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Benjamin Franklin

Complete Works

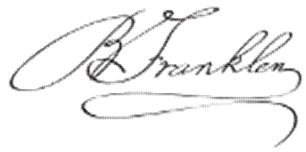
Series Thirteen

The Complete Works of

BENJAMIN FRANKLIN

(1706-1790)

Contents



Version 1

The Complete Works of

BENJAMIN FRANKLIN

By Delphi Classics, 2023

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Complete Works of Benjamin Franklin

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DELPHI CLASSICS

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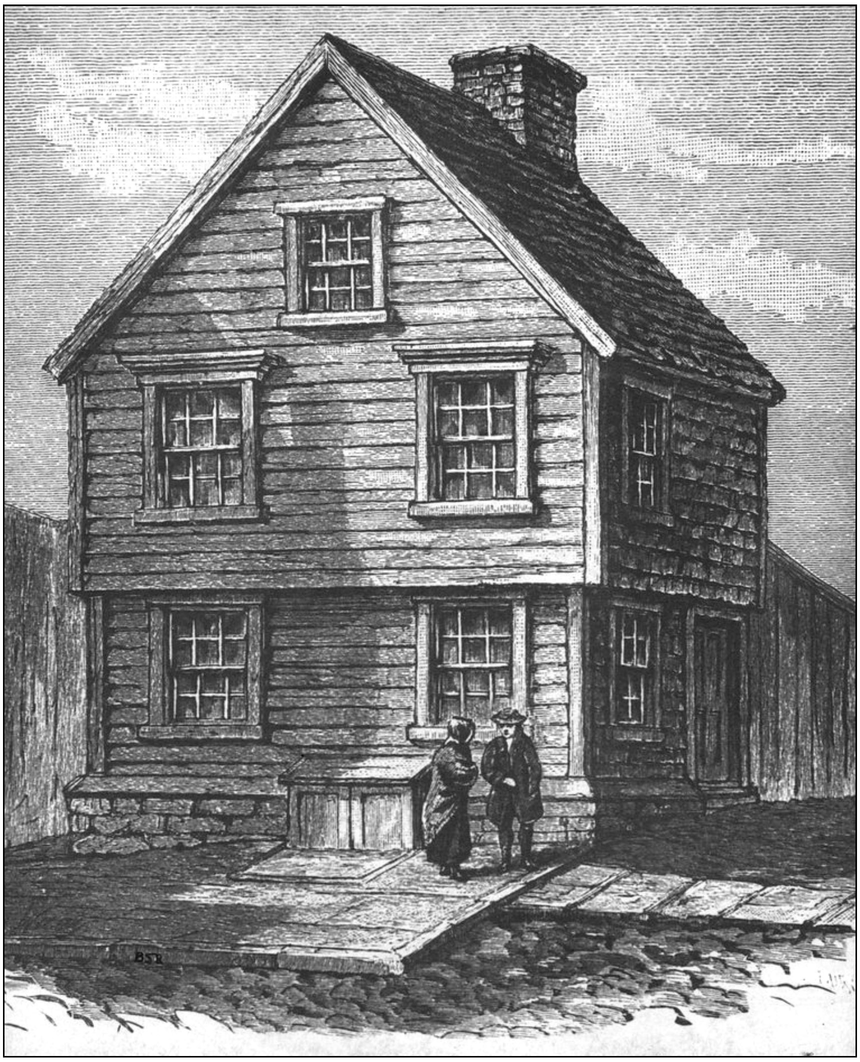
The Works

Boston, Massachusetts, 1801 — Benjamin Franklin’s birthplace

Milk Street, Boston, late nineteenth century — Franklin was born on Milk Street on 17 January 1706. His father, Josiah Franklin, was a tallow chandler from Northamptonshire, England. His mother, Abiah, was the daughter of a miller and schoolteacher.



The birthplace, Milk Street

The site of birthplace is commemorated by a bust above the second floor facade of this building.

Portrait of Benjamin Franklin by Robert Feke, c. 1746. This is widely believed to be the earliest known painting of Franklin.

Letters and Papers on Electricity

TEXT FROM LONGMAN, HURST, REES, & ORME 1806 EDITION

Benjamin Franklin had started exploring the phenomenon of electricity in 1746 when he saw some of Archibald Spencer's lectures using static electricity for illustrations. Franklin went on to argue that "vitreous" and "resinous" electricity were not different types of "electrical fluid" (as electricity was called then), but the same "fluid" under different pressures. He was the first to label them as positive and negative respectively, and he was the first to discover the principle of conservation of charge. In 1748, Franklin constructed a multiple plate capacitor, that he called an "electrical battery" by placing eleven panes of glass sandwiched between lead plates, suspended with silk cords and connected by wires.

In pursuit of more pragmatic uses for electricity, he began preparations for a practical demonstration. He proposed a dinner party where a turkey was to be killed via electric shock and roasted on an electrical spit. Having prepared several turkeys this way, he noted that "the birds kill'd in this manner eat uncommonly tender." Franklin recounted that in the process of one of these experiments, he was shocked by a pair of Leyden jars, resulting in numbness in his arms that persisted for one evening.

He also briefly investigated electrotherapy, including the use of the electric bath. This work led to the field becoming widely known. In recognition of his work with electricity, he received the Royal Society's Copley Medal in 1753, and in 1756, he became one of the few eighteenth-century Americans elected a fellow of the Society. The CGS unit of electric charge has been named after him: one franklin (Fr) is equal to one statcoulomb. Franklin went on to advise Harvard University in its acquisition of new electrical laboratory apparatus after the complete loss of its original collection in a fire that destroyed the original Harvard Hall in 1764. The collection he assembled became part of the Harvard Collection of Historical Scientific Instruments, now on public display in its noted Science Center.

'Benjamin Franklin Drawing Electricity from the Sky' by Benjamin West, c. 1816, Philadelphia Museum of Art

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No. 2. Remarks on the Abbé Nollet's Letters to Benjamin Franklin, Esq. of Philadelphia, on Electricity: by Mr. David Colden, of New York.

LETTERS AND PAPERS ON ELECTRICITY.

It may not be improper to present the reader with the following extract from the preface to the first edition of Dr. Franklin's papers on electricity, which, as we have stated in the advertisement, formed a pamphlet only.

"The following observations and experiments were not drawn up with a view to their being made public, 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 public his reflections and experiments on the subject, finished with that care and precision, of which the treatise before us shows he is alike studious and capable."

With respect to the general merit and originality of the experiments and hypothesis of Dr. Franklin, as described and explained in these letters, the following is the testimony of one of the first natural philosophers of his age — the late Dr. Priestly, in his History of Electricity.

"Nothing was ever written upon the subject of electricity which was more generally read and admired in all parts of Europe than these letters. There is hardly any European language into which they have not been translated; and, as if this were not sufficient to make them properly known, a translation of them has lately been made into Latin. It is not easy to say, whether we are most pleased with the simplicity and perspicuity with which these letters are written, the modesty with which the author proposes every hypothesis of his own, or the noble frankness with which he relates his mistakes, when they were corrected by subsequent experiments.

"Though the English have not been backward in acknowledging the great merit of this philosopher, he has had the singular good fortune to be, perhaps, even more celebrated abroad than at home; so that, to form a just idea of the great and deserved reputation of Dr. Franklin, we must read the foreign publications on the subject of electricity; in many of which the terms Franklinism, Franklinist, *and the* Franklinian system, *occur in almost every page. In consequence of this, Dr. Franklin's principles bid fair to be handed down to posterity as equally expressive of the true principles of electricity, as the Newtonian philosophy is of the true system of nature in general."*

INTRODUCTORY LETTER TO PETER COLLINSON, ESQ. F. R. S. LONDON.

Philadelphia, March 28, 1747.

Sir,

Your kind present of an electric tube, with directions for using it, has put several of us[1] on making electrical experiments, in which we have observed some particular phenomena that we look upon to be new. I shall therefore communicate them to you in my next, though possibly they may not be new to you, as among the numbers daily employed in those experiments on your side the water, it is 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 crowds to see them, I have, during some months past, had little leisure for any thing else.

I am, &c.

B. FRANKLIN.

[1] i. e. of the *Library-Company*, an institution of the author's, founded 1730. To which company the present was made.

Wonderful Effect of Points. — Positive and negative Electricity. — Electrical Kiss. — Counterfeit Spider. — Simple and commodious electrical Machine.

TO PETER COLLINSON, ESQ. F. R. S. LONDON.

Philadelphia, July 11, 1747.

Sir,

In my last I informed you that, in pursuing our electrical enquiries, we had observed some particular phenomena, 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 silken thread from the cieling, right over the mouth of the bottle, suspend a small cork-ball, about the bigness of a marble; the thread 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, it is surprising 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*[2] as well as *draw off* the electrical fire; lay a long sharp needle upon the shot, and you cannot electrise the shot so as to make it repel the cork-ball. — Or fix a needle to the end of a suspended gun-barrel, or iron-rod, so as to point beyond it like a little bayonet[3]; and while it remains there, the gun-barrel, or rod, cannot by applying the tube to the other end be electrised so as to give a spark, the fire continually running out silently at the point. In the dark you may see it make the same appearance as it does in the case before-mentioned.

The repellency between the cork-ball and the shot is likewise 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]. 4. By candle-light, even though the candle is at a foot distance: these do it suddenly. — The light of a bright coal from a wood fire; and 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[5].

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, I could, if I had time, fill you a sheet[6]. The impossibility of electrising one's self (though standing on wax) by rubbing the tube, and drawing the fire from it; and the manner of doing it, by passing the tube near a person or thing standing on the floor, &c. had also occurred to us some months before Mr. Watson's ingenious *Sequel* came to hand, and these were some of the new things I intended to have communicated to you. — But now I need only mention some particulars not hinted in that piece, with our reasonings thereupon: though perhaps the latter might well enough be spared.

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

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

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

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

These appearances we attempt to account for thus: We suppose, as aforesaid, that electrical fire is a common element, of which every one of the three persons abovementioned has his equal share, before any operation is begun with the tube. *A*, who stands on wax and rubs the tube, collects the electrical fire from himself into the glass; and his communication with the common stock being cut off by the wax, his body is not again immediately supplied. *B*,(who stands on wax likewise) passing his knuckle along near the tube, receives the fire which was collected by the glass from *A*; and his communication with the common stock being likewise cut off, he retains the additional quantity received. — To *C*, standing on the floor, both appear to be electrised: for he having only the middle quantity of electrical fire, receives a spark upon approaching *B*, who has an over quantity; but gives one to *A*, who has an under quantity. If *A* and *B* approach to touch each other, the spark is stronger, because the difference between them is greater: After such touch there is no spark between either of them and *C*, because the electrical fire in all is reduced to the original equality. If they touch while electrising, the equality is never destroyed, the fire only circulating. Hence have arisen some new terms among us; we say *B*, (and bodies like circumstanced) is electrised *positively*; *A*, *negatively*. Or rather, *B* is electrised *plus*; *A*, *minus*. And we daily in our experiments electrise bodies *plus* or *minus*, as we think proper. — To electrise *plus* or *minus*, no more needs to be known than this, that the parts of the tube or sphere that are rubbed, do, in the instant of the friction, attract the electrical fire, and therefore take it from the thing rubbing: the same parts immediately, as the friction upon them ceases, are disposed to give the fire they have received, to any body that has less. Thus you may circulate it, as Mr. Watson has shewn; you may also accumulate or subtract it, upon, or from any body, as you connect that body with the rubber or with the receiver, the communication with the common stock being cut off. We think that ingenious gentleman was deceived when he imagined (in his *Sequel*) that the electrical fire came down the wire from the cieling to the gun-barrel, thence to the sphere, and so electrised the machine and the man turning the wheel, &c. We suppose it was *driven off*, and not brought on through that wire; and that the machine and man, &c. were electrised *minus*; *i. e.* had less electrical fire in them than things in common.

As the vessel is just upon sailing, I cannot give you so large an account of American electricity as I intended: I shall only mention a few particulars more. — We find granulated lead better to fill the phial with, than water, being easily warmed, and keeping warm and dry in damp air. — We fire spirits with the wire of the phial. — We light candles, just blown out, by drawing a spark among the smoke between the wire and snuffers. — We represent lightning, by passing the wire in the dark, over a china plate that has gilt flowers, or applying it to gilt frames of looking-glasses, &c. — We electrise a person twenty or more times running, with a touch of the finger on the wire, thus: He stands on wax. Give him the electrised bottle in his hand. Touch the wire with your finger, and then touch his hand or face; there are sparks every time[7]. — 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 electrised phial in hand; let the other take hold of the wire; there will be a small spark; but when their lips approach, they will be struck and shock'd. The same if another gentleman and lady, *C* and *D*, standing also on wax, and joining hands with *A* and *B*, salute or shake hands. We suspend by fine silk thread a counterfeit spider, made of a small piece of burnt cork, with legs of linnen thread, and a grain or two of lead stuck in him, to give him more weight. Upon the table, over which he hangs, we stick a wire upright, as high as the phial and wire, 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 more in dry weather. — We electrify, upon wax in the dark, a book that has a double line of gold round upon the covers, and then apply a knuckle to the gilding; the fire appears every where upon the gold like a flash of lightning: not upon the leather, nor, if you touch the leather instead of the gold. We rub our tubes with buckskin, and observe always to keep the same side to the tube, and never to sully the tube by handling; thus they work readily and easily, without the least fatigue, especially if kept in tight pasteboard cases, lined with flannel, and sitting close to the tube[8]. 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 grind-stone. 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. It is 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[9].

I am, &c.

B. FRANKLIN.

[2] 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.

[3] 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 surprised to find little or none.

[4] 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.

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

[6] 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.

[7] 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 touched, an equal quantity is restored to him from the person touching.

[8] Our tubes are made here of green glass, 27 or 30 inches long, as big as can be grasped.

[9] This simple easily-made machine was a contrivance of Mr. Syng's.

Observations on the Leyden Bottle, with Experiments proving the different electrical State of its different Surfaces.

TO PETER COLLINSON, ESQ. F. R. S. LONDON.

PHILADELPHIA, SEPT. 1, 1747.

Sir,

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 contained in the bottle differs, when electrised, from a non-electric electrised out of the bottle, in this: that the electrical fire of the latter is accumulated *on its surface*, and forms an electrical atmosphere round it of considerable extent; but the electrical fire is crowded *into the substance* of the former, the glass confining it[10].

2. At the same time that the wire and the top of the bottle, &c. is electrised *positively* or *plus*, the bottom of the bottle is electrised *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[11]. 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 contained 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 spewed 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 bottle, between the top and bottom, by some non-electric, touching or approaching both at the same time; in which case it is restored with a violence and quickness inexpressible; or, touching each alternately, in which case the equilibrium is restored by degrees.

4. As no more electrical fire can be thrown into the top of the bottle, when all is driven out of the bottom, so in a bottle not yet electrised, none can be thrown into the top, when none *can* get out at the bottom; which happens either when the bottom is too thick, or when the bottle is placed on an electric *per se*. Again, when the bottle is electrised, but little of the electrical fire can be *drawn out* from the top, by touching the wire, unless an equal quantity can at the same time *get in* at the bottom[12]. Thus, place an electrised bottle on clean glass or dry wax, and you will not, by touching the wire, get out the fire from the top. Place it on a non-electric, and touch the wire, you will get it out in a short time; but soonest when you form a direct communication as above.

So wonderfully are these two states of electricity, the *plus* and *minus*, combined and balanced in this miraculous bottle! situated and related to each other in a manner that I can by no means comprehend! If it were possible that a bottle should in one part contain a quantity of air strongly comprest, and in another part a perfect vacuum, we know the equilibrium would be instantly restored *within*. But here we have a bottle containing at the same time a *plenum* of electrical fire, and a *vacuum* of the same fire; and yet the equilibrium cannot be restored between them but by a communication *without!* though the *plenum* presses violently to expand, and the hungry vacuum seems to attract as violently in order to be filled.

5. The shock to the nerves (or convulsion rather) is occasioned by the sudden passing of the fire through the body in its way from the top to the bottom of the bottle. The fire takes the shortest[13] 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 shocked as much, though his shoes be dry, or even standing on wax, as otherwise. And on the touch of the wire, (or of the gun-barrel, which is the same thing) the fire does not proceed from the touching finger to the wire, as is supposed, but from the wire to the finger, and passes through the body to the other hand, and so into the bottom of the bottle.

Experiments confirming the above.

EXPERIMENT I.

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

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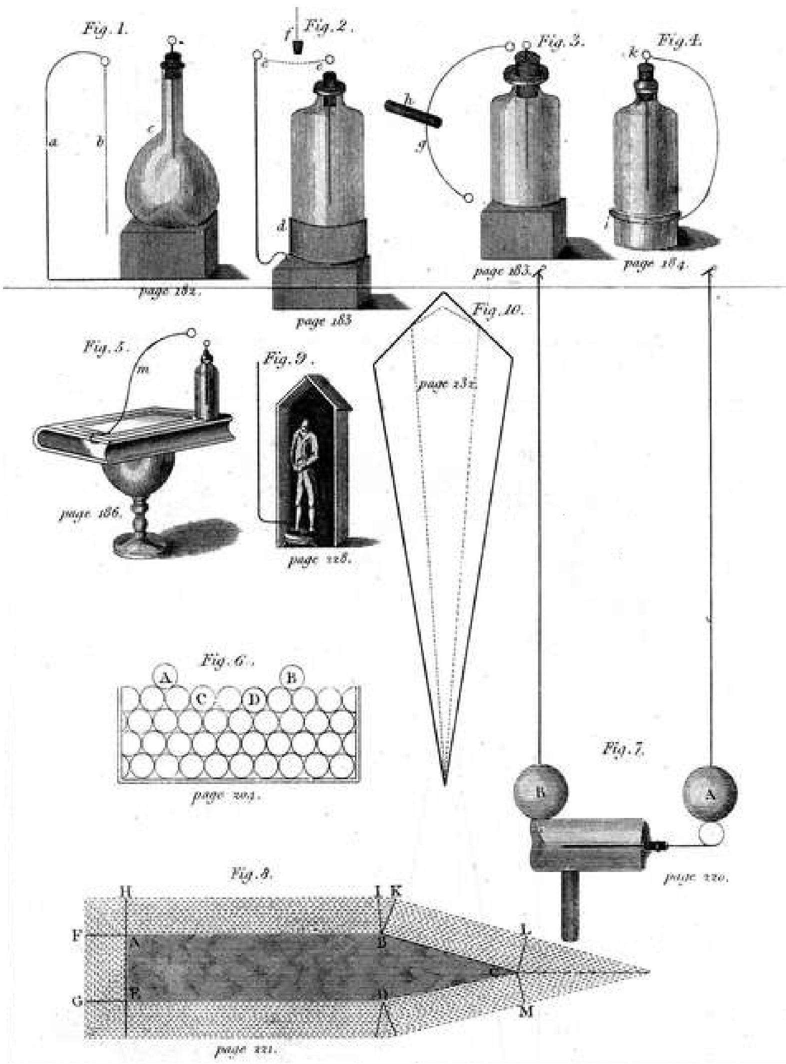
Plate I.



Published as the Act directs, April 1, 1806, by Longman, Hurst, Rees & Orme, Paternoster Row.

EXPERIMENT II.

Fig. 1. From a bent wire (*a*) sticking in the table, let a small linen thread (*b*) hang down within half an inch of the electrised phial (*c*). Touch the wire or 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 bellied-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 electrise the bottle, and place it on wax. If a cork suspended by a silk thread (*f*) hang between these two wires, it will play incessantly from one to the other, till the bottle is no longer electrised; that is, it fetches and carries fire from the top to the bottom[14] of the bottle, till the equilibrium is restored.

EXPERIMENT IV.

Fig. 3. Place an electrised phial on wax; take a wire (*g*) in form of a *C*, the ends at such a distance when bent, as that the upper may touch the wire of the bottle, when the lower touches the bottom: stick the outer part on a stick of sealing-wax (*h*), which

will serve as a handle; then apply the lower end to the bottom of the bottle, and gradually bring the upper end near the wire in the cork. The consequence is, spark follows spark till the equilibrium is restored. Touch the top first, and on approaching the bottom, with the other end, you have a constant stream of fire from the wire entering the bottle. Touch the top and bottom together, and the equilibrium will instantly be restored, the crooked wire forming the communication.

EXPERIMENT V.

Fig. 4. Let a ring of thin lead, or paper, surround a bottle (*i*) even at some distance from or above the bottom. From that ring let a wire proceed up, till it touch the wire of the cork (*k*). A bottle so fixt cannot by any means be electrised: the equilibrium is never destroyed: for 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[15]. Hence a bottle cannot be electrised that is foul or moist on the outside, if such moisture continue up to the cork or wire.

EXPERIMENT VI.

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

EXPERIMENT VII.

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

EXPERIMENT VIII.

Lay two books on two glasses, back towards back, two or three inches distant. Set the electrified phial on one, and then touch the wire; that book will be electrified *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 electrised *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 electrised a number of times by repeatedly touching the wire of an electrised bottle (held in the hand of one standing on the floor) he receiving the fire from the wire each time: yet holding it in his own hand, and touching the wire, though he draws a strong spark, and is violently shocked, no electricity remains in him; the fire only passing through him, from the upper to the lower part of the bottle. Observe, before the shock, to let some one on the floor touch him to restore the equilibrium in his body; for in taking hold of the bottom of the bottle, he sometimes becomes a little electrised *minus*, which will continue after the shock, as would also any *plus* electricity, which he 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[16] 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[17], and on the other end of the gold lines set the bottle electrised; then bend the springing wire, by pressing it with a stick of wax till its ring approaches the ring of the bottle wire, instantly there is a strong spark and stroke, and the whole line of gold, which completes the communication, between the top and bottom of the bottle, will appear a vivid flame, like the sharpest lightning. The closer the contact between the shoulder of the wire, and the gold at one end of the line, and between the bottom of the bottle and the gold at the other end, the better the experiment succeeds. The room should be darkened. If you would 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.

