

**New experiments and observations on electricity. : Made at Philadelphia in America. / By Benjamin Franklin, Esq; and communicated in several letters to P. Collinson, Esq; of London, F.R.S. Part II.**

**Contributors**

Franklin, Benjamin, 1706-1790.

**Publication/Creation**

London : Printed by R. Cave, at St John's-Gate, MDCCLXII. [1762]

**Persistent URL**

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14  
NEW EXPERIMENTS

AND

OBSERVATIONS

ON

ELECTRICITY.

MADE AT

*Philadelphia in America.*

BY

BENJAMIN FRANKLIN, *Esq;*

AND

Communicated in several Letters to P. COLLINSON, *Esq;*  
of *London*, F.R.S.

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PART II.

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The THIRD EDITION.

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LONDON:

Printed by R. CAVE, at *St John's-Gate*. MDCCLXII.  
(Price 6 *d.*)



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# LETTER V.

FROM

BENJAMIN FRANKLIN, *Esq*; of PHILADELPHIA.

TO

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

S I R,

July 27, 1750.

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

As to the experiment that ingenious gentleman mentions, and which he thinks conclusive on the other side, I persuade myself he will change his opinion of it, when he considers, that as one person applying the wire of the charged bottle to warm spirits, in a spoon held by another person, both standing on the floor, will fire the spirits, and yet such firing will not determine whether the accumula-



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

June 29, 1751. In Capt. *Waddel's* account of the effects of lightening on his ship, I could not but take notice of the large comazants (as he calls them) that settled on the spintles at the topmast 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 revers'd ; the North point turning to the South. — By electricity we have (*here at Philadelphia*) frequently given polarity to needles, and reversed them at pleasure.

Mr



Mr *Wilson*, at *London*, tried it on two large masses, and with too small force.

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

The polarity given is strongest when the needle is struck, lying North and South, weakest when lying East and West; perhaps if the force was still greater, the South end, enter'd by the fire, (when the needle lies North and South) might become the North, otherwise it puzzles us to account for the inverting of compasses by lightening; 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



and neck run. Sometimes the surface on the body of the needle is also run, and appears blister'd when examin'd by a magnifying glass: the jars I make use of hold 7 or 8 gallons, and are coated and lined with tin-foil; each of them takes a thousand turns of a globe nine inches diameter to charge it.

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

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

Yours, &c.

B. FRANKLIN.

LET-



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# LETTER VI.

FROM

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

TO

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

SIR,

1751.

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



needles that had none, and fired dry gunpowder by the electric spark. I have five bottles that contain 8 or 9 gallons each, two of which charg'd, are sufficient for those purposes; but I can charge and discharge them all together. 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 lightening, 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.**

Que-



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

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

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

O

globe



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

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

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

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

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



air receives and conducts the electrical matter in proportion to its moisture, quite dry air not all : air is therefore to be class'd with the non-conductors. Dry air assists in confining the electrical atmosphere to the body it surrounds, and prevents its dissipating : for in vacuo it quits easily, and points operate stronger, *i. e.* they throw off or attract the electrical matter more freely, and at greater distances ; so that air intervening obstructs its passing from body to body, in some degree. A clean electrical phial and wire, containing air instead of water, will not be charged nor give a shock, any more than if it was fill'd with powder of glass ; but exhausted of air it operates as well as if fill'd with water. Yet, an electrical 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 N. Wester would dissipate it. I once electrified a large cork ball, at the end of a silk thread 3 feet long, the other end of which I held in my fingers, and whirl'd it round, like a sling, 100 times in the air, with the swiftest motion I could possibly give it, yet it retained its electrical atmosphere, though it must have pass'd through 800 yards of air, allowing my arm in giving the motion to add a foot to the semi-diameter of the circle.—By quite dry air, I mean the driest we have : for perhaps we never have any perfectly free from moisture. An electrical atmosphere raised round a thick wire, inserted in a phial of air, drives out none of the air, nor



on withdrawing that atmosphere will any air rush in, as I have found by a very curious experiment, accurately made, whence we concluded that the air's elasticity was not affected thereby.

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

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

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

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

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

LET-



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# LETTER VII.

FROM

Mr E. KINNERSLEY, at *Boston*,

TO

BENJAMIN FRANKLIN, Esq; at *Philadelphia*.

S I R

*Feb. 3, 1752.*

I Have the following experiments to communicate: I held in one hand a wire, which was fasten'd 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 convey'd 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



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

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

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

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

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



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

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

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

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

*I am, &c.*

E. KINNERSLEY.

LET-



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# LETTER VIII.

