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OBSERVATIONS

ON A

SERIES

OF

ELECTRICAL EXPERIMENTS.

By Dr. *HOADLY*, and Mr. *WILSON*,
Fellows of the ROYAL SOCIETY.

The SECOND EDITION.

With ALTERATIONS and the ADDITION of some EXPERIMENTS,
LETTERS, and EXPLANATORY NOTES. By B. WILSON.

Usque adeo magni refert cui quæ adiaceat res.

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L O N D O N :

Printed for T. PAYNE, next the *Mews-Gate*, in *Castle-Street*,
near *St. Martin's-Church*.

MDCCLIX.

ON THE EFFECTS OF ELECTRICITY

IN THE TREATMENT OF THE SMALLPOX

BY JOHN HENRY WILSON

OF THE ROYAL SOCIETY OF LONDON

LECTURES DELIVERED AT THE ROYAL SOCIETY

BY DR. HONDAI, and MR. WILSON

FELLOWS OF THE ROYAL SOCIETY

IN TWO VOLUMES

THE SECOND EDITION



LONDON

Printed for T. Payne, next the Museum-Gate, in Coffin-Street,
near St. Martin's-Church.

MDCCLIX

A D V E R T I S E M E N T.

THE former edition of these observations upon electricity, was published in conjunction with my late worthy friend Doctor Hoadly; by his death, the care of this second edition devolves wholly on myself. It is therefore necessary to observe, that I have thought it expedient to make some alterations, and to add some new experiments. I have been mindful, however, to give my reasons all along for doing this; and yet I am far from imagining that what I even now offer is perfect. The variety of effects which this part of philosophy affords us, and the *nicety* required in making and observing the experiments, is so *great*, that I am sensible of the necessity of still farther improvements before the *data* can be depended upon for settling its principles.

Feb. 12, 1759.

B. W.

A D V E R T I S E M E N T

THE former edition of this work, which was published in 1859, has been revised and enlarged, and is now published in a new and improved form. The author has been enabled to do this by the aid of the latest experiments, and by the discovery of new facts, which have been brought to light since the publication of the former edition. The author has also been enabled to do this by the aid of the latest experiments, and by the discovery of new facts, which have been brought to light since the publication of the former edition.

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IR *Isaac Newton* has supposed that there is a fluid of the same nature with Air, but extremely more subtile and elastic, every where dispersed through all space.

This fluid he calls *Æther*, and supposes it to be much rarer within the dense bodies of the Sun, *Stars*, Planets and Comets, than in the empty celestial space between them; and in passing from them to great distances, it grows denser and denser perpetually, and thereby causes the gravity of those bodies towards one another, and of their parts towards the bodies; every body endeavouring to go from the *denser* parts of the æther towards the *rarer*.

* This alteration in the beginning is chiefly to do justice to Sir *Isaac Newton*; as in the former edition he was made to assert, rather than suppose, the existence of a fluid, but which in the following sheets we shall endeavour to make probable.

The earth, therefore, is surrounded every where by this æther to a very great distance, in consequence of which the air and all bodies in it gravitate towards the earth, and towards each other, agreeably to the appearances at the surface of it.

This æther likewise pervades the pores of all bodies, and lies hid in them: and whilst the bodies with this fluid in them are left to themselves, (undisturbed by any external violence) this fluid from its elastic nature conforms itself, as to its degree of density, to the particular make of that body it is in, *e. gr.* It is not so dense in dense bodies, as in rare ones.

Whence it seems to follow, that every body we have it in our power to make any experiment upon, has naturally within it (before it is disturbed by our experiment) one certain quantity of this fluid, in such a state of rarity or density, as is most agreeable to the nature of each particular body.

And hence it seems reasonable to conclude, that there will naturally arise some resistance to every endeavour; that is made, any how to alter the degree of density in the whole of any body, or in any particular part of it.

And hence, that it will require some degree of force to alter the natural quantity of this fluid contained in every particular body; and more or less force according to the nature, and make of each.

Now, as it is universally agreed among those, who are most conversant with electrical experiments, that the appearances, which occur in those experiments, arise from

from the force and action of a fluid of the same elastic nature, communicating, and freely passing in and out at the surface of the earth, and pervading likewise the pores of bodies: and as the clearest definition of what we mean, when we say a body is electrified, is this, that either the body has by the force of the experiment made in order to electrify it, been forced to part with a share of this electrical fluid, that naturally belonged to it during the experiment, and to remain without it some time after the experiment is over: or to admit more than it naturally had within it, during the experiment, and to remain so overloaded, some time after the experiment is over: *it will be worth our while to enquire whether this electrical fluid, and the æther, be not one and the same fluid.*

In order to be satisfied in this point, let us see in what manner different bodies are thus obliged, on being electrified, either to part with some of this fluid, or to receive more of it.

