philadelphia, March 12, 1761.*

**H**AVING 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 electrified, threw my hat to an unelectrified 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 electrified sufficiently to attract the thread.

I then suspended, by silk, a broad plate of metal, and electrified 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 hypothesis,



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 *Abbé 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 balanced, like the needle of a compass, on a pin, about six inches long, fixed in the center of an electric stand. Then, electrifying 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 electrified 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 electrified 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.

E e e

When



When electrified 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 electrified bodies, I begin to be somewhat doubtful of. I think all the phenomena on which it is founded, may be well enough accounted for without it. Will not cork balls, electrified negatively, separate as far as when electrified 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 electrified 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 plentifully, for, being a bad conductor, it seems loth to part with it, but yet some will evidently be collected.



lected. 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 electrified 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, and so clear that not a cloud could be seen, and found the twine each time in a small degree electrified positively. The kite had three metalline points fixed to



it; one on the top, and one on each side. That the twine was electrified, 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 electrified positively, was proved, by applying to it the wire of a charged bottle, which caused the balls to separate farther, 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 electrified 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



fenfible of any alteration in the ftate 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. See plate VI.

A B is a glafs tube, about eleven inches long, and one inch diameter in the bore. It has a brafs feril, cemented on each end, with a top and bottom part, C and D, to be fcrewed on, air-tight, and taken off at pleasure. In the center of the bottom part D, is a male fcrew, which goes into a brafs nut, in the mahogany pedeftal E. The wires F and G, are for the electric fire to pafs through, darting from one to the other. The wire G extends through the pedeftal to H, and may be raifed and lowered by means of a male fcrew on it. The wire F may be taken out, and the hook I be fcrewed into its place. K is a glafs tube, with a fmall bore, open at both ends, cemented in the brafs tube L, which fcrews into the top part C. The lower end of the tube K is immerfed in water, coloured with cochineal, at the bottom of the tube A B. (I ufed, at firft, coloured fpirits of wine, but in one experiment I made, it took fire.) On the top of the tube K is cemented, for ornament, a brafs feril, with a head fcrewed on it, which has a fmall air-hole thro' its fide, at *a*. The wire *b*, is a fmall round fpring, that embraces the tube K, fo as to ftay wherever it is placed. The weight M is to keep ftrait whatever may be fufpended in the tube A B, on the hook I. Air  
muft



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 electrified 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 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 above 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



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 woollen 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 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



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 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



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 to 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 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 electric 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 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



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 chimnies to which it was fixed, (tho' 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 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,



kind, 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.

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## LETTER XXXVIII.

T O

*Mr. KINNERSLEY, in Answer to the foregoing.*

*S I R,*

*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 from electrified 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



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 electrified. 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 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,



lows, 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 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 for-



I formerly had an opinion, that a *Leyden* bottle, charged and then sealed hermetically, might retain its electricity for ever; but having afterwards some suspicion that possibly that subtil fluid might, by flow imperceptible degrees, soak through the glass, and in time escape, I requested some of my friends, who had conveniencies 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 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



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 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."