[10] See this opinion rectified in § 16 and 17 of the next letter. The fire in the bottle was found by subsequent experiments not to be contained in the non-electric, but *in the glass*. 1748.

[11] 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.

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

[13] Other circumstances being equal.

[14] *i. e.* from the inside to the outside.

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

[16] *i. e.* From the *inside* to the *outside*.

[17] 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.

Farther Experiments confirming the preceding Observations. — Leyden Bottle analysed. — Electrical Battery. — Magical Picture. — Electrical Wheel or Jack. — Electrical Feast.

TO PETER COLLINSON, ESQ. F. R. S. LONDON.

PHILADELPHIA, 1748.

SIR,

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

2. To take the charged 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 applied to the globe or tube; as when held by the coating, and the hook applied[18].

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

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

6. Vary the experiment, by charging two phials equally, one through the hook, the other through the coating: hold that by the coating which was charged through the hook; and that by the hook which was charged through the coating: apply the hook of the first to the coating of the 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[19] through the coating, the 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 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 straight 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 *charged* 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, and drew out the cork and wire which for that purpose had been loosely put in. Then taking the bottle in one hand, and bringing a finger of the other near its mouth, a strong spark came from the water, and the shock was as violent as if the wire had remained in it, which shewed that the force did not lie in the wire. Then to find if it resided in the water, being crowded into and condensed in it, as confined by the glass, which had been our former opinion, we electrified the bottle again, and placing it on glass, drew out the wire and cork as before; then taking up the bottle, we decanted all its water into an empty bottle, which likewise stood on glass; and taking up that other bottle, we expected, if the force resided in the water, to find a shock from it; but there was none. We judged then that it must either be lost in decanting, or remain in the first bottle. The latter we found to be true; for that bottle on trial gave the shock, though filled up as it stood with fresh unelectrified water from a tea-pot. — To find, then, whether glass had this property merely as glass, or whether the form contributed any thing to it; we took a pane of sash-glass, and laying it on the hand, placed a plate of lead on its upper surface; then electrified that plate, and bringing a finger to it, there was a spark and shock. We then took two plates of lead of equal dimensions, but less than the glass by two inches every way, and electrified the glass between them, by electrifying the uppermost lead; then separated the glass from the lead, in doing which, what little fire might be in the lead was taken out, and the glass being touched in the electrified parts with a finger, afforded only very small pricking sparks, but a great number of them might be taken from different places. Then dextrously placing it again between the leaden plates, and compleating a circle between the two surfaces, a violent shock ensued. — Which demonstrated the power to reside in glass as glass, and that the non-electrics in contact served only, like the armature of a loadstone, to unite the force of the several parts, and bring them at once to any point desired: it being the property of a non-electric, that the whole body instantly receives or gives what electrical fire is given to or taken from any one of its parts.

18. Upon this we made what we called an *electrical-battery*, consisting of eleven panes of large sash-glass, 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 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[20]; 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[21] 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 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[22] 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*.

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

22. But this wheel, like those driven by wind, water, or weights, moves by a foreign force, to wit, that of the bottles. The self-moving wheel, though constructed on the same principles, appears more surprising. It is 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 in the fire. In a circle on the table which supports the wheel, are fixed twelve small pillars of glass, at about four inches distance, with a thimble on the top of each. On the edge of the wheel is a small leaden bullet, communicating by a wire with the gilding of the *upper* surface of the wheel; and about six inches from it is another bullet, communicating in like manner with the *under* surface. When the wheel is to be charged by the upper surface, a communication must be made from the under surface to the table. When it is well charged it begins to move; the bullet nearest to a pillar moves towards the thimble on that pillar, and passing by electrifies it, and then pushes itself from it; the succeeding bullet, which communicates with the other surface of the glass, more strongly attracts that thimble, on account of its being before electrified by the other bullet; and thus the wheel encreases its motion till it comes to such a height as that the resistance of the air regulates it. It will go half an hour, and make one minute with another twenty turns in a minute, which is six hundred turns in the whole; the bullet of the upper surface giving in each turn twelve sparks to the thimbles, which makes seven thousand two hundred sparks: and the bullet of the under surface receiving as many from the thimbles; those bullets moving in the time near two thousand five hundred feet. — The thimbles are well fixed, and in so exact a circle, that the bullets may pass within a very small distance of each of them. — If instead of two bullets you put eight, four communicating with the upper surface, and four with the under surface, placed alternately, which eight, at about six inches distance, completes the circumference, the force and swiftness will be greatly increased, the wheel making fifty turns in a minute; but then it will not continue moving so long. — These wheels may be applied, perhaps, to the ringing of chimes,[23] and moving of light-made orreries.

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

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

25. The gilding being varnished over with turpentine varnish, the varnish, though dry and hard, is burnt by the spark drawn through it, and gives a strong smell and visible smoke. And when the spark is drawn 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.

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 furnished 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 surprised at the account given in Mr. Watson's book, of a shock communicated through a great space of dry ground, and suspect there must be some metalline quality in the gravel of that ground; having found that simple dry earth, rammed in a glass tube, open at both ends, and a wire hook inserted in the earth at each end, the earth and wires making part of a 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[24]. 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[25].

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

Chagrined a little that we have been hitherto able to produce nothing in this way of use to mankind; and the hot weather coming on, when electrical experiments are not so agreeable, it is proposed to put an end to them for this season, somewhat humorously, in a party of pleasure, on the banks of *Skuylkil*[26]. 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[27]. 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*[28], under the discharge of guns from the *electrical battery*.

[18] This was a discovery of the very ingenious Mr. Kinnersley, and by him communicated to me.

[19] 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.

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

[21] Contrived by Mr. Kinnersley.

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

[23] This was afterwards done with success by Mr. Kinnersley.

[24] Probably the ground is never so dry.

[25] 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.

[26] 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.

[27] 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 of the spirit. A small wire fastened by one end to the handle of the spoon, containing the spirit, was carried a-cross the river, and supported in the air by the rope commonly used to hold by, in drawing the ferry-boats over. The other end of this wire was tied round the coating of the bottle; which being charged, the spark was delivered from the hook to the top of the rod standing in the water on that side. At the same instant the rod on the other side delivered a spark into the spoon, and fired the spirit; the electric fire returning to the coating of the bottle, through the handle of the spoon and the supported wire connected with them.

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

TO PETER COLLINSON, ESQ. F. R. S. LONDON.

Observations and Suppositions, towards forming a new Hypothesis, for explaining the several Phenomena of Thunder-Gusts.[29]

SIR,

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

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

3. Air is an electric *per se*, and when dry will not conduct the electrical fire; it will neither receive it, nor give it to other bodies: otherwise no body surrounded by air, could be electrified positively and negatively: for should it be 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 in 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 detached 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 distance, whereby the triangles are dilated, and the air rises, carrying up with it the water.

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

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

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

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

24. The sun supplies (or seems to supply) common fire to 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 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 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 centre 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 centre, 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 ball 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. In the cohesion they shake off and drop their water, which represents rain.

35. Thus when sea and land clouds would pass at too 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 rarefied by the sun, it rises, the denser northern and southern air pressing into its place. The air so rarefied 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, it is 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 it is 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 it is 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 it is 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; 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 rarefied 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 scarcely 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[30].

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.

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.[31] 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 unwarmed 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 sundry menstrua can) will make the solid become a fluid, as well as

fire, yet without heating it: so the electrical fire, or lightning, creating a violent repulsion between the particles of the metal it passes through, the metal is fused.

54. If you would, by a violent fire, melt off the end of a nail, which is half driven into a door, the heat given the whole nail, before a part would melt, must burn the board it sticks in; and the melted part would burn the floor it 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[32].

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 thirteen years at Bermudas, says, there was less thunder there in that whole time than he has sometimes heard in a month at Carolina.

[28] 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.

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

[30] 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.

[31] 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. Ether takes fire most readily.

[32] 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 Philosophical Transactions, Vol. LI. part I.) And Mr. Kinnersley has found that a fine iron wire, melted by Electricity, has had the same effect.

Introductory Letter to some additional Papers.

TO PETER COLLINSON, ESQ. F. R. S. LONDON.

PHILADELPHIA, JULY 29, 1750.

Sir,

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 honorable Proprietary enabled us to carry those experiments to a greater height, by his generous present of a complete electrical apparatus; it is 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.

Opinions and Conjectures, concerning the Properties and Effects of the electrical Matter

and the Means of preserving Buildings, Ships, &c. from Lightning, arising from Experiments and Observations made at Philadelphia, 1749. — Golden Fish. — Extraction of effluvial Virtues by Electricity impracticable.

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

2. If any one should doubt whether the electrical matter passes 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[33].

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

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

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

8. It is 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 and retain it strongest, and contain the greatest quantity.

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

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

11. If a piece of common matter be supposed entirely free from electrical matter, and a single particle of the latter be brought nigh, it will be attracted, and enter the body, and take place in the centre, 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. It is 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 cieling. Apply the wire of a well-charged vial, held in your hand, to one of them (A) *Fig. 7*, and it will receive from the wire a quantity of the electrical fluid; but will not imbibe it, being already full. The fluid therefore will flow round its surface, and form an electrical atmosphere. Bring 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, 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[34]. 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.

16. The atmosphere of electrical particles surrounding an electrified sphere, is not more disposed to leave it, or more easily drawn off from any one part of the sphere

than another, because it is equally attracted by every part. But that is not the case with bodies of any other figure. From a cube it is more easily drawn at the corners than at the plane sides, and so from the angles of a body of any other form, and still most easily from the angle that is most acute. Thus, if a body shaped as A, B, C, D, E, in Fig. 8. be electrified, or have an electrical atmosphere communicated to it, and we consider every side as a base on which the particles rest, and by which they are attracted, one may see, by imagining a line from A to F, and another from E to G, that the portion of the atmosphere included in F, A, E, G, has the line A, E, for its basis. So the portion of atmosphere included in H, A, B, I, has the line A B for its basis. And likewise the portion included in K, B, C, L, has B, C, to rest on; and so on the other side of the figure. Now if you would draw off this atmosphere with any blunt smooth body, and approach the middle of the side A, B, you must come very near, before the force of your attracter exceeds the force or power with which that side holds its atmosphere. But there is a small portion between I, B, K, that has less of the surface to rest on, and to be attracted by, than the neighbouring portions, while at the same time there is a mutual repulsion between its particles, and the particles of those portions, therefore here you can get it with more ease, or at a greater distance. Between F, A, H, there is a larger portion that has yet a less surface to rest on, and to attract it; here, therefore, you can get it away still more easily. But easiest of all between L, C, M, where the quantity is largest, and the surface to attract and keep it back the least. When you have drawn away one of these angular portions of the fluid, another succeeds in its place, from the nature of fluidity, and the mutual repulsion before-mentioned; and so the atmosphere continues flowing off at such angle, like a stream, till no more is remaining. The extremities of the portions of atmosphere over these angular parts, are likewise at a greater distance from the electrified body, as may be seen by the inspection of the above figure; the point of the atmosphere of the angle C, being much farther from C, than any other part of the atmosphere over the lines C, B, or B, A: and, besides the distance arising from the nature of the figure, where the attraction is less, the particles will naturally expand to a greater distance by their mutual repulsion. On these accounts we suppose electrified bodies discharge their atmospheres upon unelectrified bodies more easily, and at a greater distance from their angles and points than from their smooth sides. — Those points will also discharge into the air, when the body has too great an electrical atmosphere, without bringing any non-electric near, to receive what is thrown off: For the air, though an electric *per se*, yet has always more or less water and other non-electric matters mixed with it: and these attract and receive what is so discharged.

17. But 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 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, show 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 embossed-paper, almost totally gilt. This large metallic surface supports a much greater electrical atmosphere than a rod of iron of 50 times the weight would do. It is suspended by silk lines, and when charged will strike, at near two inches distance, a pretty hard stroke, so as to make ones knuckle ach. Let a person standing on the floor present the point of a needle at 12 or more inches distance from it, and while the needle is so presented, the conductor cannot be charged, the point drawing off the fire as fast as it is thrown on by the electrical globe. Let it be charged, and then present the point at the same distance, and it will suddenly be discharged. In the dark you may see 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 cieling, so that the bottom of the scales may be about a foot from the floor: the scales will move round in a circle by the untwisting of the pack-thread. Set the iron punch on the end upon the floor, in such a place as that the scales may pass over it in making their circle: then electrify one scale, by applying the wire of a charged phial to it. As they move round, you see that scale draw nigher to the floor, and dip more when it comes over the punch; and if that be placed at a proper distance, the scale will snap and discharge its fire into it. But if a needle be stuck on the end of the punch, its point upwards, the scale, instead of drawing nigh to the punch, and snapping, discharges its

fire silently through the point, and rises higher from the punch. Nay, even if the needle be placed upon the floor near the punch, its point upwards, the end of the punch, 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; 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, with the electrical stroke, was a well-grown pullet.

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

24. Lightning melts metals, and I hinted in my paper on that subject, that I suspected it to be a cold fusion; I do not mean a fusion by force of cold, but a fusion without heat[35]. 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 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. The pieces I send you were stained with Dutch gold. True gold makes a darker stain, somewhat reddish; silver, a greenish stain. We once took two pieces of thick looking-glass, as broad as a Gunter's scale, and six inches long; and placing leaf-gold between them, put them between two smoothly-plained pieces of wood, and fixed 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 stained 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, it cannot leap over them, and so the shock is prevented.

26. From the before-mentioned law of electricity, that points as they are more or less acute, draw on and throw off the electrical fluid with more or less power, and at greater or less distances, and in larger or smaller quantities in the same time, we may see how to account for the situation of the leaf of gold suspended between two plates, the upper one continually electrified, the under one in a person's hand standing on the floor. When the upper plate is electrified, the leaf is attracted, and raised towards it, and would fly to that plate, were it not for its own points. The corner that happens to be uppermost when the leaf is rising, being a sharp point, from the 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 stopped, 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; 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 great 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 form points where you would not have them. You may make this figure so acute below, and blunt above, as to need no under plate, it discharging fast enough into the air. When it is made narrower, as the figure between the pricked lines, we call it the *golden fish*, from its manner of acting. For if you take it by the tail, and hold it at a foot or greater horizontal distance from the prime conductor, it will, when let go, fly to it with a brisk but wavering motion, like that of an eel through the water; it will then take place under the prime conductor, at perhaps a quarter or half an inch distance, and keep a continual shaking of its tail like a fish, so that it seems animated. Turn its tail towards the prime

conductor, and then it flies to your finger, and seems to nibble it. And if you hold a plate under it at six or eight inches distance, and cease turning the globe when the electrical atmosphere of the conductor grows small, it will descend to the plate and swim back again several times with the same fish-like motion, greatly to the entertainment of spectators. By a little practice in blunting or sharpening the heads or tails of these figures, you may make them take place as desired, nearer or farther from the electrified plate.

27. It is said in Section 8, of this paper, that all kinds of common matter are supposed not to attract the electrical fluid with equal strength; and that those called electrics *per se*, as glass, &c. attract and retain it strongest, and contain the greatest quantity. This latter position may seem a paradox to some, being contrary to the hitherto received opinion; and therefore I shall now endeavour to explain it.

28. In order to this, let it first be 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 alleged to prove it. But, if the electrical fluid so easily pervades glass, how does the phial become *charged* (as we term it) when we hold it in our hands? Would not the fire, thrown in by the wire, pass through to our hands, and so escape into the floor? Would not the bottle in that case be left just as we found it, uncharged, as we know a metal bottle so attempted to be charged would be? Indeed, if there be the least crack, the minutest solution of continuity in the glass, though it remains so tight that nothing else we know of will pass, yet the extremely subtile electric fluid flies through such a crack with the greatest freedom, and such a bottle we know can never be charged: what then makes the difference between such a bottle and one that is sound, but this, that the fluid can pass through the one, and not through the other[36]?

29. It is true, there is an experiment that at first sight would be apt to satisfy a slight observer, that the 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[37]! 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 violence with which it is resumed by the part that had been deprived of it, when there is an opportunity. And by this, that we cannot from a mass of glass, draw a quantity of electric fire, or electrify the whole mass *minus*, as we can a mass of metal. We cannot lessen or increase its whole quantity, for the quantity it has it holds;

and it has as much as it can hold. Its pores are filled with it as full as the mutual repellency of the particles will admit; and what is already in, refuses, or strongly repels, any additional quantity. Nor have we any way of moving the electrical fluid in glass, but one; that is, by covering part of the two surfaces of thin glass with non-electrics, and then throwing an additional quantity of this fluid on one surface, which spreading in the non-electric, and being bound by it to that surface, acts by its repelling force on the particles of the electrical fluid contained in the other surface, and drives them out of the glass into the non-electric on that side from whence they are discharged, and then those added on the charged side can enter. But when this is done, there is no more in the glass, nor less than before, just as much having left it on one side as it received on the other.

33. I feel a want of terms here, and doubt much whether I shall be able to make this part intelligible. By the word *surface*, in this case, I do not mean mere length and breadth without thickness; but when I speak of the upper or under surface of a piece of glass, the outer or inner surface of the phial, 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 exceedingly 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 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 not say confirming) this hypothesis, I must, to avoid repetition, beg leave to refer you back to what is said of the electrical phial in my former papers.

34. Let us now see how it will account for several other appearances. — Glass, a body extremely elastic, (and perhaps its elasticity may be owing in some degree to the subsisting of so great a quantity of this repelling fluid in its pores) must, when rubbed, have its rubbed surface somewhat stretched, or its solid parts drawn a little farther asunder, so that the vacancies in which the electrical fluid resides, become larger, affording room for more of that fluid, which is immediately attracted into it from the cushion or hand rubbing, they being supplied from the common stock. But the instant the parts of the glass so opened and filled, have passed the friction, they close again, and force the additional quantity out upon the surface, where it must rest till that part comes round to the cushion again, unless some non-electric (as the prime conductor, first presents to receive it[38]). 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 rubbed part of the globe's outer surface, drives an equal quantity out of the inner surface into that non-electric lining, which receiving it, and carrying it away from the rubbed 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 charged 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 rubbed[39], 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, 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 overcharged pores in the outer surface discharge that overplus; the equilibrium being restored through your body, which could not be restored through the glass[40]. 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 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 confined, which, from the mutual repulsion of its particles, tends to dissipation, and would immediately dissipate *in vacuo*. — And thus the experiment of the feather inclosed in a glass vessel hermetically sealed, but moving on the approach of the rubbed tube, is explained. When an additional quantity of the electrical fluid is applied to the side of the vessel by the atmosphere of the tube, a quantity is repelled and driven out of the inner surface of that side into the vessel, and there affects the feather, returning again into its pores, when the tube with its atmosphere is withdrawn; not that the particles of that atmosphere did themselves pass through the glass to the feather. And every other appearance I have yet seen, in which glass and electricity are concerned, are, I think, explained with equal ease by the same hypothesis. Yet, perhaps, it may not be a true one, and I shall be obliged to him that affords me a better.

35. Thus I take the difference between non-electrics, and glass, an electric *per se*, to consist in these two particulars. 1st, That a non-electric easily suffers a change in the quantity of the electric fluid it contains. You may lessen its whole quantity, by drawing out a part, which the whole body will again resume; but of glass you can only lessen the quantity contained in one of its surfaces; and not that, but by supplying an equal quantity at the same time to the other surface: so that the whole glass may always have the same quantity in the two surfaces, their two different quantities being added together. And this can only be done in glass that is thin; beyond a certain thickness we have yet no power that can make this change. And, 2dly, that the electric fire freely removes from place to place, in and through the substance of a non-electric, but not so through the substance of glass. If you offer a quantity to one end of a long rod of metal, it receives it, and when it enters, every particle that was before in the rod pushes its neighbour quite to the 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 supplied 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, touched 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 electrised. I likewise put into a phial, instead of water, a strong purgative liquid, and then charged the phial, and took repeated shocks from it, in which case every particle of the electrical fluid must, before it went through my body, have first gone through the liquid when the phial is charging, and returned through it when discharging, yet no other effect followed than if it had been charged with water. I have also smelt the electric fire when drawn 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 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.

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.

ADDITIONAL EXPERIMENTS:

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 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.

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

But if the fire with which the inside surface is surcharged be so much precisely as is wanted by the outside surface, it will pass round through the wire fixed to the wax handle, restore the equilibrium in the glass, and make no alteration in the state of the prime conductor.

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

[33] See the ingenious Essays on Electricity, in the Transactions, by Mr. Ellicot.

[34] See page 173.

[35] See note in page 214.

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

[37] See Sect. 10, of *Farther Experiments*, &c.

[38] 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 applied to take it off the glass, the back crescent disappears.

[39] Gilt paper, with the gilt face next the glass, does well

[40] See *Further Experiments*, Sect. 15.

TO PETER COLLINSON, ESQ. F. R. S. LONDON.

Accumulation of the electrical Fire proved to be in the electrified Glass. — Effect of Lightning on the Needle of Compasses, explained. — Gunpowder fired by the electric Flame.

Philadelphia, July 27, 1750.

Sir,

Mr. Watson, I believe, wrote his Observations on my last paper in haste, without having first well considered the experiments related §. 17[41], 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, 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 bason 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 bason, with the arms and body of the person on the wax, being all together but as one long wire, reaching from the internal surface of the phial to the spirits.

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

His compasses lost the virtue of the load-stone, or the poles were reversed; the north point turning to the south. — By electricity we have (*here* at *Philadelphia*) frequently given polarity to needles, and reversed it at pleasure. Mr. Wilson, at London, tried it on too large masses, and with too small force.

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

The polarity given is strongest when the needle is struck lying north and south, weakest when lying east and west; perhaps if the force was still greater, the south end, 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 off hold seven or eight gallons, and are coated and lined with tin foil; each of them takes a thousand turns[42] 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.

Your’s, &c.

B. FRANKLIN.

[41] See the paper entitled, *Farther Experiments, &c.*

[42] 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.

Unlimited Nature of the electric Force.

TO C. C[43]. ESQ. AT NEW-YORK, COMMUNICATED TO MR. COLLINSON.