F R O M

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

T O

Mr E. KINNERSLEY, at *Boston*.

S I R,

*March 2, 1752.*

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

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

*I am, &c.*

B. FRANKLIN.

LET-



## L E T T E R IX.

F R O M

BENJAMIN FRANKLIN, *Esq*; of PHILADELPHIA.

T O

To Mr E. KINNERSLEY, at *Boston*.

S I R,

*March 16, 1752.*

**H**AVING brought your brimstone globe to work, I try'd one of the experiments you propos'd, and was agreeably surprized 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 work'd best, would again bring the conductor to afford no spark.

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

P

f6



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



verse of both happens, when you hold the same wire in your hand, and the globes are worked alternately : the brush is large, long, diverging and snapping (or rattling) when the sulphur globe is turn'd ; short, small, and hissing when the glass globe is turn'd.---When the brush is long, large, and much diverging, the body to which it joins, seems to me to be throwing the fire out ; and when the contrary appears, it seems to be drinking in. 3. I observe, that when I held 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 'tis otherwise. 4. The cool wind (or what was called so) that we used to feel as coming from an electrified point, is much 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 work'd 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



stuffed (properly mounted) might receive the fire from the sulphur, and charge the conductor positively? Such a globe would be in no danger in breaking. I think I can conceive how it may be done ; but have not time to add more than that I am,

*Yours, &c.*

B. FRANKLIN.

# LETTER X.

FROM

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

*Oct.* 19, 1752.

**A**S frequent mention is made in the news 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 inform the curious 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



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



help of a rubbed glass globe or tube; and thereby the sameness of the electric matter with that of lightening completely demonstrated.

B. F.

# LETTER XI.

F R O M

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

**A**S you tell me our friend *Cave* is about to add some later experiments to my pamphlet, with the *Errata*, I send a copy of a letter from Dr *Colden*, which may help to fill a few pages; also my kite experiment in the *Pensylvania Gazette*: to which I have nothing new to add, except the following experiment towards discovering more of the qualities of the electric fluid. From the prime conductor, hang a bullet by a wire hook; under the bullet, at half an inch distance, place a bright piece of silver to receive the sparks; then let the wheel be turned, and in a few minutes, (if the repeated sparks continually strike in the same spot) the silver will receive a blue stain near the colour of a watch spring. A bright piece of iron will also be spotted, but

not



not with that colour: it rather seems to be corroded. On gold, brass, or tin, I have not perceived that it makes any impression. But the spots on the silver or iron will be the same, whether the bullet be lead, brass, gold, or silver. On a silver bullet there will also appear a small spot, as well as in the plate below it.

F I N I S.



ADDITIONAL ERRATA in PART I. to the printed  
ELECTRICAL PAPERS. By B. FRANKLIN.

PAGE 38, line 2. read *represented by A and B,*  
*Fig. 6.*

Page 40, line 7. r. *repulsion.*

Page 65, l. 19. r. *Aqua Regia.*

Page 70, l. 6. r. *rubbed.*

Fig. VI. in the plate, wants the referring letters A, B, C, D. In the copy that Fig. was mentioned as a profile of a piece of water, the little circles representing particles. The two upper circles were mark'd A and B, and two others in the under line or row C and D. As it stands 'tis wholly unintelligible.

Page 73, l. 17. r. *air.*

Page 74, l. 7. r. *electrical.*

Page 81, l. 3. place a *comma* at conduct, and dele *comma* at floor.

Page 85, l. 5. r. *discharged.* l. 13, r. *p. 24.*

Page 86, l. 20. r. *after one.*



# NEW EXPERIMENTS

A N D

## OBSERVATIONS

O N

# ELECTRICITY.

M A D E A T

*Philadelphia in America.*

B Y

BENJAMIN FRANKLIN, *Esq;*

Communicated to P. COLLINSON, *Esq;* of *London*, F.R.S.

And read at the Royal Society *June 27*, and *July 4*, 1754.

To which are added

A Paper on the same Subject by *J. Canton*, M.A. F.R.S. and read at the Royal Society *Dec. 6*, 1753; and another in defence of Mr *Franklin* against the Abbe *Nollet*, by Mr *D. Colden*, of *New York*.

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P A R T    I I I.

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L O N D O N :

Printed and sold by D. HENRY, and R. CAVE, at *St John's-Gate*.    1754.    (Price 1 s.)



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PART III.

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LONDON:

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# LETTER XII.