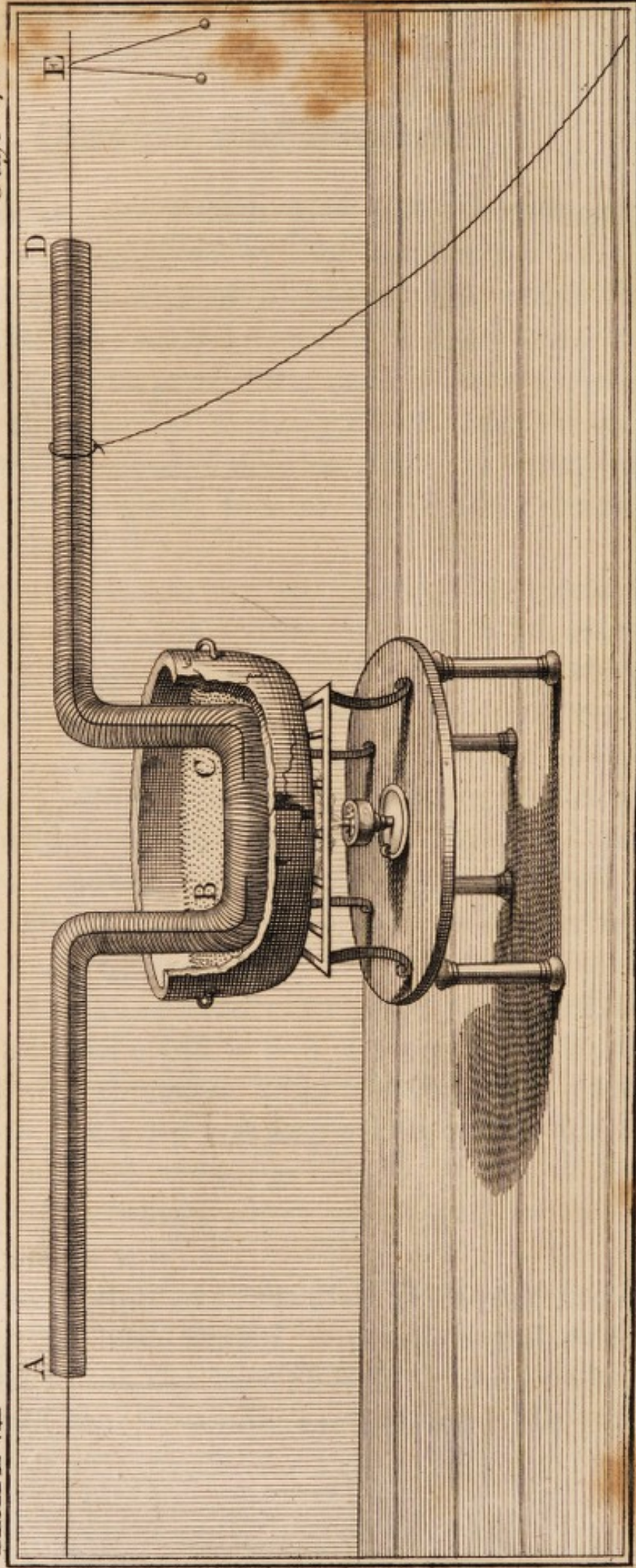
By this last experiment of Mr. *Canton's*, it appears, that though by a moderate heat, thin glass becomes, in some degree, a conductor of electricity, yet, when of the thickness of a common pane, it is not, though in a state near melting, so good a conductor as to pass the shock of a discharged bottle. 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 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











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 VII.*) 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 looked upon and mentioned the equal repulsion in cases of positive and of negative electricity, as a phenomenon 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, 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, accumu-



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 touched 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 pushed (or at least appear to be pushed) off the  
table.



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 electrified positively, repels the points; when negatively, the air opposite the points being also, by their means, electrified negatively, attraction takes place between the electricity in the air behind the heads of the pins, and the negative pins, and so they are, in this case, drawn in the same direction that in the other they were driven.---You see I am willing to meet you half way, a complaisance I have not met with in our brother *Nollet*, or any other hypothesis-maker, and therefore may value myself a little upon it, especially as they say I have some ability in defending even the wrong side of a question, when I think fit to take it in hand.

What



What you give as an established law of the electric fluid, "That quantities of different densities mutually attract each other, in order to restore the equilibrium," is, I think, not well founded, or else not well expressed. Two large cork balls, suspended by silk strings, and both well and equally electrified, separate to a great distance. By bringing into contact with one of them, another ball of the same size, suspended likewise by silk, you will take from it half its electricity. It will then, indeed, hang at a less distance 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 densities. And, supposing in nature, a mutual attraction between two parcels of any kind of matter, it would be strange if this attraction should subsist strongly 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