PHILADELPHIA, 1751.

SIR,

I inclose you answers, such as my present hurry of business will permit me to make, to the principal queries contained in your's 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 none, and fired dry gunpowder by the electric spark. I have five bottles that contain eight or nine gallons each, two of which charged 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.

The Terms, electric per se, and non-electric, improper. — New Relation between Metals and Water.

Effects of Air in electrical Experiments. — Experiment for discovering more of the Qualities of the electric Fluid.

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 rubbed, discovered 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 show, 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 *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[44]. 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. Moist air receives and conducts the electrical matter in proportion to its moisture, quite dry air not at all: air is therefore to be classed with the non-conductors.

Dry air assists in confining the electrical atmosphere to the body it surrounds, and prevents its dissipating: for in vacuo it quits easily, and points operate stronger, *i. e.*

they throw off or attract the electrical matter more freely, and at greater distances; so that air intervening obstructs its 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[45] 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 one hundred 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 eight hundred yards of air, allowing my arm in giving the motion to add a foot to the semi-diameter of the circle. — By quite dry air, I mean the dryest we have: for perhaps we never have any perfectly free from moisture. An electrical atmosphere raised round a thick wire, inserted in a phial of air, drives out none of the air, nor on withdrawing that atmosphere will any air rush in, as I have found by a curious experiment[46] 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, nearly the colour of a watch spring.

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

On gold, brass, or tin, I have not perceived 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.

[43] Cadwallader Colden, who was afterwards lieutenant-governor of New-York. *Editor.*

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

[45] A cold dry wind of North America.

[46] 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 rarefying 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 rarefy the included air, and of course depress the drop of ink in the syphon. But no such effect followed.

Mistake, that only Metals and Water were Conductors, rectified. — Supposition of a Region of electric Fire above our Atmosphere. — Theorem concerning Light. — Poke-Weed a Cure for Cancers.

TO C. C[47]. ESQ. AT NEW YORK.

READ AT THE Royal Society, Nov. 11, 1756.

Philadelphia, April 23, 1752.

Sir,

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 forgotten.

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 mistake. 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 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 mere air, as it does with other matter. It will not quit common matter to join with air. Air obstructs, in some degree, its motion. An electric atmosphere cannot be communicated at so great a distance, through intervening air, by far, as through a vacuum. — Who knows then, but there may be, as the ancients thought, a region of this fire above our atmosphere, prevented by our air, and its own too great distance for attraction, from joining our earth? Perhaps where the atmosphere is rarest, this fluid may be densest, and nearer the earth where the atmosphere grows denser, this fluid may be rarer; yet some of it be low enough to attach itself to our highest clouds, and thence they becoming electrified, may be attracted by, and descend towards the earth, and discharge their watry contents, together with that etherial fire. Perhaps the *auroræ boreales* are currents of this fluid in its own region, above our atmosphere, becoming from their motion visible. There is no end to conjectures. As yet we are but novices in this branch of natural knowledge.

You mention several differences of salts in electrical experiments. Were they all equally dry? Salt is apt to acquire moisture from a moist air, and some sorts more than others. When perfectly dried by lying before a fire, or on a stove, none that I have tried will conduct any better than so much glass.

New flannel, if dry and warm, will draw the electric fluid from non-electrics, as well as that which has been worn.

I wish you had the convenience of trying the experiments you seem to have such expectations from, upon various kinds of spirits, salts, earth, &c. Frequently, in a variety of experiments, though we miss what we expected to find, yet something valuable turns out, something surprising, and instructing, though unthought of.

I thank you for communicating the illustration of the theorem concerning light. It is very curious. But I must own I am much in the *dark* about *light*. I am not satisfied with the doctrine that supposes particles of matter called light, continually driven off from the sun's surface, with a swiftness so prodigious! Must not the smallest particle conceivable have, with such a motion, a force exceeding that of a twenty-four pounder, discharged from a cannon? Must not the Sun diminish exceedingly by such a waste of matter; and the planets, instead of drawing nearer to him, as some have feared, recede to greater distances through the lessened attraction. Yet these particles, with this amazing motion, will not drive before them, or remove, the least and lightest dust they meet with: And the Sun, for aught we know, continues of his antient dimensions, and his attendants move in their antient orbits.

May not all the phenomena of light be more conveniently solved, by supposing universal space filled with a subtle elastic fluid, which, when at rest, is not visible, but whose vibrations affect that fine sense in the eye, as those of air do the grosser organs of the ear? We do not, in the case of sound, imagine that any sonorous particles are thrown off from a bell, for instance, and fly in strait lines to the ear; why must we believe that luminous particles leave the sun and proceed to the eye? Some diamonds, if rubbed, shine in the dark, without losing any part of their matter. I can make an electrical spark as big as the flame of a candle, much brighter, and, therefore, visible further; yet this is without fuel; and, I am persuaded, no part of the electric fluid flies off in such case to distant places, but all goes directly, and is to be found in the place to which I destine it. May not different degrees of the vibration of the above-mentioned universal medium, occasion the appearances of different colours? I think the electric fluid is always the same; yet I find that weaker and stronger sparks differ in apparent colour, some white, blue, purple, red; the strongest, white; weak ones red. Thus different degrees of vibration given to the air produce the seven, different sounds in music, analagous to the seven colours, yet the medium, air, is the same.

If the Sun is not wasted by expence of light, I can easily conceive that he shall otherwise always retain the same quantity of matter; though we should suppose him made of sulphur constantly flaming. The action of fire only *separates* the particles of matter, it does not *annihilate* them. Water, by heat raised in vapour, returns to the earth in rain; and if we could collect all the particles of burning matter that go off in smoak, perhaps they might, with the ashes, weigh as much as the body before it was fired: and if we could put them into the same position with regard to each other, the mass would be the same as before, and might be burnt over again. The chymists have analysed sulphur, and find it composed, in certain proportions, of oil, salt, and earth; and having, by the analysis, discovered those proportions, they can, of those ingredients, make sulphur. So we have only to suppose, that the parts of the Sun's sulphur, separated by fire, rise into his atmosphere, and there being freed from the immediate action of the fire, they collect into cloudy masses, and growing, by degrees, too heavy to be longer supported, they descend to the Sun, and are burnt over again. Hence the spots appearing on his face, which are observed to 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 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[48].

I am, Sir, &c.

B. FRANKLIN.

[47] Cadwallader Colden. See note, page 250. *Editor.*

[48] As the poke-weed, though out of place, is introduced here, we shall translate and insert two extracts of letters from Dr. Franklin to M. Dubourg, the French translator of his works, on the same subject.

"LONDON, MARCH 27, 1773.

"I apprehend that our poke-weed is what the botanists term *phytolacca*. This plant bears berries as large as peas; the skin is black, but it contains a crimson juice. It is this juice, thickened by evaporation in the sun, which was employed. It caused great pain, but some persons were said to have been cured. I am not quite certain of the facts; all that I know is, that Dr. Colden had a good opinion of the remedy."

"LONDON, APRIL 23, 1773.

"You will see by the annexed paper by Dr. Solander, that this herb, poke-weed, in which has been found a specific remedy for cancers, is the most common species of phytolacca. (Phytolacca decandra L.)"

Editor.

New Experiments.

Paradoxes inferred from them. — Difference in the Electricity of a Globe of Glass charged, and a Globe of Sulphur. — Difficulty of ascertaining which is positive and which negative.

MR. E. KINNERSLEY, AT BOSTON, TO BENJAMIN FRANKLIN, ESQ. AT PHILADELPHIA.

FEB. 3, 1752.

SIR,

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 surprised me very much, and have induced me to infer the following paradoxes.

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

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

Probable Cause of the Different Attractions and Repulsions of the two electrified Globes mentioned in the two preceding Letters.

TO MR. E. KINNERSLEY, AT BOSTON.

PHILADELPHIA, MARCH 2, 1752.

Sir,

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.

Reasons for supposing, that the glass Globe charges positively, and the Sulphur negatively. — Hint respecting a leather Globe for Experiments when travelling.

TO MR. E. KINNERSLEY, AT BOSTON.

PHILADELPHIA, MARCH 16, 1752.

Sir,

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

I found also, that the wire of a phial 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 between the two phials, just as when one phial was charged through the wire, the other through the coating, by the glass globe alone. And two phials charged, the one by the brimstone globe, the other by the glass globe, would be both discharged by bringing their wires together, and shock the person holding the phials.

From these experiments one may be certain that your 2d, 3d, and 4th proposed experiments, would succeed exactly as you suppose, though I have not tried them, wanting time. I imagine it is the glass globe that charges positively, and the sulphur negatively, for these reasons: 1. Though the sulphur globe seems to work equally well with the glass one, yet it can never occasion so large and distant a spark between my knuckle and the conductor, when the sulphur one is working, as when the glass one is used; which, I suppose, is occasioned by this, that bodies of a certain bigness cannot so easily part with a quantity of electrical fluid they have and hold attracted *within* their substance, as they can receive an additional quantity *upon* their surface by way of atmosphere. Therefore so much cannot be drawn *out* of the conductor, as can be thrown *on* it. 2. I observe that the stream or brush of fire, appearing at the end of a wire, connected with the conductor, is long, large, and much diverging, when the glass globe is used, and makes a snapping (or rattling) noise: but when the sulphur one is used, it is short, small, and makes a hissing noise; and just the reverse of both happens, when you hold the same wire in your hand, and the globes are worked alternately: the brush is large, long, diverging, and snapping (or rattling) when the sulphur globe is 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 positively? Such a globe would be in no danger of breaking[49]. 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.

[49] 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.

[In Mr. Collinson's edition, several papers followed here, by the Abbé Mazeas, and others, upon the subject of Dr. Franklin's experiments, which, that the letters of our author might not be too much interrupted, we have thought proper to transfer to an Appendix. A subsequent paper by Mr. David Colden, entitled Remarks on the Abbé Nollet's Letters to Benjamin Franklin, esq. on Electricity, will be found transferred in the same manner.]

Electrical Kite.

TO PETER COLLINSON, ESQ. F. R. S. LONDON.

PHILADELPHIA, OCT. 19, 1752.

Sir,

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

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

Hypothesis, of the Sea being the grand Source of Lightning, retracted.

Positive, and sometimes negative, Electricity of the Clouds discovered. — New Experiments and Conjectures in Support of this Discovery. — Observations recommended for ascertaining the Direction of the electric Fluid. — Size of Rods for Conductors to Buildings. — Appearance of a Thunder-Cloud described.

TO PETER COLLINSON, ESQ. F. R. S. LONDON.

PHILADELPHIA, SEPTEMBER, 1753.

SIR,

In 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 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 electrified: 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 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 cieling, 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 lightning for me in electrical phials, and they did so, yet it was mostly dissipated before my return, and in some of the other gusts, the quantity of lightning I was able to obtain was so small, and the charge so weak, that I could not satisfy myself: yet I sometimes saw what 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 electrised *negatively*.

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

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

I had another concurring experiment, which I often repeated, to prove the negative state of the clouds, viz. while the bells were ringing, I took the phial charged from the glass globe, and applied its wire to the erected rod, considering, that if the clouds were electrised *positively*, the rod which received its electricity from them must be so too; and then the additional *positive* electricity of the phial would make the bells ring faster: — But, if the clouds were in a *negative* state, they must exhaust the electric fluid from my rod, and bring that into the same negative state with themselves, and then the wire of a positively 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 stopped 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, stopped 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 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 fixed 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 though 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 diffused throughout their substance, a quantity of the electric fluid, just as much as they can contain, which I call the *natural quantity*.

That this natural quantity is not the same in all kinds of common matter under the same dimensions, nor in the same 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 spunge, 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 a spunge 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* squeezed 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 spunge will not only resume what was lately forced out, but attract an additional quantity.

As the spunge 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 attacks and absorbs in either state, its *natural quantity*, the state being considered.

Now what the spunge 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 rarefied 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 rarefied state, or in the form of a cloud, will be in a negative state of electricity; it will have less than its *natural quantity*; that is, less than it is naturally capable of attracting and absorbing in that state.

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

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

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

The cloud thus supplied, having parted with much of what it first received, 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 compared with the earth, agreeable to most of our experiments; yet as by one experiment we found a cloud electrised 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, 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 cieling, where it passed over a pulley, and came down again to my hand, that I might at pleasure draw the chain up out of the can, extending it till within a foot of the cieling, and let it gradually sink into the can again. — From the cieling, by another thread of fine raw silk, I suspended a small light lock of cotton, so as that when it hung perpendicularly, it came in contact with the side of the can. — Then approaching the wire of a charged phial 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 phial; 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 phial 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 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 centre 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 it be asked, 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 thirty-sixth part of a grain, according to M. Reaumur; yet it is 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 five thousand times as much metal as there is in that gold line, and if so, it will conduct the charge of twenty-five thousand 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, though 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 compared, will at length point out the proper size to greater certainty.

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

Additional Proofs of the positive and negative State of Electricity in the Clouds. — New Method of ascertaining it.

TO PETER COLLINSON, ESQ. F. R. S. LONDON.

PHILADELPHIA, APRIL 18, 1754.

Sir,

Since September last, having been abroad on two long journeys, and otherwise much engaged, 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 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 rubbed 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 repelled from the glass stopper as well as from the bell. If negative, it will fly to the stopper.

B. FRANKLIN.

Electrical experiments, with an attempt to account for their several phenomena &c.

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.

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

EXPERIMENT II.

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

As the balls in the first experiment are not insulated, they cannot properly be said to be electrified: but when they hang within the atmosphere of the excited tube, they may attract and condense the electrical fluid round about them, and be separated by the repulsion of its particles. It is 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 balls, and be condensed there, in its passage towards the wax; for, according to Mr. Franklin, excited glass *emits* the electrical fluid, but excited wax *receives* it.

EXPERIMENT III.

Let a tin tube, of four or five feet in length, and about two inches in diameter, be insulated by silk; and from one end of it let the cork-balls be suspended by linen threads. Electrify it, by bringing the excited glass tube near the other end, so as that the balls may stand an inch and an half, or two inches, apart: 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.

EXPERIMENT IV.

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

The bringing the excited glass to the end, or edge of the 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 increased, or diminished; the repulsion of the balls will be increased, or diminished, accordingly.

EXPERIMENT V.

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

EXPERIMENT VI.

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

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

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

EXPERIMENT VII.

Let the tin tube, with a pair of balls at one end, be placed three feet at least from any part of the room, and the air 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 columns of mercury; but, as the tube is withdrawn, the fire is supposed to return.

EXPERIMENT IX.

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

By this experiment it appears, that wax also may be electrified positively and negatively. And it is probable, that all bodies whatsoever may have the quantity they contain of the electrical fluid, increased, or diminished. The clouds, I have observed, by a great number of experiments, to be some in a positive, and others in a negative state of electricity. For the cork balls, electrified by them, will sometimes close at the approach of excited glass; and at other times be separated to a greater distance. And this change I have known to happen five or six times in less than half an hour; the balls coming together each time and remaining in contact a few seconds, before they repel each other again. It may likewise easily be discovered, by a charged phial, whether the electrical fire be drawn out of the apparatus by a negative cloud, or forced 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 electrised for ten minutes, or a quarter of an hour, after the clouds have passed the zenith; and sometimes till they appear more than half-way towards the horizon. Rain, especially when the drops are large, generally brings down the electrical fire: and hail, in summer, I believe never fails. When the apparatus was last electrified, it was by the fall of thawing snow, which happened so lately, as on the 12th of November; that being the twenty-sixth day, and sixty-first time it has been electrified, since it was first set up; which was about the middle of May. And as Fahrenheit's thermometer was but seven degrees above freezing, it is supposed the winter will not 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 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?

EXPERIMENTS

Experiments Made in Pursuance of those made by Mr. Canton, dated December 6, 1753; with Explanations, by Mr. Benjamin Franklin.

READ AT THE Royal Society, Dec. 18, 1755.

Philadelphia, March 14, 1755.

PRINCIPLES.

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

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

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

This 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 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 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, and leaves the prime conductor electrised negatively, as appears by the next operation.

Then withdraw the tube, and they will open again.

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

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

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

Withdraw it, and they will open again.

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

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

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

EXPERIMENT III.

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

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

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

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

Turkey killed by Electricity. — Effect of a Shock on the Operator in making the Experiment.

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

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

Differences in the Qualities of Glass.

Account of Domien, an Electrician and Traveller. — Conjectures respecting the Pores of Glass. — Origin of the Author's Idea of drawing down Lightning. — No satisfactory Hypothesis respecting the Manner in which Clouds become electrified. — Six Men knocked down at once by an electrical Shock. — Reflections on the Spirit of Invention.

TO DR. L —— [50], AT CHARLES TOWN, SOUTH CAROLINA.

PHILADELPHIA, MARCH 18, 1755.

Sir,

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[51].

In answer to your several enquiries. The tubes and globes we use here, are chiefly made here. The glass has a greenish cast, but is clear and hard, and, I think, better for electrical experiments than the white glass of London, which is not so hard. There are certainly great differences in glass. A white globe I had made here some years since, would never, by any means, be excited. Two of my friends tried it, as well as myself, without success. At length, putting it on an electric stand, a chain from the prime conductor being in contact with it, I found it had the properties of a non-electric; for I could draw sparks from any part of it, though it was very clean and dry.

All I know of Domien, is, that by his own account he was a native of Transylvania, of Tartar descent, but a priest of the Greek church: he spoke and wrote Latin very readily and correctly. He set out from his own country with an intention of going round the world, as much as possible by land. He travelled through Germany, France, and Holland, to England. Resided some time at Oxford. From England he came to Maryland; thence went to New England; returned by land to Philadelphia; and from hence travelled through Maryland, Virginia, and North Carolina to you. He thought it might be of service to him in his travels to know something of electricity. I taught him the use of the tube; how to charge the Leyden phial, and some other experiments. He wrote to me from Charles-Town, that he had lived eight hundred miles upon electricity, it had been meat, drink, and cloathing to him. His last letter to me was, I think, from Jamaica, desiring me to send the tubes you mention, to meet him at the 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! 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. Kinnersley has found, by a very good experiment, that when the charge of a bottle hath an opportunity of passing two ways, *i. e.* straight 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 phial containing water will give a smart shock, one containing the same quantity of mercury will give one much stronger, the mercury being the better conductor; while one containing oil, only, will scarce give any shock at all.

Your question, how I came first to think of proposing the experiment of drawing down the lightning, in order to ascertain its sameness with the electric fluid, I cannot answer better than by giving you an extract from the minutes I used to keep of the experiments I made, with memorandums of such as I purposed to make, the reasons for making them, and the observations that arose upon them, from which minutes my letters were afterwards drawn. By 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 an electrician.

"Nov. 7, 1749. Electrical fluid agrees with lightning in these particulars: 1. Giving light. 2. Colour of the light. 3. Crooked direction. 4. Swift motion. 5. Being conducted by metals. 6. Crack or noise in exploding. 7. Subsisting in water or ice. 8. 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 linen 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 electrised 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 electrised, 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 electrised 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 knocking down of the six men was performed with two of my large jars 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 jars. When they were thus placed, I applied the other end of my rod to the prime conductor, and they all dropped 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 dropped, 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 charge might, indeed, kill a man, but I have not yet seen any hurt done by it. It would certainly, as you observe, be the easiest of all deaths.

The experiment you have heard so imperfect an account of, is merely this: I electrified a silver pint can, 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 can. The cork was not attracted to the inside of the can 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[52]. 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 practise it: I think it an honest policy. Those who affect to be thought to know every thing, and so undertake to explain every thing, often remain long ignorant of many things that others could and would instruct them in, if they appeared less conceited.

The treatment your friend has met with is so common, that no man who knows what the world is, and ever has been, should expect to escape it. There are every where a number of people, who, being totally destitute of any inventive faculty themselves, do not readily conceive that others may possess it: they think of inventions as of miracles; there might be such formerly, but they are ceased. 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 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 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. FRANKLIN.

[50] Dr. Lining. — Editor.

[51] See page 286, for the paper here mentioned.

[52] Mr. F. has since thought, that, possibly, the mutual repulsion of the inner opposite sides of the electrised can 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.

Beccaria's Work on Electricity. — Sentiments of Franklin on pointed Rods, not fully understood in Europe. — Effect of Lightning on the Church of Newbury, in New England. — Remarks on the Subject.

TO MONS. DALIBARD, AT PARIS, INCLOSED IN A LETTER TO MR. PETER COLLINSON, F. R. S.

READ AT THE Royal Society, Dec. 18, 1755.

Philadelphia, June 29, 1755.

Sir,

You desire my opinion of Pere Beccaria's Italian book[53]. 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[54]. 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 show that the electric fluid could not pass without making a hole. In the same manner we say, glass is 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 think he only speaks modestly and judiciously. I find I have been but partly understood in that matter. I have mentioned it in several of my letters, and except once, always in the *alternative, viz*. that pointed rods erected on buildings, and communicating with the moist earth, would either *prevent* a stroke, *or*, if not prevented, would *conduct* it, so as that the building should suffer no damage. Yet whenever my opinion is examined in Europe, nothing is considered but the probability of those rods *preventing* a stroke or explosion, which is only a *part* of the use I proposed for them; and the other part, their conducting a stroke, which they may happen not to prevent, seems to be totally forgotten, though of equal importance and advantage.

I thank you for communicating M. de Buffon's relation of the effect of lightning at Dijon, on the 7th of June last. In return, give me leave to relate an instance I lately saw of the same kind. Being in the town of Newbury in New England, in November last, I was shewn the effect of lightning on their church, which had been struck a few months before. The steeple was a square tower of wood, reaching seventy feet up from the ground to the place where the bell hung, over which rose a taper spire, of wood likewise, reaching seventy feet higher, to the vane of the weather-cock. Near the bell was fixed an iron hammer to strike the hours; and from the tail of the hammer a wire went down through a small gimlet-hole in the floor that the bell stood upon, and through a second floor in like manner; then horizontally under and near the plaistered cieling of that second floor, till it came near a plaistered wall; then down by the side of that wall to a clock, which stood about twenty feet below the bell. The wire was not bigger than a common knitting-needle. The spire was split all to pieces by the lightning, and the parts flung in all directions over the square in which the church stood, so that nothing remained above the bell.