FROM

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

TO

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

S I R,

*Philadelphia, September 1753.*

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

For in 1750 and 1751, being occasionally on the sea coast, I found, by experiments, that sea water in a bottle, tho' at first it would by agitation appear luminous, yet in a few hours it lost that virtue; *hence, and from this*, that I could not by agitating a solution of sea salt in water produce

Q



duce any light, I first began to doubt of my former hypothesis, and to suspect that the luminous appearance in sea water must be owing to some other principles.

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

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

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

I found the bells rang sometimes when there was no lightning or thunder, but only a dark cloud over the rod; that sometimes after a flash of lightning they would suddenly stop; and at other times, when they had not rang before, they would, after a flash, suddenly begin to ring; that the electricity was sometimes very faint, so that when  
a small



a small spark was obtain'd, another could not be got for sometime after; at other times the sparks would follow extremely quick, and once I had a continual stream from bell to bell, the size of a crow-quill: Even during the same gust there were considerable variations.

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

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

Being very intent on making this experiment, it was no small mortification to me, that I happened to be abroad during two of the greatest thunder-storms we had early in the spring; and tho' I had given orders in my family, that



if the bells rang when I was from home, they should catch some of the lightning for me in electrical phials, and they did so; yet it was mostly dissipated before my return; and in some of the other gusts, the quantity of lightning I was able to obtain, was so small, and the charge so weak, that I could not satisfy myself: Yet I sometimes saw what heighten'd my suspicious and inflam'd my curiosity.

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

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

Yet notwithstanding so many experiments, it seems I concluded too soon; for at last, *June* the 6th, in a gust which continued from five o'clock, P. M. to 7, I met with one cloud that was electrified positively, tho' several that pass'd over my rod before, during the same gust, were in the negative state. This was thus discovered:

I had



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

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

At length, while I was charging a phial by my glass globe, to repeat this experiment, my bells, of themselves, stopt ringing, and, after some pause, began to ring again.—But now, when I approached the wire of the charg'd



phial to the rod, 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 prov'd to me, that the rod was then *positively* electrify'd, 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 tho' 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, pursu'd 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, 'tis *the earth that strikes into the clouds, and not the clouds that strike into the earth.*

Those who are vers'd 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



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

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

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

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

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



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

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

Bodies of different kinds having thus attracted and absorb'd 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 shows signs of electricity.

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

When



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

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

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

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

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

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

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

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

Such a cloud, then, coming so near the earth as to be within the striking distance, will receive from the earth a

R

flash



flash of the electric fluid; which flash, to supply a great extent of cloud, must sometimes contain a very great quantity of that fluid.

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

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

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

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

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



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

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



was drawn higher, the can would receive more sparks; because the can and extended chain were capable of supporting a greater atmosphere than the can with the chain gather'd up into its belly.—And that the atmosphere round the can was diminished by raising the chain, and increased again by lowering it, is not only agreeable to reason, since the atmosphere of the chain must be drawn from that of the can, when it rose, and returned to it again when it fell; but was also evident to the eye, the lock of cotton always approaching the can when the chain was drawn up, and receding when it was let down again.

Thus we see that increase of surface makes a body capable of receiving a greater electric atmosphere: But this experiment does not, I own, fully demonstrate my new hypothesis; for the brass and silver still continue in their solid state, and are not rarified 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 rarified 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, how-



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

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



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

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

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



pers, discharge a very great quantity of electricity, which nevertheless will be all conducted round the corner of a book, by the fine filleting of gold on the cover, it following the gold the farthest way about, rather than take the shorter course through the cover, that not being so good a conductor. Now in this line of gold the metal is so extremely thin as to be little more than the colour of gold, and on an octavo book is not in the whole an inch square, and therefore not the 36th part of a grain according to M. *Reaumur*; yet 'tis sufficient to conduct the charge of 5 large jars, and how many more I know not. Now, I suppose a wire of  $\frac{1}{4}$  inch diameter to contain about 5000 times as much metal as there is in that gold line, and if so, it will conduct the charge of 25,000 such glass jars, which is a quantity, I imagine, far beyond what was ever contain'd in any one stroke of natural lightning. But a rod of half an inch diameter would conduct four times as much as one of a quarter.

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



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

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



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

These thoughts, my dear friend, are many of them crude and hasty; and if I were merely ambitious of acquiring some reputation in philosophy, I ought to keep them by me, till corrected and improved by time and farther experience. But since even short hints, and imperfect experiments in any new branch of science, being communicated, have oftentimes a good effect, in exciting the attention of the ingenious to the subject, and so become the occasion of more exact disquisitions, 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.