(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 increased, 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 mixed 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 of restoring electricity to a negatively electrified 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 electrified the air negatively. The difference was easily discovered by his cork balls, as he passed out of one room into another.---*Pere Beccaria*,  
too,



too, has a pretty experiment, which shews that air may be electrified. Suspending a pair of small light balls, by flaxen threads, to the end of his prime-conductor, he turns his globe some time, electrifying 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 electrified 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 electrified *plus* or *minus*, as well as when that is in its natural state, and they are electrified *plus* or *minus* themselves. 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 electrifying 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 electrified 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 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, seems 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,



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 general, 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 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 finge, 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



electric fluid *beats 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 compleat a description of its situation, form, and substance,



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 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



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 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



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 electrified cloud, knobs draw them nearer. An experiment which I believe I have shewn you, of a cotton fleece hanging from an electrified body, shews this clearly when a point or 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 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



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.

I i i

LETTER



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## LETTER XXXIX.

Accounts from *Carolina* (mentioned in the foregoing Letter) of the Effects of Lightning on two of the Rods commonly affixed 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 fixed 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 fixed to; but the brass wire, all down till it came opposite to the top of the gun-barrel, was destroyed \*. There the lightning made a hole through

\* 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.



the wall or back of the chimney, to get to the gun-barrel \*, down which it seems to have passed, 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 remained 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."-----

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## LETTER XL.

Mr. *William Mainé'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, composed of several joints, annexed by hooks turned at the end at each joint, and the whole fixed to the chimney of my house by iron staples. The points were elevated (*a*) six or seven inches above the top of the chimney; and the lower joint sunk three feet in the earth, in a perpendicular direction.

Thus stood the points on Tuesday last about five in the evening, when the lightning broke with a violent explosion on the chimney, cut the rod square off just under the nutt, and, I am persuaded, melted the points, nutt, and top of the rod, entirely up; as after the most diligent search nothing of either was found (*b*), and the top of the remaining rod was cased over with a congealed folder. The lightning ran down the rod, starting almost all the staples (*c*), and unhooking the joints, without affecting the rod (*d*), except on the inside of each hook where the joints were coupled, the surface of which was melted (*e*), and left as cased over with folder.---No part of the chimney was damaged, (*f*), only at the foundation (*g*), where it was shattered almost quite round, and several bricks were torn out (*h*). Considerable cavities were made in the earth quite round the foundation, but most within eight or nine inches of the rod. It also shattered the bottom weather-board (*i*), at one corner of the house, and made a large hole in the earth by the corner post. On the other side of the chimney it ploughed up several furrows in the earth, some yards in length. It ran down the inside of the chimney (*k*), carrying only foot with it; and filled the whole house with its flash (*l*),  
smoke,



Smoke, and dust. It tore up the earth 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 stunned 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.



## 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 134, 169.*) 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



conductor; and that this effect is strongest, when as much 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 electrified 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 170.) Where the quantity of the electric fluid passing is too great for the conductor through 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



smaller than the rod; therefore the current through the metal being confined 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 chimnies have been seen 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



body of earth so moist as to receive readily 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, and the weather-board, (*i*) which being near the earth are generally moist, and, in exploding that moisture, shattered them. (*See page 423.*) Part went through or under the foundation, and got under the hearth, blowing up great part of the bricks (*m*) (*n*), and producing 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, though 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 ripped 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 chimnies, 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 neither wood, brick, stone nor plaister, it is apt to rend them in its passage. It would not easily pass through the air from a cloud to a building, were it not for the aid afforded it in its passage by intervening fragments of clouds below the main body, or by the falling rain.

It is said that *the house was filled with its flash* (1). Expressions like this are common in accounts of the effects of lightning, from which we are apt to understand that the lightning filled the house. Our language indeed seems to want a word to express the *light* of lightning as distinct from the lightning itself. When a tree on a hill is struck by it, the lightning of that stroke exists only in a narrow vein between the cloud and tree, but its light fills a vast space many miles round; and people at the greatest distance from it are apt to say, "the lightning came into our rooms through our windows." As it is in itself extremely bright, it cannot, when so near as to strike a house, fail illuminating highly every room in it through the windows; and this I suppose to have been the case at Mr. *Maine's*; and that, except in and near the hearth, from the causes above-mentioned, it was not in any other part of the house; *the flash* meaning no more than *the light* of the lightning.---It is for want of considering this difference, that people suppose there is a kind of lightning not attended with thunder. In fact there is probably a loud explosion accompanying every  
every



every flash of lightning, and at the same instant;--- but as sound travels slower than light, we often hear the sound 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.