The lightning passed between the hammer and the clock in the above-mentioned wire, without hurting either of the floors, or having any effect upon them (except making the gimlet-holes, through which the wire passed, a little bigger,) and without hurting the plaistered wall, or any part of the building, so far as the aforesaid wire and the pendulum wire of the clock extended; which latter wire was about the thickness of a goose-quill. From the end of the pendulum, down quite to the ground, the building was exceedingly rent and damaged, and some stones in the foundation-wall torn out, and thrown to the distance of twenty or thirty feet. No part of the 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 toward the edges, all along the cieling, under which it passed, and down the wall. These were the effects and appearances; on which I would only make the few following remarks, viz.

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

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

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

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

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

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

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

[53] 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 4to. 1753; between the two parts is a letter to the Abbé Nollet, in defence of Mr. Franklin’s system. *J. Bevis.*

[54] These papers will be found in Vol II. *Editor.*

Notice of another Packet of Letters.

TO PETER COLLINSON, ESQ. F. R. S. LONDON.

PHILADELPHIA, NOV. 23, 1753.

DEAR FRIEND.

In 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[55].

B. FRANKLIN.

[55] These letters and papers are a philosophical correspondence between Mr. Franklin and some of his American Friends[56]. Mr. Collinson communicated them to the Royal Society, where they were read at different meetings during the year 1756. But Mr. Franklin having particularly requested that they might not be printed, none of them were inserted in the transactions. Mr. F. had at that time an intention of revising them, and pursuing some of the enquiries farther; but finding that he is not like to have sufficient leisure, he has at length been induced, imperfect as they are, to permit their publication, as some of the hints they contain may possibly be useful to others in their philosophical researches. Note in Mr. Collinson's edition.

[56] As some of these papers are upon subjects not immediately connected with electricity, we have taken such papers from the order in which they were placed by Mr. Collinson, and transferred them to other parts of the work. *Editor.*

Extract of a Letter from a Gentleman in Boston[57], to Benjamin Franklin, Esq. concerning the crooked Direction, and the Source of Lightning, and the Swiftness of the electric Fire.

Boston, Dec. 21, 1751.

Sir,

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

The remarkable effect of lightning on iron, lately discovered, in giving it the magnetic virtue, and the same effect produced on small needles by the electrical fire, is a further and convincing proof that they are both the same element; but, which is very unaccountable, Mr. K. tells me, it is necessary to produce this effect, that the direction of the needle and the electric fire should be north and south; from either to the other, and that just so far as they deviate therefrom, the magnetic power in the needle is less, till their direction being at right angles with the north and south, the effect entirely ceases. We made at Faneuil Hall, where Mr. K — — 's apparatus is, several experiments to give some small needles the magnetic virtue; previously examining, by putting them in water, on which they will be supported, whether or not they had any of that virtue; and I think we found all of them to have some small degree of it, their points turning to the north: we had nothing to do then but to invert the poles, which accordingly was done, by sending through them the charge of two large glass jars; the eye of the needle turning to the north, as the point before had done; that end of the needle which the fire is thrown upon, Mr. K. tells me always points to the north.

The electrical fire passing through air has the same crooked direction as lightning[58]. This appearance I endeavour to account for thus: Air is an electric *per se*, therefore there must be a mutual repulsion betwixt air and the electrical fire. A column or cylinder of air, having the diameter of its base equal to the diameter of the electrical spark, intervenes that part of the body which the spark is taken from, and of the body it aims at. The spark acts upon this column, and is acted upon by it, more strongly than any other neighbouring portion of air.

The column, being thus acted upon, becomes more dense, and, being more dense, repels the spark more strongly; its repellency being in proportion to its density: Having acquired, by being condensed, a degree of repellency greater than its natural, it turns the spark out of its strait course; the neighbouring air, which must be less dense, and therefore 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 phenomenon is supposed to depend, would not repel the spark more strongly than the neighbouring air.

This might be an objection, if the electrical fire was as sluggish and inactive as air. Air takes a sensible time to diffuse itself equally, as is manifest from winds which often blow for a considerable time together from the same point, and with a velocity even in the greatest storms, not exceeding, as it is said, sixty miles an hour: but the electric fire seems propagated instantaneously, taking up no perceptible time in going very great distances. It must then be an inconceivably short time in its progress from an electrified to an unelectrified body, which, in the present case, can be but a few inches apart: but this small portion of time 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 phial to the upper wire will pass through rarefied 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 phenomena 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 subtle and active, would be immediately communicated, either to those lower parts of the sea from which it was drawn, and so only perform quick revolutions; or be communicated to the adjacent islands or continent, and so be diffused instantaneously through the general mass of the earth. I say instantaneously, 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 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.

[57] Mr. Badouin. *Editor.*

[58] This is most easily observed in large strong sparks taken at some inches distance.

Observations on the Subjects of the preceding Letter. — Reasons for supposing the Sea to be the grand source of Lightning. — Reasons for doubting this hypothesis. — Improvement in a Globe for raising the Electric Fire.

TO J. B. AT BOSTON.

READ AT THE Royal Society, May 27, 1756.

Philadelphia, Jan. 24, 1752.

Sir,

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

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

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

Your 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 complete.

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 phial requires a non-electric communication from the wire to every part of the charged glass; otherwise, being dry and clean, and filled with air only, it charges slowly, and discharges gradually, by sparks, without a shock: but, exhausted of air, the communication is so open and free between the inserted wire and surface of the glass, that it charges as readily, and shocks as smartly as if filled with water: and I doubt not, but that in the experiment you propose, the sparks would not only be near strait *in vacuo*, but strike at a greater distance than in the open air, though perhaps there would not be a loud explosion. As soon as I have a little leisure, I will make the experiment, and send you the result.

My supposition, that the sea might possibly be the grand source of lightning, arose from the common observation of its luminous appearance in the night, on the least motion; an appearance never observed in fresh water. Then I knew that the electric fluid may be pumped up out of the earth, by the friction of a glass globe, on a non-electric 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 spherules, capable of touching the salt only in points, and imagined a particle of salt could therefore no more be wet by a particle of water, than a globe by a cushion; that there might therefore be such a friction between these originally constituent particles of salt and water, as in a sea of globes and cushions; that each particle of water on the surface might obtain from the common mass, some particles of the universally diffused, much finer, and more subtle electric fluid, and forming to itself an atmosphere of those particles, be repelled from the then generally electrified surface of the sea, and fly away with them into the air. I thought too, that possibly the great mixture of particles electric *per se*, in the ocean water, might, in some degree, impede the swift motion and dissipation of the electric fluid, through it to the shores, &c. — But having since found, that salt in the water of an electric 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 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 practise: but such pieces are fit only to be seen by friends.

I am, &c.

B. FRANKLIN.

Effect of Lightning on Captain Waddel's Compass, and the Dutch Church at New York.

FROM J. B. ESQ. OF BOSTON, TO BENJAMIN FRANKLIN, ESQ. AT PHILADELPHIA.

READ AT THE Royal Society, June 3, 1756.

Boston, March 2, 1752.

Sir,

I have received your favour of the 24th of January past, inclosing an extract from your letter to Mr. Collinson, and * * * *'s letter to yourself, which I have read with a great deal of pleasure, and am much obliged to you for. Your extract confirms a correction Mr. Kinnersley made a few days ago, of a mistake I was under respecting the polarity given to needles by the electrical fire, "that the end which receives the fire always points north;" and, "that the needle being situated east and west, will not have a 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 situation. As to the needle of Captain 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 Captain Waddel's account.

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

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

The effect which the discharge of your four glass jars had upon a fine wire, tied between two strips of glass, puts me in mind of a very similar one of lightning, that I observed at New-York, October, 1750, a few days after I left Philadelphia. In company with a number of gentlemen, I went to take a view of the city from the Dutch church steeple, in which is a clock about twenty or twenty-five feet below the bell. From the clock went a wire through two floors, to the clock-hammer near the bell, the holes in the floor for the wire being perhaps about a quarter of an inch diameter. We were told, that in the spring of 1750, the lightning struck the clock hammer, and descended along the wire to the clock, melting in its way several spots of the wire, from three to nine inches long, through one-third of its substance, till coming within a few feet of the lower end, it melted the wire quite through, in several places, so that it fell down in several pieces; which spots and pieces we saw. When it got to the end of the wire, it flew off to the hinge of a door, shattered the door, and dissipated. In its passage through the holes of the floors it did not do the least damage, which evidences that wire is a good conductor of lightning (as it is of electricity) provided it be substantial enough, and might, in this case, had it been continued to the earth, have conducted it without damaging the building.[59]

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.

[59] The wire mentioned in this account was re-placed by a small brass chain. In the summer of 1763, the lightning again struck that steeple, and from the clock-hammer near the bell, it pursued the chain as it had before done the wire, went off to the same hinge, and again shattered the same door. In its passage through the same holes of the same floors, it did no damage to the floors, nor to the building during the whole extent of the chain. But the chain itself was destroyed, being partly scattered about in fragments of two or three links melted and stuck together, and partly blown up or reduced to smoke, and dissipated. [See an account of the same effect of lightning on a wire at Newbury, p. 311.] The steeple, when repaired, was guarded by an iron conductor, or rod, extending from the foot of the vane-spindle down the outside of the building, into the earth. The newspapers have mentioned, that in 1765, the lightning fell a third time on the same steeple, and was safely conducted by the rod; but the particulars are not come to hand.

Proposal of an Experiment to measure the Time taken up by an Electric Spark, in moving through any given Space. By J. A.[60] Esq. of New-York.

READ AT THE Royal Society, Dec 26, 1756.

If I remember right, the Royal Society made one experiment to discover the velocity of the electric fire, by a wire of about four miles in length, supported by silk, and by turning it forwards and backwards in a field, so that the beginning and end of the wire were at only the distance of two people, the one holding the Leyden bottle and the beginning of the wire, and the other holding the end of the wire and touching the ring of the bottle; but by this experiment no discovery was made, except that the velocity was extremely quick.

As water is a conductor as well as metals, it is to be considered whether the velocity of the electric fire might not be discovered by means of water; whether a river, or lake, or sea, may not be made part of the circuit through which the electric fire passes? instead of the circuit all of wire, as in the above experiment.

Whether in a river, lake, or sea, the electric fire will not dissipate and not return to the bottle? or, will it proceed in strait lines through the water the shortest courses possible back to the bottle?

If the last, then suppose one brook that falls into Delaware doth head very near to a brook that falls into Schuylkil, and let a wire be stretched and supported as before, from the head of the one brook to the head of the other, and let the one end communicate with the water, and let one person stand in the other brook, holding the Leyden bottle, and let another person hold that end of the wire not in the water, and touch the ring of the bottle. — If the electric fire will go as in the last question, then will it go down the one brook to Delaware or Schuylkill, and down one of them to their meeting, and up the other and the other brook; the time of its doing this may possibly be observable, and the further upwards the brooks are chosen, the more observable it would be.

Should this be not observable, then suppose the two brooks falling into Sasquehana and Delaware, and proceeding as before, the electric fire may, by that means, make a circuit round the North Cape of Virginia, and go many hundreds of miles, and in doing that, it would seem it must take some observable time.

If still no observable time is found in that experiment, then suppose the brooks falling the one into the Ohio, and the other into Sasquehana, or Potomack, in that the electric fire would have a circuit of some thousands of miles to go down Ohio to 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.

READ AT THE Royal Society, Dec. 25, 1756.

Suppose a tube of any length open at both ends, and containing a moveable wire of just the same length, that fills its bore. If I attempt to introduce the end of another wire into the same tube, it must be done by pushing forward the wire it already contains; and the instant I press and move one end of that wire, the other end is also moved; and in introducing one inch of the same wire, I extrude, at the same time, an inch of the first, from the other end of the tube.

If the tube be filled with water, and I inject an additional inch of water at one end, I force out an equal quantity at the other, in the very same instant.

And the water forced out at one end of the tube is not the very same water that was forced in at the other end at the same time, it was only in motion at the same time.

The long wire, made use of in the experiment to discover the velocity of the electric fluid, is itself filled with what we call its natural quantity of that fluid, before the hook of the Leyden bottle is applied to one end of it.

The outside of the bottle being at the time of such application in contact with the other end of the wire, the whole 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 Susquehannah, or Potowmack, and Ohio, would not afford the satisfaction desired, though we could be sure that the motion of the electric fluid would be in that tract, and not under ground in the wet earth by the shortest way.

B. FRANKLIN.

[60] James Alexander. *Editor.*

Experiments on boiling Water, and Glass heated by boiling Water. — Doctrine of Repulsion in electrised Bodies doubted. — Electricity of the Atmosphere at different Heights. — Electrical Horse-race. — Electrical Thermometer. — In what Cases the electrical Fire produces Heat. — Wire lengthened by Electricity. — Good Effect of a Rod on the House of Mr. West, of Philadelphia.

FROM MR. KINNERSLEY TO B. FRANKLIN, ESQ.

Philadelphia, March 12, 1761.

Sir,

Having lately made the following experiments, I very chearfully communicate them, in hopes of giving you some degree of pleasure, and exciting you to further explore your favorite, but not quite exhausted subject, *electricity*.

I placed myself on an electric stand, and, being well electrised, threw my hat to an unelectrised person, at a considerable distance, on another stand, and found that the hat carried some of the electricity with it; for, upon going immediately to the person who received it, and holding a flaxen thread near him, I perceived he was electrised sufficiently to attract the thread.

I then suspended, by silk, a broad plate of metal, and electrised some boiling water under it at about four feet distance, expecting that the vapour, which ascended plentifully to the plate, would, upon the principle of the foregoing experiment, carry up some of the electricity with it; but was at length fully convinced, by several repeated trials, that it left all its share thereof behind. This I know not how to account for; but does it not seem to corroborate your 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 centre of an electric stand. Then, electrising the stand, I had the pleasure of seeing what I expected; the wooden needle turned round, carrying the pins with their heads foremost. I then electrised the stand negatively, expecting the needle to turn the contrary way, but was extremely disappointed, for it went still the same way as before. When the stand was electrised positively, I suppose that the natural quantity of electricity in the air being increased on one side, by what issued from the points, the needle was attracted by the lesser quantity on the other side. When electrised negatively, I suppose that the natural quantity of electricity in the air was diminished near the points; in consequence whereof, the equilibrium being destroyed, the needle was attracted by the greater quantity on the opposite side.

The doctrine of repulsion, in electrised bodies, I begin to be somewhat doubtful of. I think all the phenomena on which it is founded, may be well enough accounted for without it. Will not cork balls, electrised negatively, separate as far as when electrised positively? And may not their separation in both cases be accounted for upon the same principle, namely, the mutual attraction of the natural quantity in the air, and that which is denser or rarer in the cork balls? it being one of the established laws of this fluid, that quantities of different densities shall mutually attract each other, in order to restore the equilibrium.

I can see no reason to conclude that the air has not its share of the common stock of electricity, as well as glass, and perhaps, all other electrics *per se*. For though the air will admit bodies to be electrised in it either positively or negatively, and will not readily carry off the redundancy in the one case, or supply the deficiency in the other, yet let a person in the negative state, out of doors in the dark, when the air is dry, hold, with his arm extended, a long sharp needle, pointing upwards, and he will soon be convinced that electricity may be drawn out of the air; not very plentifully, for, being a bad conductor, it seems loth to part with it, but yet some will evidently be collected. The air near the person's body, having less than its natural quantity, will have none to spare; but, his arm being extended, as above, some will be collected from the remoter air, and will appear luminous, as it converges to the point of the needle.

Let a person electrised negatively present the point of a needle, horizontally, to a cork ball, suspended by silk, and the ball will be attracted towards the point, till it has parted with so much of its natural quantity of electricity as to be in the negative state in the same degree with the person who holds the needle; then it will recede from the point, being, as I suppose, attracted the contrary way by the electricity of greater density in the air behind it. But, as this opinion seems to deviate from electrical orthodoxy, I should be glad to see these phenomena 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 electrised positively. The kite had three metalline points fixed to it: one on the top, and one on each side. That the twine was electrised, appeared by the separating of two small cork balls, suspended on the twine by fine flaxen threads, just above where the silk was tied to it, and sheltered from the wind. That the twine was electrised positively, was proved, by applying to it the wire of a charged bottle, which caused the balls to separate further, without first coming nearer together. This experiment showed, 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 electrised point of a pair of spurs, I was entertained with an electrical horse-race.

I have contrived an electrical air thermometer, and made several experiments with it, that have afforded me much satisfaction and pleasure. It is extremely sensible of any alteration in the state of the included air, and fully determines that controverted point, Whether there be any heat in the electric fire? By the enclosed draught, and the following description, you will readily apprehend the construction of it. (See Plate II.)

A B is a glass tube, about eleven inches long, and one inch diameter in the bore. It has a brass ferrule cemented on each end, with a top and bottom part, C and D, to be screwed on, air-tight, and taken off at pleasure. In the centre of the bottom part D, is a male screw, which goes into a brass nut, in the mahogany pedestal E. The wires F and G are for the electric fire to pass through, darting from one to the other. The wire G extends through the pedestal to H, and may be raised and lowered by means of a male screw on it. The wire F may be taken out, and the hook I be screwed into its place. K is a glass tube, with a small bore, open at both ends, cemented in the brass tube L which screws into the top part C. The lower end of the tube K is immersed in water, coloured with cochineal, at the bottom of the tube A B. (I used, at first, coloured spirits of wine, but in one experiment I made, it took fire.) On the top of the tube K is cemented, for ornament, a brass ferrule, with a head screwed on it, which has a small air-hole through its side, at *a*. The wire *b*, is a small round spring, that embraces the tube K, so as to stay wherever it is placed. The weight M is to keep strait whatever may be suspended in the tube A B, on the hook I. Air must be blown through the tube K, into the tube A B, till enough is intruded to raise, by its elastic force, a column of the coloured water in the tube K, up to *c*, or thereabouts; and then, the gage-wire *b*, being slipt down to the top of the column, the thermometer is ready for use.

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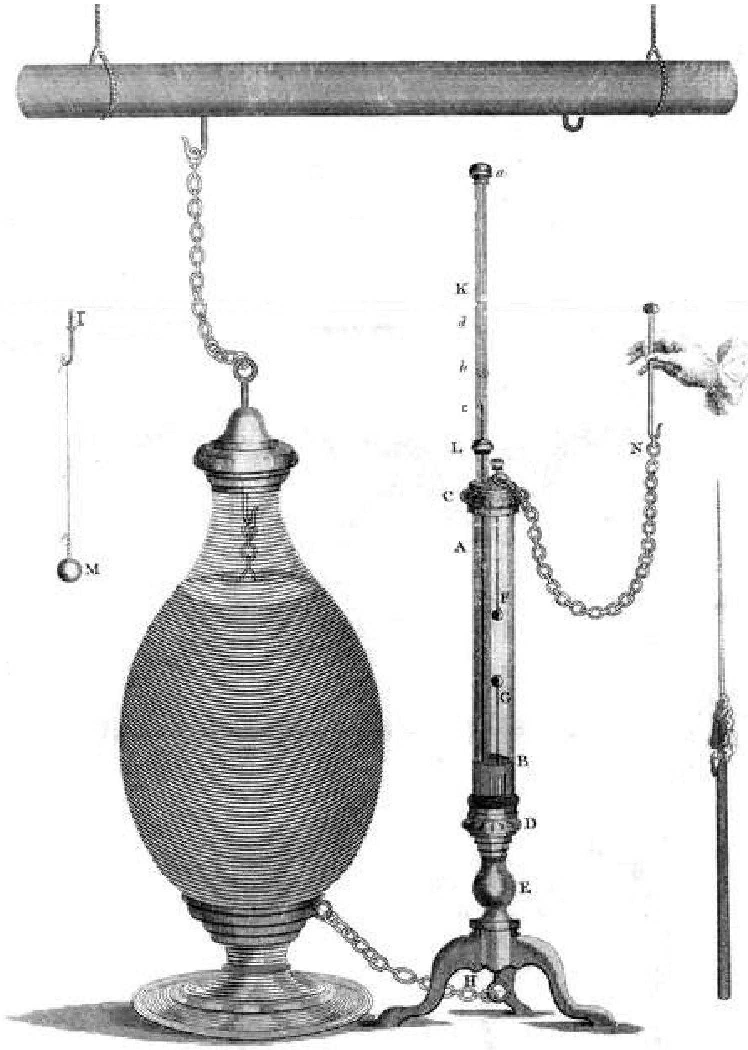
Plate II.



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I set the thermometer on an electric stand, with the chain N fixed to the prime conductor, and kept it well electrised a considerable time; but this produced no sensible effect; which shews, that the electric fire, when in a state of rest, has no more heat than the air, and other matter wherein it resides.

When the wires F and G are in contact, a large charge of electricity sent through them, even that of my case of five and thirty bottles, containing above thirty square feet of coated glass, will produce no rarefaction of the air included in the tube A B; which shows 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 shows, 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 about five gallons and a half, wine measure) darting from wire to wire, will, by the disturbance it gives the air, repelling it in all directions, raise the column in the tube K, up to *d*, or thereabouts; and the charge of the above-mentioned case of bottles will raise it to the top of the tube. Upon the air's coalescing, the column, by its gravity, instantly subsides, till it is in equilibrio with the rarefied air; it then gradually descends as the air cools, and settles where it stood before. By carefully observing at what height above the gage-wire *b*, the descending column first stops, the degree of rarefaction is discovered, which, in great explosions, is very considerable.

I hung in the thermometer, successively, a strip of wet writing paper, a wet flaxen and 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 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 lightning, by its rapid motion, produces heat in *itself*; as well as in other bodies (and that it does I think is evident from some of the foregoing experiments made with the thermometer) then its sometimes singeing the hair of animals killed by it, may easily be accounted for. And the reason of its not always doing so, may, perhaps, be this: The quantity, though sufficient to kill a large animal, may sometimes not be great enough, or not have met with resistance enough, to become, by its motion, burning hot.