## L E T T E R XIII.

F R O M

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

T O

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

S I R,

*April 18, 1754.*

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

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



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



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R E M A R K S

On the Abbe NOLLET's

LETTERS on ELECTRICITY.

T O

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

B Y

Mr DAVID COLDEN, of *New-York*,

S I R, *Coldenham, in N. York, Dec. 4, 1753.*

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



“ on both sides with metal, it is evident that whatever is  
“ placed on the side opposite to that which receives the  
“ electricity from the conductor, receives also an evident  
“ electrical virtue.” Which Mr *Franklin* says, is that equal quantity of electric matter, driven out of this side, by what is received from the conductor on the other side; and which will continue to give an electrical virtue, to any thing in contact with it, till it is entirely discharged of its electrical fire. To which the 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 maintain’d the electrification for hours, this surface, which ought, as it seems to me, to be entirely discharged of its electrical matter, considering either the vast number of sparks that were drawn from it, or the time that this matter had been exposed to the action of the expulsive cause; this surface, I say, appeared rather better electrified thereby, and more proper to produce all the effects of an actual electric body.” *p.* 68.

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



conductor, and you turn the globe in order to charge it; as soon as the electric matter is excited, you can observe a spark to issue from the external surface of the phial to your finger; which, Mr *Franklin* says, is the natural electric matter of the glass driven out by that received by the inner surface from the conductor. If it be only drawn out by sparks, a vast number of them may be drawn; but if you take hold of the external surface with your hand, the phial will soon receive all the electric matter it is capable of, and the outside will then be entirely destitute of its electric matter, and no spark can be drawn from it by the finger: here then is a want of that effect which all bodies, 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



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

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

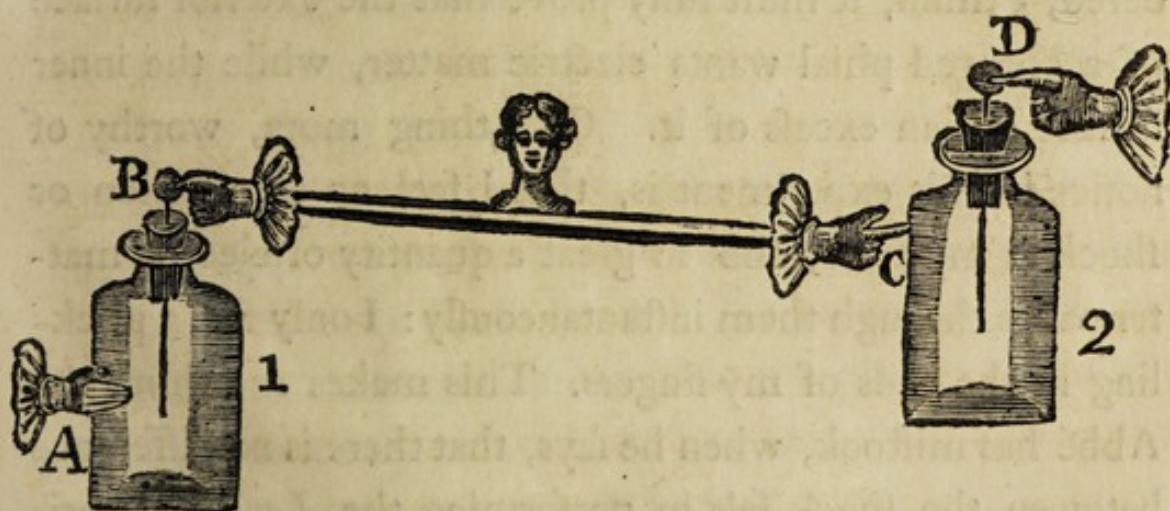


ter the electric matter was spewed out as above from the hook of a phial, prepared for the *Leyden* experiment, I hung another phial, in like manner prepared, to a hook fixed in the coating of the first, and held this other phial in my hand; now if there was any electric matter transmitted thro' the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

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



instantly discharged. If the 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 stirr'd off the wax: Wherefore this experiment fully convinces me, that the exterior surface is not charged; and not only so, but that it wants as much electric matter as the inner has of excess: For by this supposition, which is a part of Mr *Franklin's* system, the above experiment is easily accounted for, as follows:



When I stand on wax, my body is not capable of receiving all the electric matter from the hook of one phial, which it is ready to give; neither can it give as much to the coating of the other phial as it is ready to take, when one is only



applied to me: But when both are applied, the coating takes from one what the hook gives: Thus I receive the fire from the first phial at B, the exterior surface of which is supplied from the hand at A: I give the fire to the second phial at C, whose interior surface is discharged by the hand at D. This discharge at D may be made evident by receiving that fire into the hook of a third phial, which is done thus: In place of taking the hook of the second phial in your hand, run the wire of a third phial, prepared as for the *Leyden* experiment, through it, and hold this third phial in your hand, the second one hanging to it, by the ends of the hooks run through each other: When the experiment is performed, this third phial receives the fire at D, and will be charged. When this experiment is considered, I think, it must fully prove that the exterior surface of a charged phial wants electric matter, while the inner surface has an excess of it. One thing more, worthy of notice in this experiment is, that I feel no commotion or shock in my arms, tho' so great a quantity of electric matter passes through them instantaneously: I only feel a prickling in the ends of my fingers. This makes me think the *Abbé* has mistook, when he says, that there is no difference between the shock felt in performing the *Leyden* experiment, and the prickling felt on drawing simple sparks, except that of greater to less. In the last experiment, as much electric matter went through my arms, as would have given me a very sensible shock, had there been an immediate communication.



munication, by my arms, from the hook to the coating of the same phial; because when it was taken into a third phial, and that phial discharged singly thro' my arms, it gave me a sensible shock. If these experiments prove that the electric matter does not pass through the intire thickness of the glass; it is a necessary consequence that it must always come out where it enter'd.

The next thing I meet with, is in the 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<sup>l</sup> in contact with it. The experiments which Mr *Franklin* gave to prove this opinion in his *Experiments and Observations on Electricity, Letter III. p. 24.* convinced me that he was in the right; and what the Abbé has asserted in contradiction thereto, has not made me think otherwise. The Abbé perceiving, as I suppose, that the experiments, as Mr *Franklin* had perform'd them, must prove his assertion; alters them without giving any reason for it, and makes them in a manner that proves nothing. Why will he have the phial, into which the water is to be decanted from a charged phial, held in a man's hand? If the power of giving a shock is in the water contain'd in the phial, it should remain there tho' decanted into another phial, since no non-electric body touch'd it to take that power off. The phial being placed on wax is no objection, for it cannot take the power from the water, if it had any, but it is a necessary



means to try the fact; whereas, that phial's being charged when held in a man's hand, only proves that water will conduct the electric matter. The Abbé owns, *p.* 94. that he had heard this remarked, but says, Why is not a conductor of electricity an electric subject? This is not the question; Mr *Franklin* never said that water was not an electric subject; he said, that the power of giving a shock was in the glass, and not in the water; and this, his experiments, fully prove; so fully, that it may appear impertinent to offer any more: Yet as I do not know that the following has been taken notice of by any body before, my inserting of it in this place may be excused. It is this: Hang a phial, prepared for the *Leyden* experiment, to the conductor, by its hook, and charge it, which done, remove the communication from the bottom of the phial. Now the conductor shews evident signs of being electrified; for if a thread be tied round it, and its ends left about two inches long, they will extend themselves out like a pair of horns; but if you touch the conductor, a spark will issue from it, and the threads will fall, nor does the conductor shew the least sign of being electrified after this is done. I think that by this touch, I have taken out all the charge of electric matter that was in the conductor, the hook of the phial, and water or filings of iron contain'd in it; which is no more than we see all non-electric bodies will receive; yet the glass of the phial retains its power of giving a shock, as any one will find



find that pleases to try. This experiment fully evidences, that the water in the phial contains no more electric matter than it would do in an open basin, and has not any of that great quantity which produces the shock, and is only retain'd by the glass. If after the spark is drawn from the conductor, you touch the coating of the phial (which all this while is supposed to hang in the air, free from any non-electric body) the threads on the conductor will instantly start up, and shew that the conductor is electrified. It receives this electrification from the inner surface of the phial, which, when the outer surface can receive what it wants from the hand applied to it, will give as much as the bodies in contact with it can receive, or, if they be large enough, all that it has of excess. It is diverting to see how the threads will rise and fall by touching the coating and conductor of the phial alternately. May it not be that the difference between the charged side of the glass, and the outer or emptied side, being lessen'd 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 retain electrifies the water, or filings and conductor: For it seems to be a rule, that the one side must be emptied in the same proportion that the other is fill'd: Tho' this from experiment appears evident, yet it is still a mystery not to be accounted for.