## L E T T E R XLI.

*Saturday, July 3, 1762.*

**T**O 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  $2\frac{1}{2}$  inches long, the

K k k 2

bore



bore about  $\frac{1}{2}$  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



the quantity of electric fluid, by discharging through it at once five rows of my bottles. On opening the tube I found that some of the 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, SIR,

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



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  
brims.



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 three are twenty-three different sizes, differing from each other a quarter of an inch in diameter.---

To



To make 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 three 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 by grinding somewhat off the brim, which will afterwards require polishing, and thus encrease the trouble.

The



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 thro' 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 25lb 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 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 glasses. 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 shews 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 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



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 sponge 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.

L 112 LETTER

LETTER



## 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 phaenomenon I never observed but twice, viz. on the 31st of *January*, 1760, and the 3d of *March*, 1762.

I am, Sir, &c.

LETTER



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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



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 subtracting 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 subtle fluid, the conductor of sound, which moves at different times in different



different directions over the surface of the earth, and 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.

LETTER



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## LETTER XLV.

To Dr. P. in *London*.

S I R,

*Philadelphia, Dec. 1, 1762.*

**D**URING our passage to Madeira, the weather being warm, and the cabin 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, flung in wire, and suspended to the ceiling of the cabin, and a little wire hoop for the wick, furnished 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 though 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 during the whole evening.



ing. The lamp was kept burning as a watch light all night, till the oil was spent, and the water only remained. 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 resumed 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 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

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attempt



attempt to explain it; but their explanations have been different, and to me not very intelligible.--- Others more deeply skilled 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 explained 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.

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## 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  
wit



with me, that 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.

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## 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 cannot do better than to erect a mast not

M m m 2.

far



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 public 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 corked; or in large tin canisters, with small covers shutting close by means of oiled 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, though it comes all the way by sea in the damp hold of a ship. And by this method, grain, meal, &c. if well dried before it is put up, may be kept for ages sound and good.

There



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 extremely close. It may be applied to the wood with common paste, made with boiling water thickened 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 boiled. 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 hooped 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, is but about 18 pence sterling a pound, and is so extremely thin, that I imagine a pound of it would line three or four powder barrels.

*I am, &c.*

B. F.

LETTER



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## LETTER XLVIII.

To Miss S——n, at *Wanstead*.

*Craven-street, May 17, 1760.*

I Send my good girl the books I mentioned to her last night. I beg her to accept of 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, unembarrassed 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 conversation, 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



diately when you meet with a word 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



## LETTER XLIX.

To the same.

*Craven-street, June 11, 1760.*

**T**IS 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 pin-hole would 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



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 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 Linnaeus, 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 hatched, 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 increased. The season in which the fly laid its eggs, Linnaeus knew to be about a fortnight (I think) in the month of May, and at no other time in the year. He therefore advised, that some days before that 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 of 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 E T T E R 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 some time been much engaged in business.