We find that dwelling-houses, struck with lightning, are seldom set on fire by it; but when it passes through barns, with hay or straw in them, or store-houses, containing large quantities of hemp, or such like matter, they seldom, if ever, escape a conflagration; which may, perhaps, be owing to such combustibles being apt to kindle with a less degree of heat than is necessary to kindle wood.

We had four houses in this city, and a vessel at one of the wharfs, struck and damaged by lightning last summer. One of the houses was struck twice in the same storm. But I have the pleasure to inform you, that your method of 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 to be discovered in the conductor, as far as we could examine it below, I proposed to Mr. West our going to the top of the house, to examine the pointed rod, assuring him, that if the lightning had passed through it, the point must have been melted; and, to our great satisfaction, we found it so. This iron rod extended in height about nine feet and a half above a stack of chimneys to which it was fixed (though I suppose three or four feet would have been sufficient.) It was somewhat more than half an inch diameter in the thickest part, and tapering to the upper end. The conductor, from the lower end of it to the earth, consisted of square iron nail-rods, not much above a quarter of an inch thick, connected together by interlinking joints. It extended down the cedar roof to the eaves, and from thence down the wall of the house, four story and a half, to the pavement in Water-street, being fastened to the wall, in several places, by small iron hooks. The lower end was fixed to a ring, in the top of an iron stake that was drove about four or five feet into the ground.

The above-mentioned iron rod had a hole in the top of it, about two inches deep, wherein was inserted a brass wire, about two lines thick, and, when first put there, about ten inches long, terminating in a very acute point; but now its whole length was no more than seven inches and a half, and the top very blunt. Some of the metal appears to be missing, the slenderest part of the wire being, as I suspect, consumed into smoke. But some of it, where the wire was a little thicker, being only melted by the lightning, sunk down, while in a fluid state, and formed a rough irregular cap, lower on one side than the other, round the upper end of what remained, and became intimately united therewith.

This was all the damage that Mr. West sustained by a terrible stroke of lightning; — a most convincing proof of the great utility of this method of preventing its dreadful effects. Surely it will now be thought as expedient to provide conductors for the lightning, as for the rain.

Mr. West was so good as to make me a present of the melted wire, which I keep as a great curiosity, and long for the pleasure of shewing it to you. In the mean time, I beg your acceptance of the best representation I can give of it, which you will find by the side of the thermometer, drawn in its full dimensions as it now appears. The dotted lines above are intended to shew the form of the wire before the lightning melted it.

And now, Sir, I most heartily congratulate you on the pleasure you must have in finding your great and well-grounded expectations so far fulfilled. May this method of security from the destructive violence of one of the most awful powers of nature, meet with such further success, as to induce every good and grateful heart to bless God for the important discovery! May the benefit thereof be diffused over the whole globe! May it extend to the latest posterity of mankind, and make the name of FRANKLIN, like that of NEWTON, *immortal*.

I am, Sir, with sincere respect,

Your most obedient and most humble servant,

EBEN. KINNERSLEY.

Answer to some of the foregoing Subjects. — How long the Leyden Bottle may be kept charged. — Heated Glass rendered permeable by the electric Fluid. — Electrical Attraction and Repulsion. — Reply to other Subjects in the preceding Paper. — Numerous Ways of kindling Fire. — Explosion of Water. — Knobs and Points.

TO MR. KINNERSLEY.

London, Feb. 20, 1762.

Sir,

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 electrised water, reminds me of one I formerly made, to try if negative electricity might be produced by evaporation only. I placed a large heated brass plate, containing four or five square feet on an electric stand; a rod of metal, about four feet long, with a bullet at its end, extended from the plate horizontally. A light lock of cotton, suspended a fine thread from the cieling, hung opposite to, and within an inch of the bullet. I then sprinkled the heated plate with water, which arose fast from it in vapour. If vapour should be disposed to carry off the electrical, as it does the common fire from bodies, I expected the plate would, by losing some of its natural quantity, become negatively electrised. But I could not perceive, by any motion in the cotton, that it was at all affected: nor by any separation of small cork-balls suspended from the plate, could it be observed that the plate was in any manner electrified.

Mr. Canton here has also found, that two tea-cups, set on electric stands, and filled, one with boiling, the other with cold water, and equally electrified, continued equally so, notwithstanding the plentiful evaporation from the hot water. Your experiment and his agreeing, show another remarkable difference between electric and common fire. For the latter quits most readily the body that contains it, where water, or any other fluid, is evaporating from the surface of that body, and escapes with the vapour. Hence the method, long in use in the east, of cooling liquors, by wrapping the bottles round with a wet cloth, and exposing them to the wind. Dr. Cullen, of Edinburgh, has given some experiments of cooling by evaporation; and I was present at one made by Dr. Hadley, then professor of chemistry at Cambridge, when, by repeatedly wetting the ball of a thermometer with spirit, and quickening the evaporation by the blast of a bellows, the mercury fell from 65, the state of warmth in the common air, to 7, which is 22 degrees below freezing; and, accordingly, from some water mixed with the spirit, or from the breath of the assistants, or both, ice gathered in small spicula round the ball, to the thickness of near a quarter of an inch. To such a degree did the mercury lose the fire it before contained, which, as I imagine, took the opportunity of escaping, in company with the evaporating particles of the spirit, by adhering to those particles.

Your experiment of the Florence flask, and boiling water, is very curious. I have repeated it, and found it to succeed as you describe it, in two flasks out of three. The third would not charge when filled with either hot or cold water. I repeated it, because I remembered I had once attempted to make an electric bottle of a Florence flask, filled with cold water, but could not charge it at all; which I then imputed to some imperceptible cracks in the small, extremely thin bubbles, of which that glass is full, and I concluded none of that kind would do. But you have shown 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 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 subtle fluid might, by slow imperceptible degrees, soak through the glass, and in time escape, I requested some of my friends, who had conveniences for doing it, to make trial, whether, after some months, the charge of a bottle so sealed would be sensibly diminished. Being at Birmingham, in September, 1760, Mr. Bolton of that place opened a bottle that had been charged, and its long tube neck hermetically sealed in the January preceding. On breaking off the end of the neck, and introducing a wire into it, we found it possessed of a considerable quantity of electricity, which was discharged by a snap and spark. This bottle had lain near seven months on a shelf, in a closet, in contact with bodies that would undoubtedly have carried off all its electricity, if it could have come readily through the glass. Yet as the quantity manifested by the discharge was not apparently so great as might have been expected from a bottle of that size well charged, some doubt remained whether part had escaped while the neck was sealing, or had since, by degrees, soaked through the glass. But an experiment of Mr. Canton's, in which such a bottle was kept under water a week, without having its electricity in the least impaired, seems to show, that when the glass is cold, though extremely thin, the electric fluid is well retained by it. As that ingenious and accurate experimenter made a discovery, like yours, of the effect of heat in rendering thin glass permeable by that fluid, it is but doing him justice to give you his account of it, in his own words, extracted from his letter to me, in which he communicated it, dated Oct. 31, 1760, *viz*.

"Having procured some thin glass balls, of about an inch and a half in diameter, with stems, or tubes, of eight or nine inches in length, I electrified them, some positively on the inside, and others negatively, after the manner of charging the Leyden bottle, and sealed them hermetically. Soon after I applied the naked balls to my electrometer, and could not discover the least sign of their being electrical, but holding them, before the fire, at the distance of six or eight inches, they became strongly electrical in a very short time, and more so when they were cooling. These balls will, every time they are heated, give the electrical fluid to, or take it from other bodies, according to the *plus* or *minus* state of it within them. Heating them frequently, I find will sensibly diminish their power; but keeping 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 phial, 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 phial, 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 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* III.) 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 phial.

“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.”

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Plate III.

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, shows 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, 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. 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 would be solved by it. But I find, or think I find, the two causes more convenient than one of them alone. Thus I might solve the circular motion of your horizontal stick, supported on a pivot, with two pins at their ends, pointing contrary ways, and moving in the same direction when electrified, whether positively or negatively: when positively, the air opposite to the points being electrised positively, repels the points; when negatively, the air opposite the points being also, by their means, electrised 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 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 in particular circumstances) throughout all other matter. But this you jokingly call “electrical orthodoxy.” It is so with some at present, but not with all; and, perhaps, it may not always be orthodoxy with any body. Opinions are continually varying, where we cannot have mathematical evidence of the nature of things; and they must vary. Nor is that variation without its use, since it occasions a more thorough discussion, whereby error is often dissipated, true knowledge is encreased, and its principles become better understood and more firmly established.

Air should have, as you observe, “its share of the common stock of electricity, as well as glass, and, perhaps, all other electrics *per se*.” But I suppose, that, like them, it does not easily part with what it has, or receive more, unless when 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 electrised person, by extending his arm upwards into the air, with a needle between his fingers, on the point of which light may be seen in the night, is, indeed, a curious one. In this town the air is generally moister than with us, and here I have seen Mr. Canton electrify the air in one room positively, and in another, which communicated by a door, he has electrised the air negatively. The difference was easily discovered by his cork balls, as he passed out of one room into another. — Pere Beccaria, too, has a pretty experiment, which shows that air may be electrised. Suspending a pair of small light balls, by flaxen threads, to the end of his prime conductor, he turns his globe some time, electrising positively, the balls diverging and continuing separate all the time. Then he presents the point of a needle to his conductor, which gradually drawing off the electric fluid, the balls approach each other, and touch, before all is drawn from the conductor; opening again as more is drawn off, and separating nearly as wide as at first, when the conductor is reduced to the natural state. By this it appears, that when the balls came together, the air surrounding the balls was just as much electrised as the conductor at that time; and more than the conductor, when that was reduced to its natural state. For the balls, though in the natural state, will diverge, when the air that surrounds them is electrised *plus* or *minus*, as well as when that is in its natural state and they are electrised *plus* or *minus* themselves. I foresee that you will apply this experiment to the support of your hypothesis, and I think you may make a good deal of it.

It was a curious enquiry of yours, Whether the electricity of the air, in clear dry weather, be of the same density at the height of two or three hundred yards, as near the surface of the earth; and I am glad you made the experiment. Upon reflection, it should seem probable, that whether the general state of the atmosphere at any time be positive or negative, that part of it which is next the earth will be nearer the natural state, by having given to the earth in one case, or having received from it in the other. In electrising the air of a room, that which is nearest the walls, or floor, is least altered. There is only one small ambiguity in the experiment, which may be cleared by more trials; it arises from the supposition that bodies may be electrised positively by the friction of air blowing strongly on them, as it does on the kite and its string. If at some times the electricity appears to be negative, as that friction is the same, the effect must be from a negative state of the upper air.

I am much pleased with your electrical thermometer, and the experiments you have made with it. I formerly satisfied myself by an experiment with my phial and syphon, that the elasticity of the air was not increased by the mere existence of an electric atmosphere within the phial; but I did not know, till you now inform me, that heat may be given to it by an electric explosion. The continuance of its rarefaction, for some time after the discharge of your glass jar and of your case of bottles, seem to make this clear. The other experiments on wet paper, wet thread, green grass, and green wood, are not so satisfactory; as possibly the reducing part of the moisture to vapour, by the electric fluid passing through it, might occasion some expansion which would be gradually reduced by the condensation of such vapour. The fine silver thread, the very small brass wire, and the strip of gilt paper, are also subject to a similar objection, as even metals, in such circumstances, are often partly reduced to smoke, particularly the gilding on paper.

But your subsequent beautiful experiment on the wire, which you made hot by the electric explosion, and in that state fired gunpowder with it, puts it out of all question, that heat is produced by our artificial electricity, and that the melting of metals in that way, is not by what I formerly called a cold fusion. A late instance here, of the melting a bell-wire, in a house struck by lightning, and parts of the wire burning holes in the floor on which they fell, has proved the same with regard to the electricity of nature. I was too easily led into that error by accounts given, even in philosophical books, and from remote ages downwards, of melting money in purses, swords in scabbards, &c. without burning the inflammable matters that were so near those melted metals. But men are, in 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 a 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 singe, melt, or burn them, whenever it is, by any means, set at liberty, so that it may exert itself upon them, or be disengaged from them. This liberty seems to be afforded it by the passage of electricity through them, which we know can and does, of itself, separate the parts even of water; and perhaps the immediate appearances of fire are only the effects of such separations? If so, there would be no need of supposing that the electric fluid *heats itself* by the swiftness of its motion, or heats bodies by the resistance it meets with in passing through them. They would only be heated in proportion as such separation could be more easily made. Thus a melting heat cannot be given to a large wire in the flame of a candle, though it may to a small one; and this not because the large wire resists *less* that action of the flame which tends to separate its parts, but because it resists it *more* than the smaller wire; or because the force being divided among more parts acts weaker on each.

This reminds me, however, of a little experiment I have frequently made, that shows, at one operation, the different effects of the same quantity of electric fluid passing through different quantities of metal. A strip of tinfoil, three inches long, a quarter of an inch wide at one end, and tapering all the way to a sharp point at the other, fixed between two pieces of glass, and having the electricity of a large glass jar sent through it, will not be discomposed in the broadest part; towards the middle will appear melted in spots; where narrower, it will be quite melted; and about half an inch of it next the point will be reduced to smoke.

You were not mistaken in supposing that your account of the effect of the pointed rod, in securing Mr. West's house from damage by a stroke of lightning, would give me great pleasure. I thank you for it most heartily, and for the pains you have taken in giving me so complete a description of its situation, form, and substance, with the draft of the melted point. There is one circumstance, viz. that the lightning was seen to diffuse itself from the foot of the rod over the wet pavement, which seems, I think, to indicate, that the earth under the pavement was very dry, and that the rod should have been sunk deeper, till it came to earth moister, and therefore apter to receive and dissipate the electric fluid. And although, in this instance, a conductor formed of nail rods, not much above a quarter of an inch thick, served well to convey the lightning, yet some accounts I have seen from Carolina, give reason to think, that larger may be sometimes necessary, at least for the security of the conductor itself, which, when too small, may be destroyed in executing its office, though it does, at the same time, preserve the house. Indeed, in the construction of an instrument so new, and of which we could have so little experience, it is rather lucky that we should at first be so near the truth as we seem to be, and commit so few errors.

There is another reason for sinking deeper the lower end of the rod, and also for turning it outwards under ground to some distance from the foundation; it is this, that water dripping from the eaves falls near the foundation, and sometimes soaks down there in greater quantities, so as to come near the end of the rod, though the ground about it be drier. In such case, this water may be exploded, that is, blown into vapour, whereby a force is generated, that may damage the foundation. Water reduced to vapour, is said to occupy 14,000 times its former space. I have sent a charge through a small glass tube, that has borne it well while empty, but when filled first with water, was shattered to pieces and driven all about the room: — Finding no part of the water on the table, I suspected it to have been reduced to vapour; and was confirmed in that suspicion afterwards, when I had filled a like piece of tube with ink, and laid it on a sheet of clean paper, whereon, after the explosion, I could find neither any moisture nor any sully from the ink. This experiment of the explosion of water, which I believe was first made by that most ingenious electrician, father Beccaria, may account for what we sometimes see in a tree struck by lightning, when part of it is reduced to fine splinters like a broom; the sap vessels being so many tubes containing a watry fluid, which, when reduced to vapour, rends every tube lengthways. And perhaps it is this rarefaction of the fluids in animal bodies killed by lightning or electricity, that, by separating its fibres, renders the flesh so tender, and apt so much sooner to putrify. I think too, that much of the damage done by lightning to stone and brick-walls may sometimes be owing to the explosion of water, found, during showers, running or lodging in the joints or small cavities or cracks that happen to be in the walls.

Here are some electricians that recommend knobs instead of points on the upper end of the rods, from a supposition that the points invite the stroke. It is true that points draw electricity at greater distances in the gradual silent way; but knobs will draw at the greatest distance a stroke. There is an experiment that will settle this. Take a crooked wire of the thickness of a quill, and of such a length as that one end of it being applied to the lower part of a charged bottle, the upper may be brought near the ball on the top of the wire that is in the bottle. Let one end of this wire be furnished with a knob, and the other may be gradually tapered to a fine point. When the point is presented to discharge the bottle, it must be brought much nearer before it will receive the stroke, than the knob requires to be. Points besides tend to repel the fragments of an electrised cloud, knobs draw them nearer. An experiment, which I believe I have shewn you, of cotton fleece hanging from an electrised body, shows this clearly when a point or a knob is presented under it.

You seem to think highly of the importance of this discovery, as do many others on our side of the water. Here it is very little regarded; so little, that though it is now seven or eight years since it was made public, I have not heard of a single house as yet attempted to be secured by it. It is true the mischiefs done by lightning are not so frequent here as with us, and those who calculate chances may perhaps find that not one death (or the destruction of one house) in a hundred thousand happens from that cause, and that therefore it is scarce worth while to be at any expence to guard against it. — But in all countries there are particular situations of buildings more exposed than others to such accidents, and there are minds so strongly impressed with the apprehension of them, as to be very unhappy every time a little thunder is within their hearing; — it may therefore be well to render this little piece of new knowledge as general and as well understood as possible, since to make us *safe* is not all its advantage, it is some to make us *easy*. And as the stroke it secures us from might have chanced perhaps but once in our lives, while it may relieve us a hundred times from those painful apprehensions, the latter may possibly on the whole contribute more to the happiness of mankind than the former.

Your kind wishes and congratulations are very obliging. I return them cordially; — being, with great regard and esteem,

My dear Sir,

Your affectionate friend,

And most obedient humble servant,

B. FRANKLIN.

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.

CHARLESTOWN, 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[61]. There the lightning made a hole through the wall or back of the chimney, to get to the gun-barrel[62], 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." ——

[61] A proof that it was not of sufficient substance to conduct with safety to itself (though with safety *so far* to the wall) so large a quantity of the electric fluid.

[62] A more substantial conductor.

Mr. William Maine's Account of the Effects of the 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 rivetted 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 curtain-rod, composed of several joints, annexed by hooks turned at the ends of 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 nut, and I am persuaded, melted the points, nut, 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 solder. 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 solder. — 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 soot with it; and filled the whole house with its flash (*l*), smoke, and dust. It tore up the hearth in several places (*m*), and broke some pieces of china in the beaufet (*n*). A copper tea-kettle standing in the chimney was beat together, as if some great weight had fallen upon it (*o*); and three holes, each about half an inch diameter, melted through the bottom (*p*). What seems to me the 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 logger-head, an Indian pot, an earthen cup, and a cat, were all in the chimney at the time unhurt, though a 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 BY BENJAMIN FRANKLIN.

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* 283, 310.) 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 shows, that a finger under it, being a blunt body, extends the cotton, drawing its lower part downwards; when a needle, with its point presented to the cotton, makes it fly up again to the prime conductor; and that this effect is strongest when as much of the needle as possible appears above the end of the finger; grows weaker as the needle is shortened between the finger and thumb; and is reduced to nothing when only a short part below the point appears above the finger. Now it seems the points of Mr. Maine's rod were elevated only (*a*) *six or seven inches above the top of the chimney*; which, considering the bulk of the chimney and the house, was too small an elevation. For the great body of matter near them would hinder their being easily brought into a negative state by the repulsive power of the electrised cloud, in which negative state it is that they attract most strongly and copiously the electric fluid from other bodies, and convey it into the earth.

(*b*) *Nothing of the points, &c. could be found.* This is a common effect. (*See page* 312.) 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 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 showing, that the second and principal intention of the rods is obtainable, viz. that of *conducting* the lightning. In all the instances yet known of the lightning's falling on any house guarded by rods, it has pitched down upon the point of the rod, and has not fallen upon any other part of the house. Had the lightning fallen on this chimney, unfurnished with a rod, it would probably have rent it from top to bottom, as we see, by the effects of the lightning on the points and rod, that its quantity was very great; and we know that many chimneys have been so demolished. But *no part of this was damaged, only* (*f*)(*g*)(*h*) *at the foundation, where it was shattered and several bricks torn out*. Here we learn the principal defect in fixing this rod. The lower joint being sunk but three feet into the earth, did not it seems go low enough to come at water, or a large body of earth so moist as to receive readily from its end the quantity it conducted. The electric fluid therefore, thus accumulated near the lower end of the rod, quitted it at the surface of the earth, dividing in search of other passages. Part of it tore up the surface in furrows, and made holes in it: part entered the bricks of the foundation, which being near the earth are generally moist, and, in exploding that moisture, shattered them. (*See page* 358.) Part went through or under the foundation, and got under the hearth, blowing up great part of the bricks (*m*)(*s*), and producing the other effects (*o*)(*p*)(*q*)(*r*). The iron dogs, loggerhead and iron pot were not hurt, being of sufficient substance, and they probably protected the cat. The copper tea-kettle being thin suffered some damage. Perhaps, 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 plaster and bricks. The shake, from the explosion on the rod, was sufficient to shake down a good deal of loose soot. Lightning does not usually enter houses by the doors, windows, or chimneys, as open passages, in the manner that air enters them: its nature is, to be attracted by substances, that are conductors of electricity; it penetrates and passes *in* them, and, if they are not good conductors as are neither wood, brick, stone nor plaster, 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* (*l*). 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 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.

On the Electricity of the Tourmalin.

TO DR. H[63]. AT LONDON.

CRAVEN-STREET, JUNE 7, 1759.

Sir,

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 parts more easily supplied by the positive. Perhaps the lapidaries who have hitherto cut these stones, had no regard to the situation of the two powers, but chose to make the faces of the stone where they could obtain the greatest breadth, or some other advantage in the form. If any of these 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 side 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 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 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 stone, when heated in water, is positive, and the high side negative; the side A of the large stone positive, and the side B negative.

The effect was apparently the same as would have been produced, if one magnet had been suspended by a thread, and the different poles of another brought alternately near it.

I find that the face A, of the large stone, being coated with leaf-gold (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. FRANKLIN.