Now from a very great variety of experiments, we have evident proof given, that there is a resistance made by all bodies against the admission of any more of this electrical fluid into them, than what naturally belonged to them.

2° That there is a resistance likewise made against any of this electrical fluid's getting out of all bodies, and consequently to any diminution of their natural quantity.

3° That this resistance is greater, and less in different bodies.

4° That there is a limit, beyond which we cannot encrease or diminish the natural quantity of this electrical fluid, in each particular body.

5° That when we have thus changed the natural state of this fluid within any body, whether by encreasing or diminishing its quantity, or any other way; there is a resistance, greater or less according to various circumstances, made to the fluid's returning to it's natural state again within that body.

6° and lastly, That there must therefore be some accidental or designed assistance given from without (independent on the body and the electrical fluid contained within it) before they can return to their natural state again.

With regard to the different resistances which are made by different bodies, against being electrified, or unelectrified; from a great variety of experiments, very carefully made, we may depend upon the truth of the following observations, *viz.*

1° That Glafs, Wax, Rosin, Brimstone, and such like bodies, resist the most, provided they are of a sufficient thickness: and Silk, Hair, &c. provided they are of a sufficient length.

2° That next to these sort of bodies the Air resists most, provided there be a sufficient quantity of it, and it be clear and free from vapours.

3° That this resistance is weakest in Metals, Minerals,

rals, Water, Quicksilver, Animals and Vegetables, and at the surface of the Earth.

But, lastly, That the resistance in these last mentioned bodies is greater, when their surfaces are polished and extended in length, and the electrifying power acts on the middle of these surfaces: and less, when their surfaces are rough and short or end with sharp points or edges, and the electrifying power acts at those ends.

These facts, I believe, are so well known at present, that it seems unnecessary to repeat any of the experiments from whence they were deduced. And I shall only observe, that I have ranged Animals, Minerals, Metals, Vegetables, &c. in one class, because there is so very little difference in their resistance, that it is not worth notice.

Now from this variety in the resistances made by different bodies to being electrified, as well as to their returning again to their natural states; there seems to be sufficient direction afforded us, how to dispose the body we want to try any experiment upon, in the manner most suitable to the end, we propose in making the experiment.

For example, Have I a mind to electrify a bar of iron so that it shall, receive an additional quantity of the electric fluid, and make a very great resistance to being un-electrified, or to returning again to its natural state; I consider that silk lines of a sufficient length, kept clean and dry, resist being electrified very strongly; and that air likewise, when it is dry and free from vapour, does
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the same: and therefore that a bar of iron suspended carefully by silk lines, surrounded by air at a proper distance from other bodies, is disposed of in the best manner to remain electrified strongly, after it is once electrified.

And the reason of this is, that the fluid within the bar cannot return to its natural state without part of it is thrown out of the bar; but the silk lines, by which it is suspended, and the clean dry air, with which it is every where surrounded, resist the admission of this fluid within them the strongest of most bodies: and therefore when the bar is once electrified, it is thus disposed in the properest manner to remain so, as all the bodies contiguous to it, will not admit any of this fluid into them, but with the greatest difficulty.

In the next place, I consider that a more extended surface resists more than one less so, and that bodies ending with points hardly resist at all; and consequently, that I should choose a bar of iron extended in length, and having its ends shaped into spherical forms, or ending with large knobs.

And lastly, I consider, that if I take care to have a high polish given to the bar, I shall still give a greater power, when once it is electrified, to resist being un-electrified: which I have supposed to be the drift of my experiment.

But now I have thus made choice of the most proper bar, and disposed of this bar in the best manner in order to produce the greatest effect when it is made to re-
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turn to its natural state; I have evidently placed it in the most disadvantageous circumstances for electrifying it: for the silk lines, and the quantity of air surrounding it, and the particular shape, and polish of the bar, are all of them nearly equal impediments to any of this fluid's forcing itself into the bar, as they are to its forcing itself out.

But the same way of arguing will lead us to the easiest way of electrifying it, in these disfavoured circumstances.

First, I consider that a surface but little extended resists less, than one more so; and therefore, that I shall more readily electrify this bar by taking off the resistance arising from the air from a small part of the surface, than from a larger one.

Secondly, That as metals, especially when they end in points, resist electrifying very little, and consequently part with the electrical fluid most easily; I have reason to conclude, that if I hang on to the bar a small metal wire doubled, with its two ends sharpen'd, and reaching to the electrical machine so as to have those ends in contact with it, as the part where it is doubled is in contact with the bar, I shall on putting the machine in motion most readily electrify the bar; and upon removing this wire, when the bar is electrified, I shall leave it in the best state to resist being unelectrified again.