I am



I am, in many places of the Abbé's book, surpris'd to find that experiments have succeeded so differently at *Paris* from what they did with Mr *Franklin*, and as I have always observ'd them to do. The 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 10 o'clock at night till 8 next morning, when I found it to retain a sufficient quantity of its charge, to give me a sensible commotion in my arms, though the room in which the phial stood had been swept in that time, which must have rais'd much dust to facilitate the discharge of the phial.

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



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

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

The Abbé, *p.* 108, says, “ that it is not impossible, as  
“ Mr *Franklin* says it is, to charge a phial while there is a  
“ communication form’d between its coating and its hook”. I have always found it impossible to charge such a phial so as to give 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



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 conceal'd a material part of the experiment ; a thing too mean for any gentleman to be charged with, who has not shewn as great a partiality in relating experiments, as the Abbé has done.





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# ELECTRICAL EXPERIMENTS,

With an Attempt to account for their

## SEVERAL PHÆNOMENA;

Together with

### Some Observations on *Thunder-Clouds*,

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

Dec. 6, 1753.

#### E X P E R I M E N T I.

**F**ROM the cieling, or any convenient part of a room, let two cork-balls, each about the bigness of a small pea, be suspended by linen threads of eight or nine inches in length, so as to be in contact with each other. Bring the excited glass tube under the balls,

U

and



and they will be separated by it, when held at the distance of three or four feet ; let it be brought nearer, and they will stand farther apart ; intirely withdraw it, and they will immediately come together. This experiment may be made with very small brass balls hung by silver wire ; and will succeed as well with sealing-wax made electrical, as with glass.

## E X P E R I M E N T II.

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

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



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

### E X P E R I M E N T III.

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

### E X P E R I M E N T IV.

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

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



the tin-tube, in the third experiment, is suppos'd to electrify it positively, or to add to the electrical fire it before contained ; and therefore some will be running off through the balls, and they will repel each other. But at the approach of excited glass, which likewise *emits* the electrical fluid, the discharge of it from the balls will be diminish'd ; or part will be driven back, by a force acting in a contrary direction ; and they will come nearer together. If the tube be held at such a distance from the balls, that the excess of the density of the fluid round about them, above the common quantity in air, be equal to the excess of the density of that within them, above the common quantity contain'd in cork ; their repulsion will be quite destroy'd. But if the tube be brought nearer ; the fluid 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 imbib'd by the balls to supply the deficiency ; and that more plentifully at the approach of excited glass, or a body positively electrified, than before ; whence the distance between the balls will be increased, as the fluid surrounding them is augmented. And in general, whether by the approach or recess of any body ; if the difference between the density of the internal and external fluid



be increased, or diminished; the repulsion of the balls will be increased, or diminished, accordingly.

### E X P E R I M E N T 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.

### E X P E R I M E N T VI.

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



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

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

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

## EXPERIMENT VII.

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



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

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

### E X P E R I M E N T VIII.

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

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



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

## E X P E R I M E N T IX.

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

By this experiment it appears, that wax also may be electrified positively and negatively. And it is probable, that all bodies whatsoever may have the quantity they contain of the electrical fluid, increased, or diminished. The clouds, I have observed, by a great number of experiments, to be some in a positive, and others in a negative state of electricity. For the cork balls, electrified by them, will sometimes close at the approach of excited glass ; and at other times be separated to a greater distance. And this change I have known to happen five or six times in less than half an hour ; the balls coming together each time, and remaining in contact a few seconds, before they repel each other again. It may likewise easily be discover'd, by a charged phial, whether the electrical fire be drawn out of the apparatus by a negative cloud, or forced into it by  
a posi-



a positive one : and by whichsoever it be electrified, should that cloud either part with its overplus, or have its deficiency supplied suddenly, the apparatus will lose its electricity : which is frequently observed to be the case, immediately after a flash of lightning. Yet when the air is very dry, the apparatus will continue to be electrified for ten minutes, or a quarter of an hour, after the clouds have passed the zenith ; and sometimes till they appear more than half-way towards the horizon. Rain, especially when the drops are large, generally brings down the electrical fire : and hail, in summer, I believe never fails. When the apparatus was last electrified, it was by the fall of thawing snow ; which happened so lately, as on the 12th of *November* ; that being the twenty-sixth day, and sixty-first time, it has been electrified, since it was first set up ; which was about the middle of *May*. And as *Fahrenheit's* thermometer was but seven degrees above freezing, it is supposed the winter will not intirely put a stop to observations of this sort. At *London*, no more than two thunderstorms 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?





# A P P E N D I X.

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

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

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



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

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

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