[63] Dr. Heberden. *Editor.*

New Observation relating to Electricity in the Atmosphere.

FROM PROFESSOR WINTHROP, TO B. FRANKLIN.

Cambridge, N. E. Sept. 29, 1762.

Sir,

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

I am, Sir, &c.

FROM MR. A. S[64]. TO B. F.

Flash of Lightning that struck St. Bride's Steeple.

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 strait rod of fire, moving at a very sharp angle with the horizon. It appeared to my eye as about two inches diameter, and had nothing of the zig-zag lightning motion. I instantly told a person sitting with me, that some 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. Clements'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

Your's, A. S.

[64] Mr. Alexander Small. *Editor.*

Best Method of securing a Powder Magazine from Lightning.

TO MR. P. F[65]. 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 far from it, which may reach fifteen or twenty 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 spoiled 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 is another thing very proper to line small barrels with; it is what they call tin-foil, or leaf-tin, being tin milled 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 eighteen 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. FRANKLIN.

[65] Peter Franklin. *Editor.*

Of Lightning, and the Methods (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 subtile fluid, penetrating other bodies, and subsisting in them, equally diffused.

When by any operation of art or nature, there happens to be a greater proportion of this fluid in one body than in another, the body which has most will communicate to that which has least, till the proportion becomes equal; provided the distance between them be not too great; or, if it is too great, till there be proper conductors to convey it from one to the other.

If 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 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 watery fluids. And in passing through the house it follows the direction of these conductors, taking as many in its way as can assist it in its passage, whether in a strait, or crooked line leaping from one to the other, if not far distant from each other, only rending the wall in the spaces where these partial good conductors are too distant from each other.

An iron rod being placed on the outside of a building, from the highest part continued down into the moist earth, in any direction strait or crooked, following the form of the roof or other parts of the building, will receive the lightning at its upper end, attracting it so as to prevent its striking any other part; and, affording it a good conveyance into the earth, will prevent its damaging any part of the building.

A small quantity of metal is found able to conduct a great quantity of this fluid. A wire no bigger than a goose-quill has been known to conduct (with safety to the building as far as the wire was continued) a quantity of lightning that did prodigious damage both above and below 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, 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 can have in any room whatever; and what indeed may be deemed quite free from danger of any stroke by lightning.

B. FRANKLIN.

Paris, Sept. 1767.

St. Bride's Steeple. — Utility of Electrical Conductors to Steeples. — Singular kind of Glass tube.

FROM J. W.[66] 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, 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.[67] He cautions people not to ring the church bells during a thunder-storm, lest the lightning, in its way to the earth, should be conducted down to them by the bell ropes,[68] 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[69], in expectation that the sound of such blessed bells would drive away those storms, and secure our buildings from the stroke of lightning; and during so long a period, it has not been found by experience, that places within the reach of such blessed sound, are safer than others where it is never heard; but that on the contrary, the lightning seems to strike steeples of choice, and that at the very time the bells are ringing[70]; 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 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 subtle invisible vapour continually rising from the water, and extremely rarefiable 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 surprise 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 shown me by Mr. Nairne. There is something in it similar to the old observation, I think mentioned by Aristotle, that the bottom of a boiling pot is not warm; and perhaps it may help to explain that fact; — if indeed it be a fact. — When the water stands at an equal height in both these balls, and all at rest; if you wet one of the balls by means of a feather dipt in spirit, though that spirit is of the same temperament as to heat and cold with the water in the glasses, yet the cold occasioned by the evaporation of the spirit from the wetted ball will so condense the vapour over the water contained in that ball, as that the water of the other ball will be pressed up into it, followed by a succession of bubbles, till the spirit is all dried away. Perhaps the observations on these little instruments may suggest and be applied to some beneficial uses. It has been thought, that water reduced to vapour by heat was rarefied 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 rarefied from water is capable of being itself still farther rarefied 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. FRANKLIN.

[66] John Winthrop. *Editor.*

[67] Notre curiosité pourroit peut-être s'applandir des recherches qu'elle nous a fait faire sur la nature du tonnerre, & sur la mécanisme de ses principaux effets, mais ce n'est point ce qu'il y a de plus important; il vaudroit bien mieux que nous puissions tronver quelque moyen de nous en garantir: on y a pensé; on s'est même flatté d'avoir fait cette grande découverte; mais malheureusement douze années d'épreuves & un peu de réflexion, nous apprennent qu'il ne faut pas compter sur les promesses qu'on nous a faites. Je l'ai dit, il y a long temps, and avec regret, toutes ces pointes de fer qu'on dresse en l'air, soit comme *électroscopes*, soit comme préservatifs, —— sont plus propre à nous attirer le feu du tonnerre qu'à nous en préserver; —— & je persiste â dire que le projet d'épuiser une nuée orageuse du feu dont elle est chargée, n'est pas celui d'un physicien, —— . *Memoire sur les Effets du Tonnerre.*

[68] Les cloches, en vertu de leur bénédiction, doivent écarter les orages & nous preserver 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 sais si le son, considéré physiquement, est capable ou non de faire crever une nuée, & de causer l'épanchement de son feu vers les objets terrestres, mais il est certain & prouvé par l'expérience, que la tonnerre peut tomber sur un clocher, soit que l'on y sonne ou que l'on n'y sonne point; & si cela arrive dans le premier cas, les sonneurs sont en grand danger, parcequ'ils tiennent des cordes par lesquelles la commotion de la foudre peut se communiquer jusq'à eux: il est donc plus sage de laisser les cloches en repos quand l'orage est arrivé au-dessus de l'église. Ibid.

[69] Suivant le rituel de Paris, lorsqu'on benit des cloches, on recite les oraisons suivantes:

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

Deus, qui per beatum Moïsen, &c. ... procul pellentur insidiæ inimici, fragor grandinum, procella turbinum, impetus tempestatum, temperentur infesta tonitrua. &c.

Omnipotens sempiterne Deus, &c. ... ut ante sonitum ejus effugentur ignita jacula inimici, percussio fulminum, impetus lapidum, læsio tempestatum, &c.

[70] En 1718. M. Deslandes fit savoir à l'Academie Royale des sciences, que la nuit du 14 ou 15 d'Avril de la mème année, le tonnerre étoit tombé sur vingtquatre églises, dequis 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 ou l'on ne sonnoit pas: que dans celle de Gouisnon, qui fut entièrement ruinée, le tonnerre tua deux personnes de quatre qui sonnoient, &c. *Hist. du l'Ac. R. des Sci. 1719.*

Experiments, Observations, and Facts, tending to support the Opinion of the Utility of long pointed Rods, for securing Buildings from Damage by Strokes of Lightning.

READ AT THE Committee appointed to consider the erecting Conductors to secure the Magazines at Purfleet, Aug. 27, 1772.

EXPERIMENT I.

The prime conductor of an electric machine, A. B. (*See Plate* IV.) being supported about 10 inches and a half above the table by a wax-stand, and under it erected a *pointed wire* 7 inches and a half high, and one-fifth of an inch thick, and tapering to a sharp point, and communicating with the table; when the *point* (being uppermost) is *covered* by the end of a finger, the conductor may be full charged, and the electrometer c[71], will rise to the height indicating a full charge: but the moment the point is *uncovered*, the ball of the electrometer drops, showing the prime conductor to be instantly discharged and nearly emptied of its electricity. Turn the wire its *blunt* end upwards (which represents an unpointed bar) and no such effect follows, the electrometer remaining at its usual height when the prime conductor is charged.

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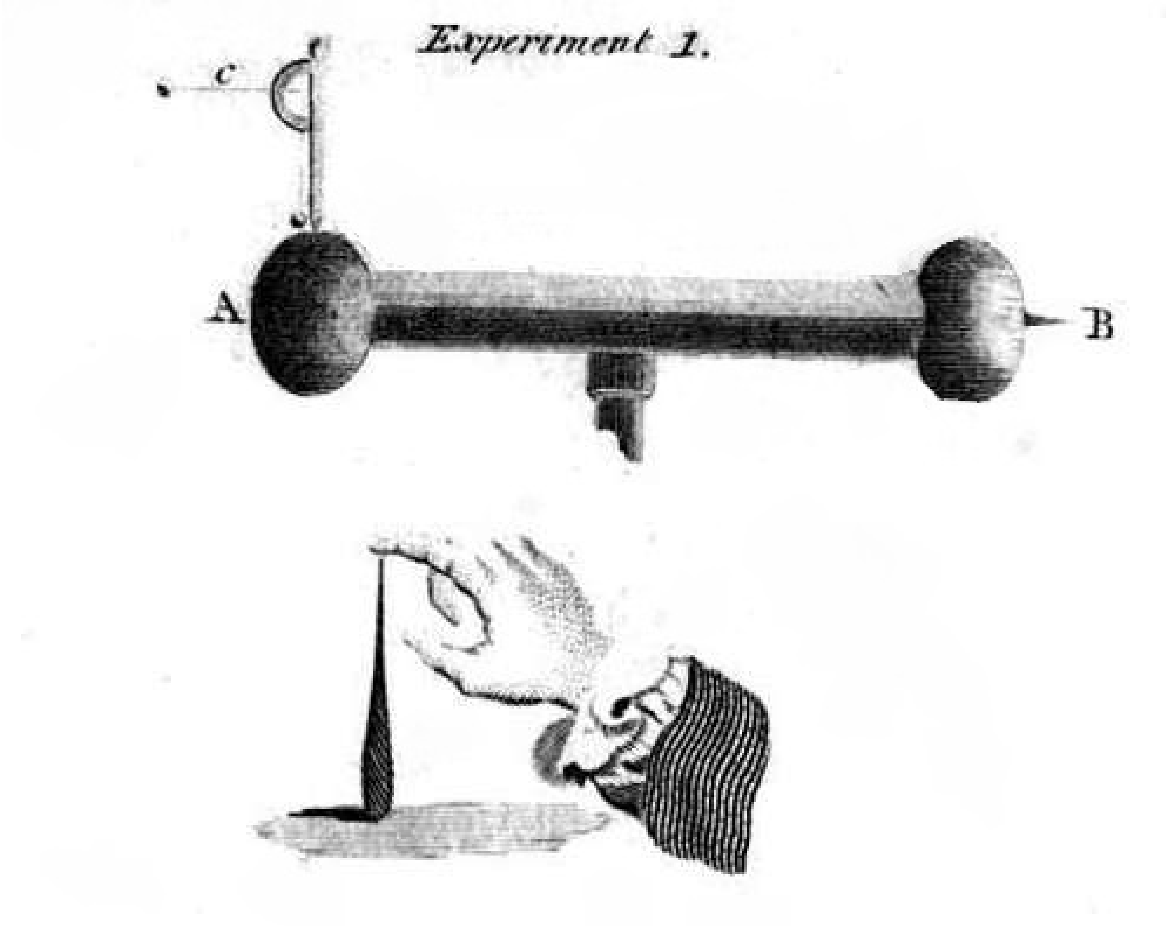
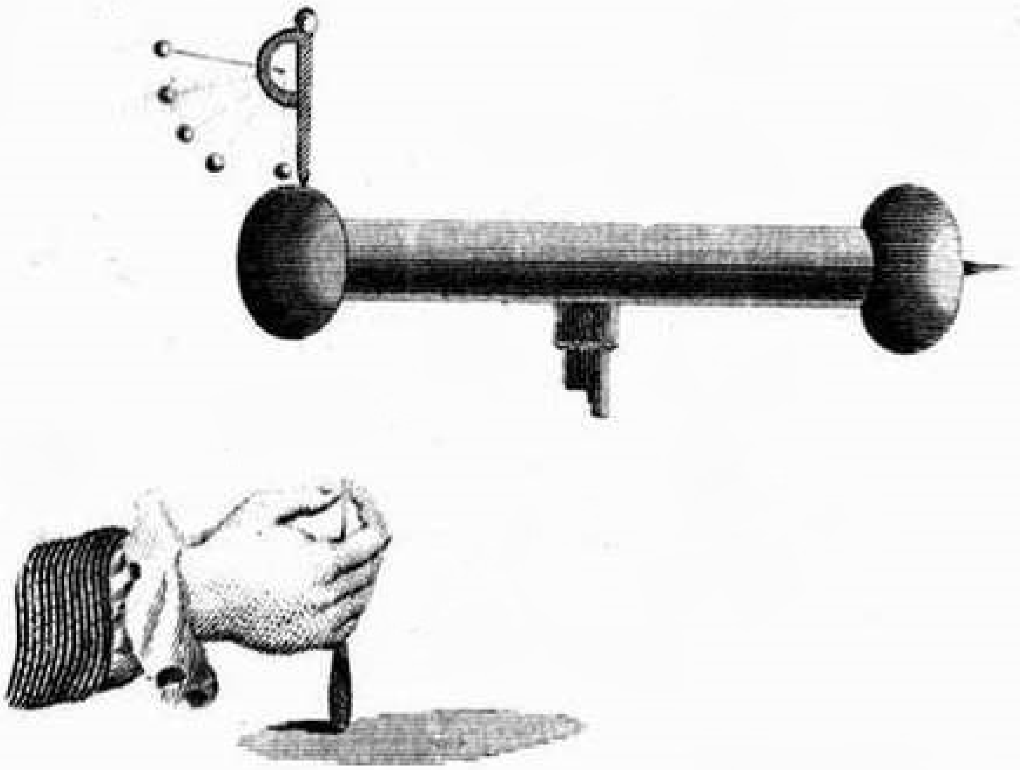
Plate IV.



Exp. 2.



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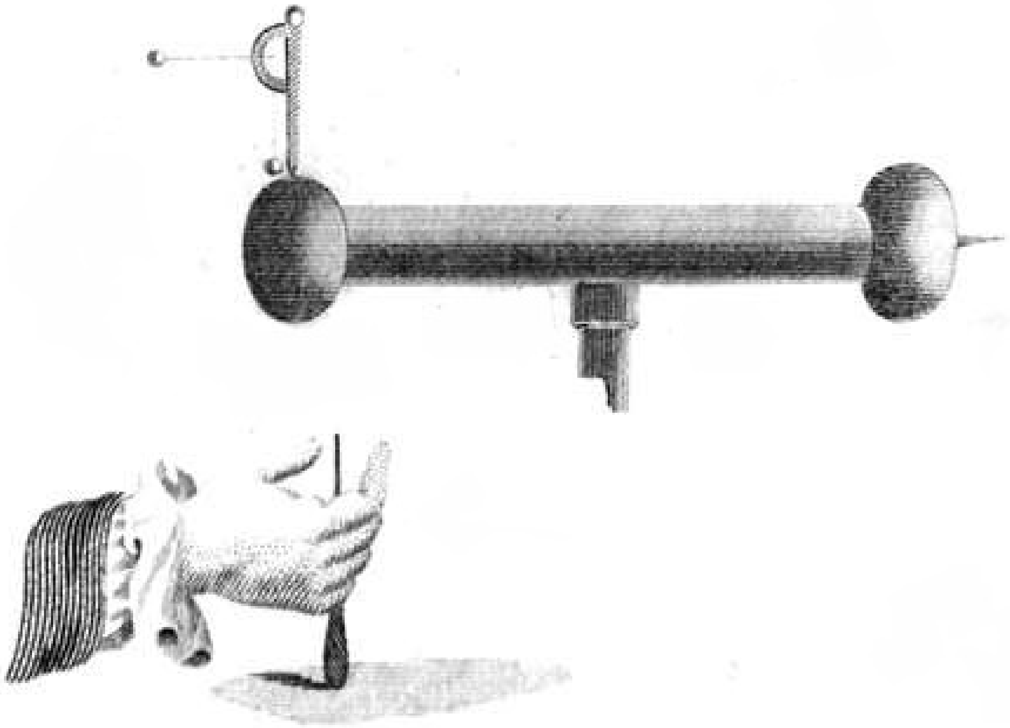

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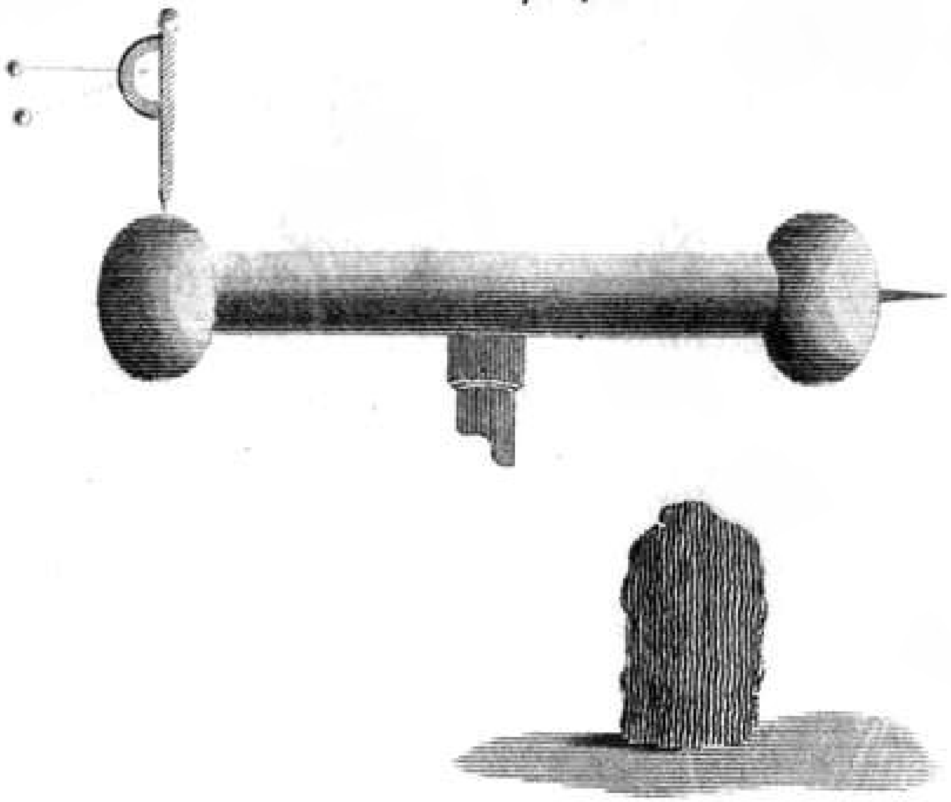

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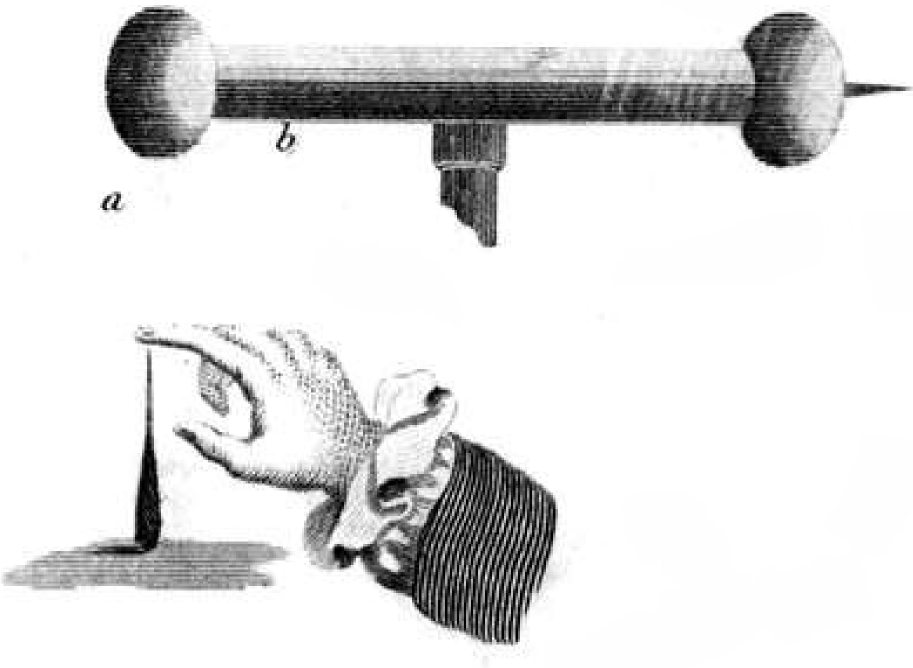

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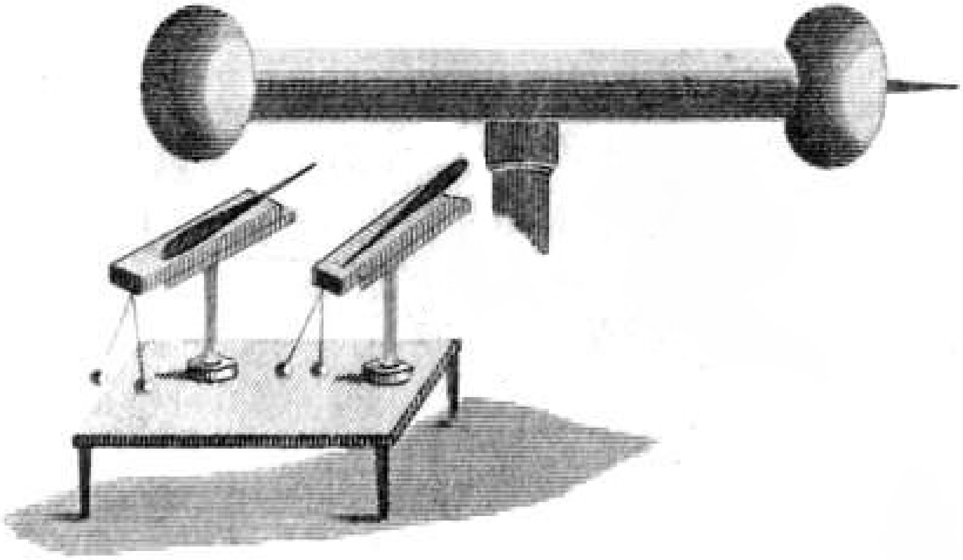


Published as the Act directs, April 1, 1806, by Longman, Hurst, Rees & Orme, Paternoster Row.

OBSERVATION.