For this wire, with sharpen'd points, resisting vastly less than the air that surrounds it, very readily admits
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the electrical fluid flowing into it from the machine, and conducts it to the bar, with which it is in contact, and electrifies it; and when the wire is withdrawn, the air closes over the bar, and serves to keep it electrified.

Now the same way of reasoning will show us the easiest way of unelectrifying it again.

For first, I can take off the resistance of the air by approaching any body more capable, than the air, of receiving the electrical fluid from the bar, nearer and nearer to it, even till it comes into contact with it.

Next I am to consider what shape I ought to give this body. If I make it end with a sharp point, it will with the greatest ease receive the fluid that endeavours to get out of the bar, as soon as the equilibrium begins to be destroyed by thus removing the resistance of the air from off that point of the bar, the point of the body is opposed to: and if I stand upon the ground, whilst I do this, I give a free passage to any quantity of this fluid the bar may have to part with, into the earth.

But lastly, I am to consider, that by thus making the discharge of this fluid so easy, it will begin to be made very early in the approach of this pointed body, and the whole be done gradually, and without that violence our design was to produce: because as this pointed body approaches the bar, there lies so small a quantity of air between the point and the bar, that it hardly resists at all.

Consequently, we should try the experiment with some body that ends bluntly and is well polished, and bring it at last with some degree of quickness within the sphere of the bar's action to restore itself to its natural state, in order that it may be forced to do it at once, and not gradually; violently, and not with ease.

And the effects produced answer in all experiments of this kind, which have been made thus either by chance or with design. For in this way the greatest spark has appeared, the loudest snap has been heard, and the strongest feel has been perceived, when the finger's end or knuckle has been made use of to make the discharge.

How these effects are produced on the passing of this fluid so forcibly from the bar through the air to the body brought to it, it is not my purpose at present to enquire into: and I shall therefore only observe here, that as this manner of reasoning *a priori* is so far confirmed by experiment, we have sufficient encouragement to pursue it farther.

My design therefore is to see how far the attending to the variety of these resistances at the surfaces of bodies will serve us to explain the particular appearances in every particular experiment, *i. e.* why in such circumstances this fluid is forced into a body, and retained there; and why in other circumstances part of it is thrown out, and cannot get in again; lastly, in what circumstances the electrical fluid restores itself to its natural state; and when it will do this at once, and violently; and when gradually, and with ease.

In the first case, when the body has more of this fluid forced into it than it has naturally, it is said to be electrified *plus*; and in the other case, when part of its natural quantity is driven out of it, it is said to be electrified *minus*: and in both of these states the body shews the usual signs of being electrified.

I shall begin with the experiments in which bodies are electrified *plus* and *minus*.

Let us now take a bar, that is tapered down at each end to pretty sharp points; and placing it horizontally on a tall drinking glass that is clean and dry; see what effects will follow on bringing an excited tube within the distance of about six inches from the bar (more or less, according to the degree of force given to the excited tube) first towards the middle of it, and next towards either of its ends. *

In the first case, the bar will be electrified *minus*, and in the latter case *plus*; or in other words, in the first case, the bar, when the experiment is over, will have less of this electrical fluid in it than naturally belonged to it, and in the latter case more.

We shall begin our explanation of these experiments with some observations on the first, where the bar is found to be electrified *minus*.

How we know this will be seen by and by.

Now if the bar is electrified *minus*, then some of this fluid, more than was forced into it, must have escaped out of it.

* See Mr. *John Canton's* experiments in the *Phil. Transf.* Vol. 48, page 350.

The question then arises, at what parts of the bar did this fluid escape? and the answer, according to our manner of reasoning is very obvious, *viz.* at the tapering ends of the bar, which we have observed to resist the exit of this fluid through them, less than the extended opposite surface of the bar.

To be satisfied in this point, bring a second bar, shaped as the first was, into contact with the first, end to end, and it will follow from our way of reasoning, that this second bar will, on repeating the experiment, more readily receive what fluid is thrown out of the first, than the air did before; and consequently, that the fluid, which had before been thrown into the air, will now flow into the second bar.

And accordingly, when the first and second bar are placed on two drinking glasses, so as to make one horizontal line, touching one another at their ends; if an excited tube be brought, as before, to the middle of the first bar, the consequence will be, that the first will be electrified *minus* and the second *plus*.

To prove this; before the excited tube is withdrawn, separate the two bars by moving the glass which supports the second bar: then withdraw the tube, and you will find that on approaching your knuckle to them, they will each of them give the usual signs of being electrified.

But if the experiment be repeated again, without thus separating them; and on withdrawing the excited tube they be left for ever so short a time to themselves, nei-

ther of them will shew any marks of being electrified on the approach of the knuckle: because they will each of them have returned to their natural state, the second bar having discharged its overplus of this fluid into the first, which wanted exactly as much to make up its deficiency.