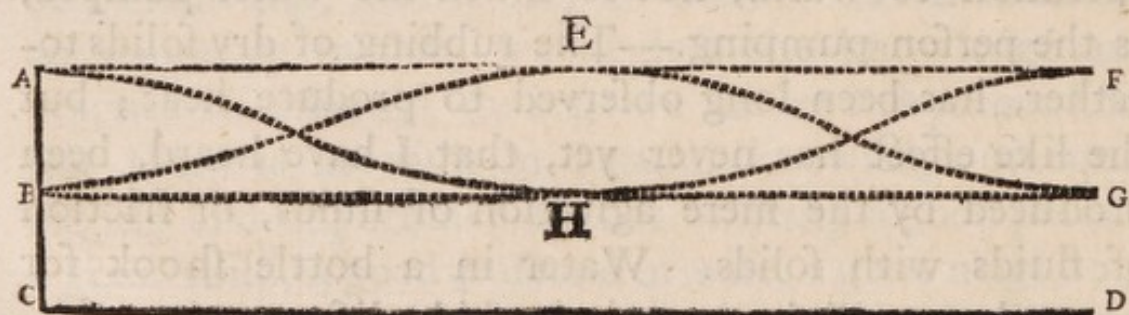
Your first question, *What is the reason the water at this place, though cold at the spring, becomes warm by pumping?* It will be most prudent in me to forbear attempting to answer, till, by a more circumstantial account, you assure me of the fact. I own I should expect that operation to warm, not so 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 pricked 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



quite full at that place up to 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



That this is not mere theory, but conformable to fact, we know by our long rivers in *America*. The *Delaware*, on which *Philadelphia* stands, is in this particular similar to the canal I have supposed of one wave: For when it is high water at the *Capes* or mouth of the river, it is also high water at *Philadelphia*, which stands about 140 miles from the sea; and there is at the same time a low water in the middle between the two high waters; where, when it comes to be high water, it is at the same time low water at the *Capes* and at *Philadelphia*. And the longer rivers have, some a wave and half, some two, three, or four waves, according to their length.---In the shorter rivers of this island, one may see the same thing in part: for instance, it is high water at *Gravesend* an hour before it is high water at *London Bridge*; and 20 miles below *Gravesend* an hour before it is high water at *Gravesend*. Therefore at the time of high water at *Gravesend* the top of the wave is there, and the water is then not so high by some feet where the top of the wave was an hour before, or where it will be an hour after, as it is just then at *Gravesend*.

Now we are not to suppose, that because the swell or top of the wave runs at the rate of 20 miles an hour, that therefore the current or water itself of which the wave is composed, runs at that rate. Far from it. To conceive this motion of a wave, make a small experiment or two. Fasten one end of a cord in a window near the top of a house, and let the  
other



other end come down to the ground; take this end in 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 the 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,



gravity, or tendency to run from a higher 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 saltier 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 a quantity of water  
equal



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 finish my letter in the mode, I should yet add something that means nothing, and is *merely*  
O o o civil



civil and polite.---But being naturally awkward 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.*

**S**UPPOSING 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



immediate 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 passed by. I have not expressed 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 through 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.

*Craven-street, 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 with it. It is true that distilled sea water will not be salt, but there are other disagreeable



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 filtering 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



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 through the doubly difficult task of reading so big a book, on an abstruse subject, and in a foreign language.

In answer to your question concerning the *Leyden* phial.---The hand that holds the bottle receives and conducts away the electric fluid that is driven out of  
the



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.

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## 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 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 facheverell or blower



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 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



conversation the least deviation from it, I should earnestly recommend it to your observation.

*I am, &c.*

B. FRANKLIN.

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## LETTER LV.

To Mr. O. N.

*Dear SIR,*

I Cannot be of opinion with you that it is too late in life for you to learn to swim. The river near the bottom of your garden affords 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, or of supporting yourself in the water till a boat could come to take you up.

P p p

I do



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 order 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



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 satisfied 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 perusing them at your leisure, you may possibly imprint them so in your memory as on occasion to be of some use to you.



1. That though the legs, arms, and head, of a human body, being solid parts, are specifically something heavier than fresh water, yet the trunk, particularly the upper part, from its hollowness, is so much lighter than water, as that the whole of the body taken together is too light to sink wholly under water, but some part will remain above, until the lungs become filled with water, which happens from drawing water into them instead of air, when a person in the fright attempts breathing while the mouth and nostrils are under water.

2. That the legs and arms are specifically lighter than salt-water, and will be supported by it, so that a human body would not sink in salt-water, though the lungs were filled as above, but from the greater specific gravity of the head.