What quantity of lightning, a high pointed rod well communicating with the earth may be expected to discharge from the clouds silently in a short time, is yet unknown; but I have reason from a particular fact to think it may at some times be very great. In Philadelphia I had such a rod fixed to the top of my chimney, and extending about nine feet above it. From the foot of this rod, a wire (the thickness of a goose-quill) came through a covered glass tube in the roof, and down through the well of the stair-case; the lower end connected with the iron spear of a pump. On the stair-case opposite to my chamber-door, the wire was divided; the ends separated about six inches, a little bell on each end; [and] between the bells a little brass ball suspended by a silk thread, to play between and strike the bells when clouds passed with electricity in them. After having frequently drawn sparks and charged bottles from the bell of the upper wire, I was one night waked by loud cracks on the stair-case. Starting up and opening the door, I perceived that the brass ball, instead of vibrating as usual between the bells, was repelled and kept at a distance from both; while the fire passed sometimes in very large quick cracks from bell to bell; and sometimes in a continued dense white stream, seemingly as large as my finger, whereby the whole stair-case was enlightened as with sunshine, so that one might see to pick up a pin[72]. And from the apparent quantity thus discharged, I cannot but conceive that a *number*[73] of such conductors must considerably lessen that of any approaching cloud, before it comes so near as to deliver its contents in a general stroke: — An effect not to be expected from bars *unpointed*; if the above experiment with the blunt end of the wire is deemed pertinent to the case.

EXPERIMENT II.

The pointed wire under the prime conductor continuing of the same height, *pinch* it between the thumb and finger near the top, so as *just to conceal* the point; then turning the globe, the electrometer will rise and mark the full charge. Slip the fingers down so as to discover about half an inch of the wire, then another half inch, and then another; at every one of these motions *discovering more and more* of the pointed wire; you will see the electrometer fall quick and proportionably, stopping when you stop. If you slip down the *whole distance* at once, the ball falls instantly down to the stem.

OBSERVATION.

From this experiment it seems that a greater effect in drawing off the lightning from the clouds may be expected from *long* pointed rods, than from *short* ones; I mean from such as show the greatest length, *above the building* they are fixed on.

EXPERIMENT III.

Instead of pinching the point between the thumb and finger, as in the last experiment, keep the thumb and finger each at *near an inch distance* from it, but at the *same height*, the point between them. In this situation, though the point is fairly exposed to the prime conductor, it has little or no effect; the electrometer rises to the height of a full charge. — But the moment the fingers are *taken away*, the ball falls quick to the stem.

OBSERVATION.

To explain this, it is supposed, that one reason of the sudden effect produced by a long naked pointed wire is, that (by the repulsive power of the positive charge in the prime conductor) the natural quantity of electricity contained in the pointed wire is driven down into the earth, and the point of the wire made strongly *negative*; whence it attracts the electricity of the prime conductor more strongly than bodies in their natural state would do; the *small quantity of common matter* in the point, not being able by its attractive force to retain its *natural quantity of the electric fluid*, against the force of that repulsion. — But the finger and thumb being substantial and blunt bodies, though as near the prime conductor, hold up better their *own* natural quantity against the force of that repulsion; and so, continuing nearly in the natural state, they jointly operate on the electric fluid in the point, opposing its descent, and *aiding the point* to retain it; contrary to the repelling power of the prime conductor, which would drive it down. — And this may also serve to explain the different powers of the point in the preceding experiment, on the slipping down the finger and thumb to different distances.

Hence is collected, that a pointed rod erected *between two tall chimnies*, and very little higher (an instance of which I have seen) cannot have so good an effect, as if it had been erected on one of the chimneys, its whole length above it.

EXPERIMENT IV.

If, *instead* of a long pointed wire, a *large solid body* (to represent a building without a point) be brought under and as near the prime conductor, when charged; the ball of the electrometer will *fall* a little; and on taking away the large body, will *rise again*.

OBSERVATION.

Its *rising again* shows that the prime conductor lost little or none of its electric charge, as it had done through the point: the *falling* of the ball while the large body was under the conductor therefore shows, that a quantity of its atmosphere was drawn from the end where the electrometer is placed to the part immediately over the large body, and there accumulated *ready* to strike into it with its whole undiminished force, as soon as within the striking distance; and, were the prime conductor moveable like a *cloud*, it would approach the body by attraction till within that distance. The swift motion of clouds, as driven by the winds, probably prevents this happening so often as otherwise it might do: for, though parts of the cloud may stoop towards a building as they pass, in consequence of such attraction, yet they are carried forward beyond the striking distance before they could by their descending come within it.

EXPERIMENT V.

Attach a small light *lock of cotton* to the underside of the prime conductor, so that it may hang down towards the pointed wire mentioned in the first experiment. *Cover* the point with your finger, and the globe being turned, the cotton will extend itself, stretching down towards the finger, as at *a*; but on *uncovering* the point, it instantly flies up to the prime conductor, as at *b*, and continues there as long as the point is uncovered. The moment you cover it again, the cotton flies down again, extending itself towards the finger; and the same happens in degree, if (instead of the finger) you use, uncovered, the *blunt* end of the wire uppermost.

OBSERVATION.

To explain this, it is supposed that the cotton, by its connection with the prime conductor, receives from it a quantity of its electricity; which occasions its being attracted by the *finger* that remains still in nearly its natural state. But when a *point* is opposed to the cotton, its electricity is thereby taken from it, faster than it can at a distance be supplied with a fresh quantity from the conductor. Therefore being reduced *nearer* to the natural state, it is attracted *up* to the electrified prime conductor; *rather than down*, as before, to the finger.

Supposing farther that the prime conductor represents a cloud charged with the electric fluid; the cotton, a ragged fragment of cloud (of which the underside of great thunder-clouds are seen to have many) the finger, a chimney or highest part of a building. — We then may conceive that when such a cloud passes over a *building*, some one of its ragged under-hanging fragments may be drawn down by the chimney or other high part of the edifice; creating thereby a *more easy communication* between it and the great cloud. — But a *long pointed rod* being presented to this fragment, may occasion its receding, like the cotton, up to the great cloud; and thereby *increase*, instead *of lessening* the distance, so as often to make it greater than the striking distance. Turning the *blunt end of a wire* uppermost (which represents the unpointed bar) it appears that the same good effect is not from that to be expected. A long pointed rod it is therefore imagined, may *prevent* some strokes; as well as *conduct* others that fall upon it, when a great body of cloud comes on so heavily that the above repelling operation on fragments cannot take place.

EXPERIMENT VI.

Opposite the side of the prime conductor place *separately*, isolated by wax stems, Mr. Canton’s two boxes with pith balls suspended by fine linen threads. On each box, lay a wire six inches long and one-fifth of an inch thick, tapering to a sharp point; but so laid, as that four inches of the *pointed* end of *one* wire, and an equal length of the *blunt* end of the *other*, may project beyond the ends of the boxes; and both at eighteen inches distance from the prime conductor. Then charging the prime conductor by a turn or two of the globe, the balls of each pair will separate; those of the box, whence the point projects most, *considerably*; the others *less*. Touch the prime conductor, and those of the box with the *blunt* point will *collapse*, and join. Those connected with the *point* will at the same time approach each other, *till* within about an inch, and there *remain*.

OBSERVATION.

This seems a proof, that though the small sharpened part of the wire must have had a *less natural* quantity in it before the operation, than the thick blunt part; yet a greater quantity was *driven down from it* to the balls. Thence it is again inferred, that the pointed rod is rendered *more negative*: and farther, that if a *stroke must fall* from the cloud over a building, furnished with such a rod, it is more likely to be drawn to that pointed rod, than to a blunt one; as being more strongly negative, and of course its attraction stronger. And it seems more eligible, that the lightning should fall on the point of the conductor (provided to convey it into the earth) than on any other part of the building, *thence* to proceed to such conductor. Which end is also more likely to be obtained by the length and loftiness of the rod; as protecting more extensively the building under it.

It has been *objected*, that erecting pointed rods upon *edifices*, is to *invite* and draw the lightning into *them*; and therefore dangerous. Were such rods to be erected on buildings, *without continuing the communication* quite down into the moist earth, this objection might then have weight; but when such compleat conductors are made, the lightning is invited not into the building, but into the *earth*, the situation it aims at, and which it always seizes every help to obtain, even from broken partial metalline conductors.

It has also been suggested, that from such electric experiments *nothing certain can be concluded as to the great operations of nature*; since it is often seen, that experiments, which have succeeded in small, in large have failed. It is true that in mechanics this has sometimes happened. But when it is considered that we owe our first knowledge of the nature and operations of lightning, to observations on such small experiments; and that on carefully comparing the most accurate accounts of former facts, and the exactest relations of those that have occurred since, the effects have surprizingly agreed with the theory; it is humbly conceived that in natural philosophy, in this branch of it at least, the suggestion has not so much weight; and that the farther new experiments now adduced in recommendation of *long* sharp-pointed rods, may have some claim to credit and consideration.

It has been urged too, that though points may have considerable effects on a *small* prime conductor at *small distances*; yet on *great* clouds and at *great distances*, nothing is to be expected from them. To this it is answered, that in those *small* experiments it is evident the points act at a greater than the *striking* distance; and in the large way, their service is *only expected* where there is *such* nearness of the cloud, as to *endanger a stroke*; and there, it cannot be doubted the points must have some effect. And if the quantity discharged by a single pointed rod may be so considerable as I have shown it; the quantity discharged by a number will be proportionably greater.

But this part of the theory does not depend alone on *small* experiments. Since the practice of erecting pointed rods in America (now near twenty years) five of them have been struck by lightning, viz. Mr. Raven's and Mr. Maine's in South Carolina; Mr. Tucker's in Virginia; Mr. West's and Mr. Moulder's in Philadelphia. Possibly there may have been more that have not come to my knowledge. But in every one of these, the lightning did *not* fall upon the *body of the house*, but precisely on the several *points* of the rods; and, though the conductors were sometimes *not sufficiently large and complete*, was conveyed into the earth, without any material damage to the buildings. Facts then *in great*, as far as we have them authenticated, justify the opinion that is drawn from the experiments *in small* as above related.

It has also been objected, that unless we knew the quantity that might *possibly* be discharged at one stroke from the clouds, we cannot be sure we have provided *sufficient* conductors; and therefore cannot depend on their conveying away *all* that may fall on their points. Indeed we have nothing to form a judgment by in this but past facts; and we know of no instance where a *compleat* conductor to the moist earth *has* been insufficient, if half an inch diameter. It is probable that many strokes of lightning have been conveyed through the common leaden pipes affixed to houses to carry down the water from the roof to the ground: and there is no account of such pipes being melted and destroyed, as must sometimes have happened if they had been insufficient. We can then only judge of the dimensions proper for a conductor of lightning, as we do of those proper for a *conductor of rain*, by past observation. And as we think a pipe of three inches bore sufficient to carry off the rain that falls on a square of 20 feet, because we never saw such a pipe glutted by any shower; so we may judge a conductor of an inch diameter, more than sufficient for any stroke of lightning that will fall on its point. It is true that if another deluge should happen wherein the windows of heaven are to be opened, such pipes may be unequal to the falling quantity; and if God for our sins should think fit to rain fire upon us, as upon some cities of old, it is not expected that our conductors of whatever size, should secure our houses against a miracle. Probably as water drawn up into the air and there forming clouds, is disposed to fall again in *rain* by its natural gravity, as soon as a number of particles sufficient to make a drop can get together; so when the clouds are (by whatever means) over or under-charged [with the *electric fluid*] to a degree sufficient to attract them towards the earth, the equilibrium is restored, before the difference becomes great beyond that degree. Mr. Lane's *electrometer*, for limiting precisely the quantity of a shock that is to be administered in a medical view, may serve to make this more easily intelligible. The discharging knob does by a screw approach the conductor to the distance intended, but there remains fixed. Whatever power there may be in the glass globe to collect the fulminating fluid, and whatever capacity of receiving and accumulating it there may be in the bottle or glass jar; yet neither the accumulation or the discharge ever exceeds the destined quantity. Thus, were the *clouds* always at a certain fixed distance from the earth, all discharges would be made when the quantity accumulated was equal to the distance: But there is a circumstance which by occasionally lessening the distance, lessens the discharge; to wit, the moveableness of the clouds, and their being drawn nearer to the earth by attraction when electrified; so that discharges are thereby rendered more frequent and of course less violent. Hence whatever the quantity may be in nature, and whatever the power in the clouds of collecting it; yet an accumulation and force beyond what mankind has hitherto been acquainted with is scarce to be expected[74].

B. F.

Aug. 27, 1772.

[71] Mr. Henley's.

[72] Mr. de Romas saw still greater quantities of lightning brought down by the wire of his kite. He had "explosions from it, the noise of which greatly resembled that of thunder, and were heard (from without) into the heart of the city, notwithstanding the various noises there. The fire seen at the instant of the explosion had the shape of a spindle eight inches long and five lines in diameter. Yet from the time of the explosion to the end of the experiment, no lightning was seen above, nor any thunder heard. At another time the streams of fire issuing from it were observed to be an inch thick and ten feet long." — *See Dr. Priestley's History of Electricity*, pages 134-6, *first edition*.

[73] Twelve were proposed on and near the magazines at Purfleet.

[74] It may be fit to mention here, that the immediate occasion of the dispute concerning the preference between pointed and blunt conductors of lightning, arose as follows: — A powder-mill having blown up at Brescia, in consequence of its being struck with lightning, the English board of ordnance applied to their painter, Mr. Wilson, then of some note as an electrician, for a method to prevent the like accident to their magazines at Purfleet. Mr. Wilson having advised a blunt conductor, and it being understood that Dr. Franklin's opinion, formed upon the spot, was for a pointed one; the matter was referred in 1772, to the Royal Society, and by them as usual, to a committee, who, after consultation, prescribed a method conformable to Dr. Franklin's theory. But a harmless stroke of lightning, having under particular circumstances, fallen upon one of the buildings and its apparatus in May 1777; the subject came again into violent agitation, and was again referred to the society, and by the society again referred to a new committee, which committee confirmed the decision of the first committee.

B. V.[75]

[75] Wherever this signature occurs, the note is taken from a volume of Dr. Franklin's writings, entitled Political, Miscellaneous, and Philosophical Pieces, printed for Johnson, 1779. The editor of that volume, though a young man at the time, had already evinced extraordinary talents, and was the friend and correspondent of our author. As he has chosen to withhold his name, we conceive ourselves not entitled to disclose it: but we shall take the freedom of an acquaintance to use the notes occasionally, deeming them in many instances valuable historical records. *Editor.*

On the Utility of Electrical Conductors.

TO PROFESSOR LANDRIANI, OF ITALY.

PHILADELPHIA, OCT. 14, 1787.

Sir,

I have received the excellent work, *Upon the Utility of electrical Conductors*, which you had the goodness to send me. I read it with great pleasure, and beg you to accept my sincere thanks for it.

Upon my return to this country, I found the number of conductors much increased, many proofs of their efficacy in preserving buildings from lightning having demonstrated their utility. Among other instances, my own house was one day attacked by lightning, which occasioned the neighbours to run in to give assistance, in case of its being on fire. But no damage was done, and my family was only found a good deal frightened with the violence of the explosion.

Last year, my house being enlarged, the conductor was obliged to be taken down. I found, upon examination, that the pointed termination of copper, which was originally nine inches long, and about one third of an inch in diameter in its thickest part, had been almost entirely melted; and that its connection with the rod of iron below was very slight. Thus, in the course of time, this invention has proved of use to the author of it, and has added this personal advantage to the pleasure he before received, from having been useful to others.

Mr. Rittenhouse, our astronomer, has informed me, that having observed with his excellent telescope, many conductors that are within the field of his view, he has remarked in various instances, that the points were melted in like manner. There is no example of a house, provided with a perfect conductor, which has suffered any considerable damage; and even those which are without them have suffered little, since conductors have come common in this city.

B. FRANKLIN.

On the Effects of Electricity in paralytic Cases.

TO JOHN PRINGLE, M. D. AND F. R. S.

CRAVEN-STREET, DEC. 21, 1757.

Sir,

In compliance with your request, I send you the following account of what I can at present recollect relating to the effects of electricity in paralytic cases, which have fallen under my observation.

Some years since, when the news-papers made mention of great cures performed in Italy and Germany, by means of electricity, a number of paralytics were brought to me from different parts of Pensylvania, and the neighbouring provinces, to be electrised, which I did for them at their request. My method was, to place the patient first in a chair, on an electric stool, and draw a number of large strong sparks from all parts of the affected limb or side. Then I fully charged two six-gallon glass jars, each of which had about three square feet of surface coated; and I sent the united shock of these through the affected limb or limbs, repeating the stroke commonly three times each day. The first thing observed, was an immediate greater sensible warmth in the lame limbs that had received the stroke than in the others; and the next morning the patients usually related, that they had in the night felt a pricking sensation in the flesh of the paralytic limbs; and would sometimes show a number of small red spots, which they supposed were occasioned by those prickings. The limbs, too, were found more capable of voluntary motion, and seemed to receive strength. A man, for instance, who could not the first day lift the lame hand from off his knee, would the next day raise it four or five inches, the third day higher; and on the fifth day was able, but with a feeble languid motion, to take off his hat. These appearances gave great spirits to the patients, and made them hope a perfect cure; but I do not remember that I ever saw any amendment after the fifth day; which the patients perceiving, and finding the shocks pretty severe, they became discouraged, went home, and in a short time relapsed; so that I never knew any advantage from electricity in palsies that was permanent. And how far the apparent temporary advantage might arise from the exercise in the patients 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 a skilful physician. It may be, too, that a few great strokes, as given in my method, may not be so proper as many small ones; since by the account from Scotland of a case, in which two hundred shocks from a phial were given daily, it seems, that a perfect cure has been made. As to any uncommon strength supposed to be in the machine used in that case, I imagine it could have no share in the effect produced; since the strength of the shock from charged glass, is in proportion to the quantity of surface of the glass coated; so that my shocks from those large jars, must have been much greater than any that could be received from a phial held in the hand.

I am, with great respect, Sir,

Your most obedient Servant,

B. FRANKLIN.

Electrical Experiments on Amber.

Saturday, July 3, 1762.

To try, at the request of a friend, whether amber finely powdered might be melted and run together again by means of the electric fluid, I took a piece of small glass tube, about two inches and a half long, the bore about one-twelfth 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 one-eighth of an inch diameter. I rammed powder of amber into this as I had done in the other, and as the quantity of amber was greater, I increased the quantity of electric fluid, by discharging through it at once 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 hurt, and most of the powder remaining was turned black, which I suppose might be by the smoke forced through it from the burned 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. FRANKLIN.

On the Electricity of the Fogs in Ireland.

TO THOMAS RONAYNE, ESQ. AT CORKE[76].

LONDON, APRIL 20, 1766.

Sir,

I have received your very obliging and very ingenious letter by Captain Kearney. Your observations upon the electricity of fogs and the air in Ireland, and upon different circumstances of storms, appear to me very curious, and I thank you for them. There is not, in my opinion, any part of the earth whatever which is, or can be, naturally in a state of negative electricity: and though different circumstances may occasion an inequality in the distribution of the fluid, the equilibrium is immediately restored by means of its extreme subtilty, and of the excellent conductors with which the humid earth is amply provided. I am of opinion, however, that when a cloud, well charged positively, passes near the earth, it repels and forces down into the earth that natural portion of electricity, which exists near its surface, and in buildings, trees, &c. so as actually to reduce them to a negative state before it strikes them. I am of opinion too, that the negative state in which you have frequently found the balls, which are suspended from your apparatus, is not always occasioned by clouds in a negative state; but more commonly by clouds positively electrified, which have passed over them, and which in their passage have repelled and driven off a part of the electrical matter, which naturally existed in the apparatus; so that what remained after the passing of the clouds, diffusing itself uniformly through the apparatus, the whole became reduced to a negative state.

If you have read my experiments made in continuation of those of Mr. Canton, you will readily understand this; but you may easily make a few experiments, which will clearly demonstrate it. Let a common glass be warmed before the fire that it may continue very dry for some time; set it upon a table, and place upon it the small box made use of by Mr. Canton, so that the balls may hang a little beyond the edge of the table. Rub another glass, which has previously been warmed in a similar manner, with a piece of black silk or a silk handkerchief, in order to electrify it. Hold then the glass above the little box, at about the distance of three or four inches from that part, which is most distant from the balls; and you will see the balls separate from each other; being positively electrified by the natural portion of electricity, which was in the box, and which is driven to the further part of it by the repulsive power of the atmosphere in the excited glass. Touch the box near the little balls (the excited glass continuing in the same state) and the balls will again unite; the quantity of electricity which had been driven to this part being drawn off by your finger. Withdraw then both your finger and the glass at the same instant, and the quantity of electricity which remained in the box, uniformly diffusing itself, the balls will again be separated; being now in a negative state. While things are in this situation, begin once more to excite your glass, and hold it above the box, but not too near, and you will find, that when brought within a certain distance, the balls will at first approach each other, being then in a natural state. In proportion as the glass is brought nearer, they, will again separate, being positive. When the glass is moved beyond them, and at some little farther distance, they will unite again, being in a natural state. When it is entirely removed, they will separate again, being then made negative. The excited glass in this experiment may represent a cloud positively charged, which you see is capable of producing in this manner all the different changes in the apparatus, without the least necessity for supposing any negative cloud.

I am nevertheless fully convinced, that these are negative clouds; because they sometimes absorb, through the medium of the apparatus, the positive electricity of a large jar, the hundredth part of which the apparatus itself would have not been able to receive or contain at once. In fact, it is not difficult to conceive, that a large cloud, highly charged positively, may reduce smaller clouds to a negative state, when it passes above or near them, by forcing a part of their natural portion of the fluid either to their inferior surfaces, whence it may strike into the earth, or to the opposite side, whence it may strike into the adjacent clouds; so that when the large cloud has passed off to a distance, the small clouds shall remain in a negative state, exactly like the apparatus; the former (like the latter) being frequently insulated bodies, having communication neither with the earth nor with other clouds. Upon the same principle it may easily be conceived, in what manner a large negative cloud may render others positive.