3. That therefore a person throwing himself on his back in salt-water, and extending his arms, may easily lie so as to keep his mouth and nostrils free for breathing; and by a small motion of his hands may prevent turning, if he should perceive any tendency to it.

4. That in fresh water, if a man throws himself on his back, near the surface, he cannot long continue in that situation but by proper action of his hands on the water. If he uses no such action, the legs and lower part of the body 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.

4. But



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 cloaths, 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



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



## LETTER LVI.

To Miss S——n, at *Wanstead*.

My dear Friend,

Sept. 20, 1761.

IT is, as you observed in our last conversation, a very general opinion, that *all rivers run into the sea*, or depofite their waters there. It is a kind of audacity to call fuch general opinions in queftion, and may fubject one to cenfure. But we muft hazard fomething in what we think the caufe of truth: And if we propofe our objections modeftly, we fhall, though miftaken, deferve a cenfure lefs fevere, than when we are both miftaken and infolent.

That fome rivers run into the fea is beyond a doubt: Such, for inftance, are the *Amazones*, and I think the *Oronoko* and the *Missiffipi*. The proof is, that their waters are freft quite to the fea, and out to fome diftance from the land. Our queftion is, whether the freft waters of thofe rivers whofe beds are filled with falt-water for a confiderable diftance up from the fea (as the *Thames*, the *Delaware*, and the rivers that communicate with *Chesapeak-bay* in *Virginia*) do ever arrive at the fea? And as I fufpect  
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, though 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 receive, but by having their course  
stopped



stopped 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 *Chesapeake* in *Virginia*, mentioned

Q q q

above;



above; you will see, communicating with it by their mouths, the great rivers *Sasquebanah*, *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 *Sasquebanah* 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. Try to fire paper with a burning glass. If it is white, you will not easily burn it;---but if  
you



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 warmed 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 cloaths are not so fit to wear in a hot sunny climate or season, as white ones; because in such cloaths the body is more heated by the sun when we walk abroad,

LETTER Q q q 2 and



and we 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, though it would if placed *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 through the night, and thereby preserve the fruit from frosts, or forward its growth?---with fundry other particulars of less or greater importance, that will occur from time to time to attentive minds?---

*I am,*

*Yours affectionately,*

B. FRANKLIN.

LETTER



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## 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;



felves; 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 agreeable, 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 paradoxical, I must explain my meaning. In common acceptation, indeed, only an agreeable *succession* of sounds is called *melody*, and only the *co-existence* of agreeable 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 notes which have a stress laid on them in singing the  
tune,



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 founding them separately, as by founding them together; their disagreement is also as easily, I believe I may say more easily and better distinguished when founded separately; for when founded together, though 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 figure of the window will still remain in the eye, and so distinct  
that



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 against reversed. This I know not how to account for.---Nor for the following; that after looking long through 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 through 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 antient 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 began. To avoid actual discord, it was therefore necessary that the succeeding emphatic note should be a chord with the preceding,

as



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, though 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 antient, 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 musick 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

\* The celebrated *Rousseau* in his *Dictionnaire de Musique*, printed 1768, appears to have similar sentiments of our modern *Harmony*, viz.

“ M. Rameau prétend que les dessus d’une certaine simplicité suggèrent naturellement leur basse, & qu’un homme ayant l’oreille juste et non exercée, entonnera naturellement cette basse. C’est-là un préjugé de musicien, démenti



old tunes do not 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.

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 unison.

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



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## LETTER LVIII.

*To Mr. P. F. Newport, New England.*

*Dear Brother,*

\* \* \* \* “ I like your ballad, and I 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 composed for it. I think too, that if you had given it to some country girl in the heart of the *Massachusetts*, who has never heard any other than psalm tunes, or *Chevy Chase*, 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,

R r r 2

and



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 finger 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 musick 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.

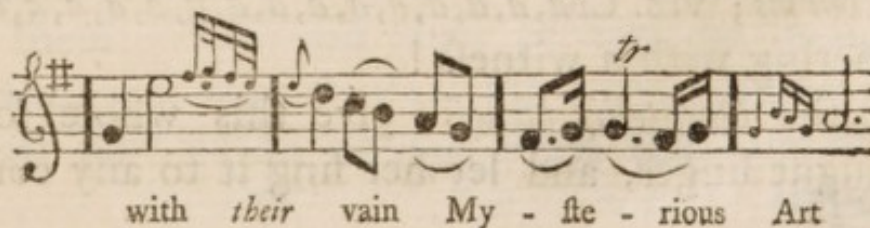
You, in the spirit of some antient legislators, would influence the manners of your country by the united powers of poetry and music. By what I can learn of *their* songs, the musick was simple, conformed itself to the usual pronunciation of words, as to measure, cadence, or emphasis, &c. never disguised 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



for this, and therefore I must endeavour 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*.



An



And on the word *from*, and the wrong syllable *like*.



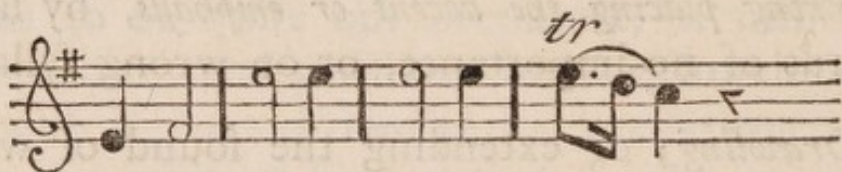
God-like Wisdom *from* a — above.

For the *Drawling*, see the last syllable of the word *wounded*.



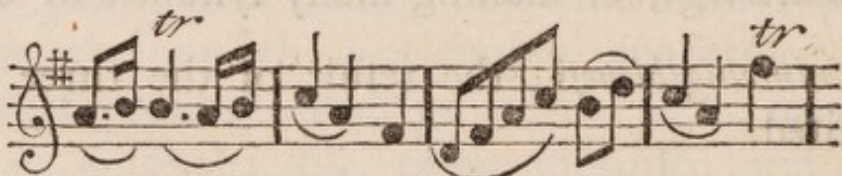
Nor can heal the wounded Heart

And in the syllable *wis*, and the word *from*, and syllable *bove*



God-like Wisdom *from* a - bove

For the *Stuttering*, see the words *ne'er relieve*, in



Ma - gick Charms can *ne'er* re - lieve you

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 finger, and let her sing it to any company that



that have never heard it; you shall find that they will not understand three words in ten. It is therefore that at the oratorios 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 screamed by some singers.



*No magic charms can e'er re—lieve you.*

I send you inclosed the song with its music at length. Read the words without the repetitions. Observe how 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 antient song, they are of small importance



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



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## 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 extreme subtle 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 be-

S f f

tween



tween 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 the communication be through the air without any conductor, a bright light is seen between the bodies, and a sound is heard. In our small experiments we call this light and sound the electric spark and snap; but in the great operations of nature, the light is what we call *lightning*, and the sound (produced at the same time, though 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 sound, the subtle fluid passing in the substance of the conductor.

If the conductor be good and of sufficient bigness, the fluid passes through it without hurting it. If otherwise, it is damaged or destroyed.

All metals, and water, are good conductors.---Other bodies may become conductors by having some quantity of water in them, as wood, and other materials used in building, but not having much water in them, they are not good conductors, and therefore are often damaged in the operation.

Glass, wax, silk, wool, hair, feathers, and even wood, perfectly dry are non-conductors: that is, they resist instead of facilitating the passage of this subtle fluid.

When this fluid has an opportunity of passing through two conductors, one good, and sufficient, as  
of



of metal, the other not so good, it passes in the best, and will follow it in any direction.

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 charged, 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 through the house it follows the direction of these conductors, taking as many in it's way as can assist it in it's 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 it's striking any other part; and, affording it a good conveyance into the earth, will prevent it's 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



quantity of lightning that did prodigious damage both above and below it ; and probably larger rods are not necessary, though 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,



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 through a continued better conductor the wall. But where it can be had, a hammock 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 a person



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.