The experiment which you mention, of filing your glass, is analogous to one which I made in 1751, or 1752. I had supposed in my preceding letters, that the pores of glass were smaller in the interior parts than near the surface, and that on this account they prevented the passage of the electrical fluid. To prove whether this was actually the case or not, I ground one of my phials in a part where it was extremely thin, grinding it considerably beyond the middle, and very near to the opposite superficies, as I found, upon breaking it after the experiment. It was charged nevertheless after being ground, equally well as before, which convinced me, that my hypothesis on this subject was erroneous. It is difficult to conceive where the immense superfluous quantity of electricity on the charged side of a glass is deposited.

I send you my paper concerning meteors, which was lately published here in the Philosophical Transactions, immediately after a paper by Mr. Hamilton on the same subject.

I am, Sir, &c.

B. FRANKLIN.

[76] This letter is translated from the French edition of Dr. Franklin’s works, as are also all that follow, to the Appendix, the one to Miss Stephenson excepted. *Editor.*

Mode of ascertaining, whether the Power, giving a Shock to those who touch either the Surinam Eel, or the Torpedo, be electrical.

1. TOUCH THE fish with a stick of dry sealing-wax, or a glass rod, and observe if the shock be communicated by means of those bodies.

Touch the same fish with an iron, or other metalline rod.

If the shock be communicated by the latter body, and not by the others, it is probably not the mechanical effect, as has been supposed, of some muscular action in the fish, but of a subtile fluid, in this respect analogous at least to the electric fluid.

2. Observe farther, whether the shock can be conveyed without the metal being actually in contact with the fish, and if it can, whether, in the space between, any light appear, and a slight noise or crackling be heard.

If so, these also are properties common to the electric fluid.

3. Lastly, touch the fish with the wire of a small Leyden bottle, and if the shock can be received across, observe whether the wire will attract and repel light bodies, and you feel a shock, while holding the bottle in one hand, and touching the wire with the other.

If so, the fluid, capable of producing such effects seems to have all the known properties of the electric fluid.

Addition, *12th August, 1772,*

In Consequence of the Experiments and Discoveries made in France by Mr. Walsh, and communicated by him to Dr. Franklin.

Let several persons, standing on the floor, hold hands, and let one of them touch the fish, so as to receive a shock. If the shock be felt by all, place the fish flat on a plate of metal, and let one of the persons holding hands touch this plate, while the person farthest from the plate touches the upper part of the fish with a metal rod: then observe, if the force of the shock be the same as to all the persons forming the circle, or is stronger than before.

Repeat this experiment with this difference: let two or three of the persons forming the circle, instead of holding by the hand, hold each an uncharged electrical bottle, so that the little balls at the end of the wires may touch, and observe, after the shock, if these wires will attract and repel light bodies, and if a ball of cork, suspended by a long silk string between the wires, a little distance from the bottles, will be alternately attracted and repelled by them.

On the Analogy between Magnetism and Electricity.

TO M. DUBOURG.

London, March 10, 1773.

Sir,

As to the magnetism, which seems produced by electricity, my real opinion is, that these two powers of nature have no affinity with each other, and that the apparent production of magnetism is purely accidental. The matter may be explained thus:

1st, The earth is a great magnet.

2dly, There is a subtile fluid, called the magnetic fluid, which exists in all ferruginous bodies, equally attracted by all their parts, and equally diffused through their whole substance; at least where the equilibrium is not disturbed by a power superior to the attraction of the iron.

3dly, This natural quantity of the magnetic fluid, which is contained in a given piece of iron, may be put in motion so as to be more rarefied in one part and more condensed in another; but it cannot be withdrawn by any force that we are yet made acquainted with, so as to leave the whole in a negative state, at least relatively to its natural quantity; neither can it be introduced so as to put the iron into a positive state, or render it *plus*. In this respect, therefore magnetism differs from electricity.

4thly, A piece of soft iron allows the magnetic fluid which it contains to be put in motion by a moderate force, so that being placed in a line with the magnetic pole of the earth, it immediately acquires the properties of a magnet; its magnetic fluid being drawn or forced from one extremity to the other; and this effect continues as long as it remains in the same position, one of its extremities becoming positively magnetised, and the other negatively. This temporary magnetism ceases as soon as the iron is turned east and west, the fluid immediately diffusing itself equally through the whole iron, as in its natural state.

5thly, The magnetic fluid in hard iron, or steel, is put in motion with more difficulty, requiring a force greater than the earth to excite it; and when once it has been forced from one extremity of the steel to the other, it is not easy for it to return; and thus a bar of steel is converted into a permanent magnet.

6thly, A great heat, by expanding the substance of this steel, and increasing the distance between its particles, affords a passage to the electric fluid, which is thus again restored to its proper equilibrium; the bar appearing no longer to possess magnetic virtue.

7thly, A bar of steel which is not magnetic, being placed in the same position, relatively to the pole of the earth, which the magnetic needle assumes, and in this position being heated and suddenly cooled, becomes a permanent magnet. The reason is, that while the bar was hot, the magnetic fluid which it naturally contained was easily forced from one extremity to the other by the magnetic virtue of the earth; and that the hardness and condensation, produced by the sudden cooling of the bar, retained it in this state without permitting it to resume its original situation.

8thly, The violent vibrations of the particles of a steel bar, when forcibly struck in the same position, separate the particles in such a manner during their vibration, that they permit a portion of the magnetic fluid to pass, influenced by the natural magnetism of the earth; and it is afterwards so forcibly retained by the re-approach of the particles when the vibration ceases, that the bar becomes a permanent magnet.

9thly, An electric shock passing through a needle in a like position, and dilating it for an instant, renders it, for the same reason, a permanent magnet; that is, not by imparting magnetism to it, but by allowing its proper magnetic fluid to put itself in motion.

10thly, Thus, there is not in reality more magnetism in a given piece of steel after it is become magnetic, than existed in it before. The natural quantity is only displaced or repelled. Hence it follows, that a strong apparatus of magnets may charge millions of bars of steel, without communicating to them any part of its proper magnetism; only putting in motion the magnetism which already existed in these bars.

I am chiefly indebted to that excellent philosopher of Petersburgh, Mr. Æpinus, for this hypothesis, which appears to me equally ingenious and solid. I say, *chiefly*, because, as it is many years since I read his book, which I have left in America, it may happen, that I may have added to or altered it in some respect; and if I have misrepresented any thing, the error ought to be charged to my account.

If this hypothesis appears admissible, it will serve as an answer to the greater part of your questions. I have only one remark to add, which is, that however great the force is of magnetism employed, you can only convert a given portion of steel into a magnet of a force proportioned to its capacity of retaining its magnetic fluid in the new position in which it is placed, without letting it return. Now this power is different in different kinds of steel, but limited in all kinds whatever.

B. FRANKLIN.

Concerning the Mode of rendering Meat tender by Electricity.

TO MESSRS. DUBOURG AND D'ALIBARD[77].

MY DEAR FRIENDS,

My answer to your questions concerning the mode of rendering meat tender by electricity, can only be founded upon conjecture; for I have not experiments enough to warrant the facts. All that I can say at present is, that I think electricity might be employed for this purpose, and I shall state what follows as the observations or reasons, which make me presume so.

It has been observed, that lightning, by rarefying and reducing into vapour the moisture contained in solid wood, in an oak, for instance, has forcibly separated its fibres, and broken it into small splinters; that by penetrating intimately the hardest metals, as iron, it has separated the parts in an instant, so as to convert a perfect solid into a state of fluidity: it is not then improbable, that the same subtile matter, passing through the bodies of animals with rapidity, should possess sufficient force to produce an effect nearly similar.

The flesh of animals, fresh killed in the usual manner, is firm, hard, and not in a very eatable state, because the particles adhere too forcibly to each other. At a certain period, the cohesion is weakened and in its progress towards putrefaction, which tends to produce a total separation, the flesh becomes what we call tender, or is in that state most proper to be used as our food.

It has frequently been remarked, that animals killed by lightning putrify immediately. This cannot be invariably the case, since a quantity of lightning sufficient to kill, may not be sufficient to tear and divide the fibres and particles of flesh, and reduce them to that tender state, which is the prelude to putrefaction. Hence it is, that some animals killed in this manner will keep longer than others. But the putrefaction sometimes proceeds with surprising celerity. A respectable person assured me, that he once knew a remarkable instance of this: A whole flock of sheep in Scotland, being closely assembled under a tree, were killed by a flash of lightning; and it being rather late in the evening, the proprietor, desirous of saving something, sent persons early the next morning to flay them; but the putrefaction was such, and the stench so abominable, that they had not the courage to execute their orders, and the bodies were accordingly buried in their skins. It is not unreasonable to presume, that between the period of their death and that of their putrefaction, a time intervened in which the flesh might be only tender, and only sufficiently so to be served at table. Add to this, that persons, who have eaten of fowls killed by our feeble imitation of lightning (electricity) and dressed immediately, have asserted, that the flesh was remarkably tender.

The little utility of this practice has perhaps prevented its being much adopted. For though it sometimes happens, that a company unexpectedly arriving at a country-house, or an unusual conflux of travellers to an inn, may render it necessary, to kill a number of animals for immediate use; yet as travellers have commonly a good appetite, little attention has been paid to the trifling inconvenience of having their meat a little tough. As this kind of death is nevertheless more sudden, and consequently less severe, than any other, if this should operate as a motive with compassionate persons to employ it for animals sacrificed for their use, they may conduct the process thus:

Having prepared a battery of six large glass jars (each from 20 to 24 pints) as for the Leyden experiment, and having established a communication, as usual, from the interior surface of each with the prime conductor, and having given them a full charge (which with a good machine may be executed in a few minutes, and may be estimated by an electrometer) a chain which communicates with the exterior of the jars must be wrapped round the thighs of the fowl; after which the operator, holding it by the wings, turned back and made to touch behind, must raise it so high that the head may receive the first shock from the prime conductor. The animal dies instantly. Let the head be immediately cut off to make it bleed, when it may be plucked and dressed immediately. This quantity of electricity is supposed sufficient for a turkey of ten pounds weight, and perhaps for a lamb. Experience alone will inform us of the requisite proportions for animals of different forms and ages. Probably not less will be required to render a small bird, which is very old, tender, than for a larger one, which is young. It is easy to furnish the requisite quantity of electricity, by employing a greater or less number of jars. As six jars, however, discharged at once, are capable of giving a very violent shock, the operator must be very circumspect, lest he should happen to make the experiment on his own flesh, instead of that of the fowl.

B. FRANKLIN.

[77] This letter has no date, but the one to which it is an answer is dated May 1, 1773. *Editor.*

In Answer to some Queries concerning the Choice of Glass for the Leyden Experiment.

TO M. DUBOURG.

London, June 1, 1773.

Sir,

I wish, with you, that some chemist (who should, if possible, be at the same time an electrician) would, in pursuance of the excellent hints contained in your letter, undertake to work upon glass with the view you have recommended. By means of a perfect knowledge of this substance, with respect to its electrical qualities, we might proceed with more certainty, as well in making our own experiments, as in repeating those, which have been made by others in different countries, which I believe have frequently been attended with different success on account of differences in the glass employed, thence occasioning frequent misunderstandings and contrariety of opinions.

There is another circumstance much to be desired with respect to glass, and that is, that it should not be subject to break when highly charged in the Leyden experiment. I have known eight jars broken out of twenty, and at another time, twelve out of thirty-five. A similar loss would greatly discourage electricians desirous of accumulating a great power for certain experiments. — We have never been able hitherto to account for the cause of such misfortunes. The first idea which occurs is, that the positive electricity, being accumulated on one side of the glass, rushes violently through it, in order to supply the deficiency on the other side and to restore the equilibrium. This however I cannot conceive to be the true reason, when I consider, that a great number of jars being united, so as to be charged and discharged at the same time, the breaking of a single jar will discharge the whole; for, if the accident proceeded from the weakness of the glass, it is not probable, that eight of them should be precisely of the same degree of weakness, as to break every one at the same instant, it being more likely, that the weakest should break first, and, by breaking, secure the rest; and again, when it is necessary to produce a certain effect, by means of the whole charge passing through a determined circle (as, for instance, to melt a small wire) if the charge, instead of passing in this circle, rushed through the sides of the jars, the intended effect would not be produced; which, however, is contrary to fact. For these reasons, I suspect, that there is, in the substance of the glass, either some little globules of air, or some portions of unvitrified sand or salt, into which a quantity of the electric fluid may be forced during the charge, and there retained till the general discharge: and that the force being suddenly withdrawn, the elasticity of the fluid acts upon the glass in which it is inclosed, not being able to escape hastily without breaking the glass. I offer this only as a conjecture, which I leave to others to examine.

The globe which I had that could not be excited, though it was from the same glass-house which furnished the other excellent globes in my possession, was not of the same frit. The glass which was usually manufactured there, was rather of the green kind, and chiefly intended for drinking-glasses and bottles; but the proprietors being desirous of attempting a trial of white glass, the globe in question was of this frit. The glass not being of a perfect white, the proprietors were dissatisfied with it, and abandoned their project. I suspected that too great a quantity of salt was admitted into the composition; but I am no judge of these matters.

B. FRANKLIN.

Concerning the Leyden Bottle.

TO MISS STEPHENSON.

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 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.

APPENDIX.

No. 1 [78]. Account of experiments made in electricity at Marly.

*T*HE EARLY LETTERS *of Dr. Franklin on Electricity having been translated into French, and printed at Paris; the Abbé 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, Pages 227, 228.*

Sir,

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 conjectures 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 forty feet high. On the 10th of May, twenty 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 ninety-nine feet high, placed upon a cake of resin, two feet square, and three inches thick. On the 18th of May, between four and five in the afternoon, a stormy cloud having passed over the bar, where it remained half an hour, he drew sparks from the bar, like those from the gun barrel, when, in the electrical experiments, the globe is only rubbed by the cushion, and they produced the same noise, the same fire, and the same crackling. They drew the strongest sparks at the distance of nine lines, while the rain, mingled with a little hail, fell from the cloud, without either thunder or lightning; this cloud being, according to all appearance, only the consequence of a storm, which happened elsewhere.

I am, with a profound respect,

Your most humble and obedient servant,

G. MAZEAS.

[78] See the paragraph between brackets, page 267.

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.

"En suivant la route que M. Franklin nous a tracée, j'ai obtenu une satisfaction complette. Voici les préparatifs, le procédé & le succès.

"1º. 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 pointue par son extrémité supérieure; pour lui ménager 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.

"2º. 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 troisieme est au-dedans du jardin. Pour les affermir toutes ensemble, l'on à cloué sur chacune des entretoises à vingt pieds de hauteur; & comme le grand vent agitoit encore cette espéce d'édifice, l'on a attaché 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.

"3º. J'ai fait construire entres les deux perches voisines du mur, & adosser contre ce mur une petite guerite de bois capable de contenir un homme & une table.

"4º. J'ai fait placer au milieu de la guérite une petite table d'environ un demi-pied de hauteur; & sur cette table j'ai fait dresser & affermir un tabouret electrique. Ce tabouret n'est autre chose qu'une petite planche quarrée, portée sur trois bouteilles à vin; il n'est fait de cette matiere que pour suppléer au defaut d'un gâteau de résine qui me manquoit.

"5º. Tout étant ainsi préparé, j'ai fait elever perpendiculairement la verge de fer au milieu des trois perches, & je l'ai affermie en l'attachant à chacune des perches avec de forts cordons de soie par deux endroits 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 appuyé sur le milieu du tabouret electrique, où j'ai fait creuser un trou propre à le recevoir.

"6º. Comme il étoit important de garantir de la pluie te tabouret & les cordons de soie, parce qu'ils laisseroient passer la matiére électrique s'ils etoient mouillés, j'ai pris les précautions necessaires pour en empêcher. C'est dans cette vue que j'ai mis mon tabouret 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 côtes 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; & afin de se garantir des piqûres 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 chargé de faire les observations en mon absence, 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 pétillement; il tire une seconde étincelle plus fort que la premiere & 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'allarme se répand dans le village: la grêle qui survient n'empêche 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 â 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 suivante, qu'il m'ecrivit à la hâte."

Je vous annonce, Monsieur, ce que veus attendez: l'expérience est complette. 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 fauteüil, où j'étois occupé à lire: je suis allé chez Coiffier, qui déja m'avoit dépêché un enfant que j'ai rencontré en chemin, pour me prier de veenir; j'ai doublé 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 evançant 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 répeté 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 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 brass, 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 recontré 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 frappait davantage à mesure qu'ils s'approichient de moi: j'ai porté chez moi la même odeur, & mes domestiques s'en sont apperçus sans que je leur aye rien dit.*

Voilà, Monsieur, un récit fait à la hâte, mais naif & 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 bruyantes que celles dont il est parlé dans la lettre. Ces premieres personnes arrivant successivement, n'osient 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 matiere 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 devenue une réalité, & j'ose croire que plus on approfondira tout ce qu'il a publié sur l'électricité, plus on reconnoîtra combien la physique lui est redevable pour cette partie."

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, though several members of the Royal Society, as well as myself, did, upon the first advices from France, prepare and set up the necessary apparatus for this purpose, we were defeated in our expectations, from the uncommon coolness and dampness of the air here, during the whole summer. We had only at London one thunder-storm; viz. on July 20; and then the thunder was accompanied with rain; so that, by wetting the apparatus, the electricity was dissipated too soon to be perceived upon touching those parts of the apparatus, which served to conduct 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 approaching it a second time, I received the spark at the distance of about half an inch, and saw it distinctly. This I repeated four or five times in the space of a minute, but the sparks grew weaker and weaker; and in less than two minutes the tin tube did not appear to be 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. Wilson likewise of the Society, to whom we are much obliged for the trouble he has taken in these pursuits, had an opportunity of verifying Mr. Franklin's hypothesis. He informed me, by a letter from near Chelmsford, in Essex, dated August 12, 1752, that, on that day about noon, he perceived several electrical snaps, during, or rather at the end of a thunder-storm, from no other apparatus than an iron curtain rod, one end of which he put into the neck of a glass phial, and held this phial in his hand. To the other end of the iron he fastened three needles with some silk. This phial, supporting the rod, he held in one hand, and drew snaps from the rod with a finger of his other. This experiment was not made upon any eminence, but in the garden of a gentleman, at whose house he then was.

Dr. Bevis observed, at Mr. Cave's, at St. John's Gate, nearly the same phenomena 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.

No. 2.

No. 2. Remarks on the Abbé Nollet's Letters to Benjamin Franklin, Esq. of Philadelphia, on Electricity: by Mr. David Colden, of New York.

COLDENHAM, IN NEW York, Dec. 4, 1753.

Sir,

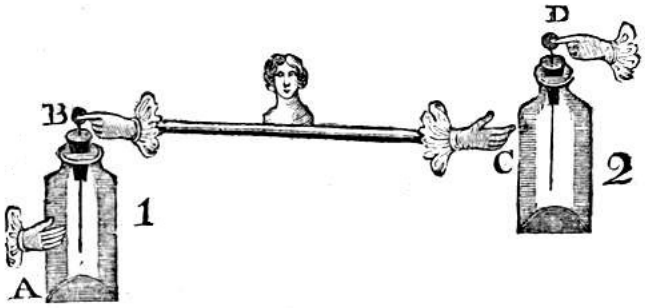
In considering the Abbé Nollet's Letters to Mr. Franklin, I am obliged to pass by all the experiments which are made with, or in, bottles hermetically sealed, or exhausted of air; because, not being able to repeat the experiments, I could not second any thing which occurs to me thereon, by experimental 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 entire thickness of the glass: he takes Mr. Franklin's experiment of the magical picture, and writes thus of it. "When you electrise a pane of glass coated on both sides with metal, it is evident that whatever is placed on the side opposite to that which receives the electricity from the conductor, receives also an evident electrical virtue." Which Mr. Franklin says, is that equal quantity of electric matter, driven out of this side, by what is received from the conductor on the other side; and which will continue to give an electrical virtue to any thing in contact with it, till it is entirely discharged of its electrical fire. To which the Abbé thus objects; "Tell me (says he), I pray you, how much time is necessary for this pretended discharge? I can assure you, that after having maintained the electrisation 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 electrised 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 electricity is, that when you present your finger to it, a spark will issue from it to your finger: now when a phial, prepared for the Leyden experiment, is hung to the gun-barrel or prime-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 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 electrise it; then bring this ball nigh to any fixed body, and it will appear to be attracted by that body, for it will fly to it: now, by the consent of electricians, the attractive cause is in the ball itself, and not in the fixed body to which it flies: this is a similar case with the apparent attraction of light bodies, to the external surface of a charged phial.

The Abbé says, *p.* 69, "that he can electrise a hundred men, standing on wax, if they hold hands, and if one of them touch one of these surfaces (the exterior) with the end of his finger:" this I know he can, while the phial is charging, but after the phial is charged I am as certain he cannot: that is, hang a phial, prepared for the Leyden experiment, to the conductor, and let a man, standing on the floor, touch the coating with his finger, while the globe is 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 electrised. After the electric matter was spewed out as above from the hook of the 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 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 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 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 pricking 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 a third phial, and that phial discharged singly through my arms, it gave me a sensible shock. If these experiments prove that the electric matter does not pass through the entire 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 *Observations on the Leyden Bottle*, p. 179, 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 though decanted into another phial, since no non-electric body touched 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 electrised; for if a thread be tied round it, and its ends left about two inches long, they will extend themselves out like a pair of horns; but if you touch the conductor, a spark will issue from it, and the threads will fall, nor does the conductor shew the least sign of being electrised after this is done. I think that by this touch, I have taken out all the charge of electric matter that was in the conductor, the hook of the phial, and water or filings of iron 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 bason, and has not any of that great quantity which produces 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 electrised. It receives this electrisation 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 electrises 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 the 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 electrised, 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 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 shown as great a partiality in relating experiments, as the Abbé has done.

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