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## L E T T E R 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. It is amazing to me, that after the full demonstration 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 enquiry !”

ANSWER to the above.

\* \* \* It is perhaps not so extraordinary that unlearned men, such as commonly compose our church vestries,



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 mécanique de ses principaux effets, mais ce n'est point ce qu'il y a de plus important; il vaudroit bien mieux 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, & 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



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 the sound of such blessed bells would drive away those storms, and secure our buildings from the stroke of

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.—\*\*\*. *Mémoire sur les Effets du Tonnerre.*

\* Les cloches, en vertu de leur bénédiction, doivent écarter les orages & nous préserver des coups de foudre ; mais l'église permet à la prudence humaine le choix des momens où il convient d'user de ce préservatif. Je ne fais 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 bénit des cloches, on recite les oraisons suivantes :

*Benedic, Domine . . . . quotiescumque sonuerit, procul recedat virtus insidiantium, umbra phantasmatis, incurso turbinum, percussio fulminum, læsio tonitruum, calamitas tempestatum, omnisque spiritus procellarum, &c.*

*Deus, qui per beatum Moïsen, &c. . . . . procul pellentur insidiæ inimici, frager 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.*

T t t

lightning ;



lightning; and during so long a period, it has not been found by experience, that places within the reach of such blessed sounds, 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 several 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

† En 1718. M. Deslandes fit savoir à l'Académie Royale des sciences, que la nuit du 14 ou 15 d'Avril de la même année, le tonnerre étoit tombé sur vingt-quatre églises, depuis Landernau jusqu'à Saint Pol-de-Léon en Bretagne; que ces églises étoient précisément celles où l'on sonnoit, & que la foudre avoit épargné celles où l'on ne sonnoit pas: que dans celle de Gouifnon, qui fut entièrement ruinée, le tonnerre tua deux personnes de quatre qui sonnoient, &c. *Hist. de l'Ac. Roy. des Sc.* 1719.



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 the 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 extremely 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 through 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 bored 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 placed 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, 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



these balls, and all at rest; if you wet one of the balls by means of a feather dipped 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 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



---

## LETTER LXI.

To Sir *John Pringle*, Bart.

S I R,

*Craven-street, May 10, 1768.*

**Y**OU may remember that when we were travelling together in *Holland*, you remarked that the trackschuyt 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 touched 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 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



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 expressing the quantity of the difference; some supposing it was equal to a mile in six, others to a mile in three, &c. As I did not recollect to have met with any mention of this matter in our philosophical books, and conceiving that if the difference



difference should really be great, it might be an object of consideration in the many projects now on foot for digging new navigable canals in this island, I lately put my design of making the experiment in execution, in the following manner:

I provided a trough of plained boards fourteen feet long, six inches wide and six inches deep, in the clear, filled with water within half an inch of the edge, to represent a canal. I had a loose board of nearly the same length and breadth, that being put into the water might be sunk to any depth, and fixed by little wedges where I would chuse to have it stay, in order to make different depths of water, leaving the surface at the same height with regard to the sides of the trough. I had a little boat in form of a lighter or boat of burthen, six inches long, two inches and a quarter wide, and one inch and a quarter deep. When swimming, it drew one inch water. To give motion to the boat, I fixed one end of a long silk thread to its bow, just even with the water's edge, the other end passed over a well-made brass pulley, of about an inch diameter, turning freely on a small axis; and a shilling was the weight. Then placing the boat 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



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 $1\frac{1}{2}$ inch 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                      |    |  |
|          |                                 | <hr/> |       | <hr/>     |    | <hr/> |                         |    |  |
|          |                                 | 813   |       | 717       |    | 632   |                         |    |  |
|          |                                 | <hr/> |       | <hr/>     |    | <hr/> |                         |    |  |
|          | 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 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.

U u u

Whether



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.



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