This may be proved to the sight, by repeating the experiment thus. Bring the tube, as before, to the middle of the first bar, and before the tube is withdrawn, separate them: and when the tube is withdrawn, bring them into contact again. The event will be this: A spark will be seen just before they come into contact; and afterwards they will give no signs of their being either of them electrified on the approach of the knuckle, for the reason already assigned.

A plain demonstration this that one is electrified *minus* and the other *plus*, for it is very well known to be fact, that if both had been electrified *plus*, or both *minus*, no such spark would have appeared on their coming into contact, and they would afterwards have given marks of being electrified on the approach of a knuckle towards either of them. The reason of this will appear presently.

And as it appeared above that when the excited tube was applied to the end of the bar, it electrified it *plus*; it is reasonable to conclude that it is the second bar that is electrified *plus*, as the fluid is forced into it through its end; and consequently, that the first is electrified *minus*, as it was when no second bar was made use of: and that

that the only difference in the two experiments is this, that the fluid is now thrown into the second bar, which before was thrown into the air.

We are next to consider what happens at the other end of the bar, when no second bar was used. Now if the two ends be supposed to be equally tapered down to points, their resistance will be equally weak, and the elastic fluid in the bar being put into a state of dilatation near its middle, will equally press both ways to condense or throw out the fluid, that stands the nearest to each of its ends.

To try this, we may vary the experiment by applying in the same manner a third bar, shaped like the others, to the other end of the first, so that they may all three lie in one horizontal line; and bringing the excited tube, as before, to the middle of the first bar, now the middlemost bar.

The event will be this: the middlemost, or first bar, will be electrified *minus*, and the two end ones *plus*.

This may be proved thus. As soon as the excited tube has produced its effect, separate the three bars by moving the two outer glasses, and they will each separately shew signs of being electrified on the approach of the knuckle.

Try the experiment over again, separate them as before: but now bring the two outer bars, by moving their glasses, into contact at their ends; and no spark will appear in doing this, or at least a very insignificant one, when they happen not to have been exactly electrified equally.

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After this, bring first one and then the other of these outward bars in the same manner into contact with the first, and in doing this there will appear sparks, sufficient to give strong proof of their being electrified: and yet after this, if you approach your knuckle to any of them, no signs will appear of their being then electrified; those that had the overplus having readily supplied the want of this fluid in the other, and the three bars having in consequence of this returned to their natural state.

Hence we may conclude, that the quantity of this fluid which had been thrown into the second bar, when only two were used, was divided exactly between the second, and third bar in the present experiment; and therefore in the former experiment, the fluid that was driven out of the first bar passed into the second, and little or none was forced out at the other end into the air.

This is the natural consequence of an elastic fluid's dilating itself, when there is a resistance to its doing it one way, and little or none another way; for it will most certainly flow that way, where the resistance is least.

We may, from what we have seen, be pretty well assured that if we cover these tapering ends of a single bar with hollow pieces of glass that exactly fit them, we shall in all probability prevent the fluid from passing out through these ends; and accordingly the experiment answers. For when the excited tube is brought to the same distance from the middle of the bar, as before; the

the bar, now its end are thus fortified with glafs, will be electrified *plus*, instead of *minus*, even though the tube be withdrawn: which shews how much more strongly the extended surface of the bar, opposite to the excited tube, resists the fluid's escape, than the tapered ends did.

So much (at present) for the first experiment, in which the tube was presented at a distance to the middle of a single bar, and electrifies it *minus*.

In the second experiment, when the tube was presented at the same distance to the end of the bar, it was electrified *plus*, and therefore more of this fluid was thrown into the bar at one end, than was suffered to pass out at the other: which was reasonably to be expected, as the tube, acting with no more force than it did before, is now at double the distance from the end, at which the fluid is to escape; and therefore has a greater resistance to overcome before any can be thrown out at that end.

But yet, that some of this fluid does escape out at the opposite end, will appear from bringing a second bar into contact, end to end, with the first; for then, on repeating the experiment, they will both be electrified *plus*: and on bringing a third in the same manner, end to end with the second, they will all three be electrified *plus*.

But then the third bar will be electrified *plus* less forcibly than the second, and the second less than the first.

By which it should seem, that if a fourth bar, and a fifth, and a sixth, &c. were in the same manner added

to the string of bars; the most distant bar, from the excited tube, would at last not be sensibly affected at all by it; and consequently, that the virtue of the tube is limited, and can affect the fluid in these bars to a certain distance only.

But this will be made more evident hereafter.

Let us now reflect a little on what we have seen with regard to the air, surrounding the bars in these experiments.

First, We have seen that although air makes a strong resistance against being electrified, *i. e.* against admitting more of this fluid into it than what naturally belongs to it; yet the excited tube does overcome this resistance, and forces the fluid within the bar through its ends into the air, in the experiments with a single bar, both when it was electrified *minus* and *plus*.

2° That the quantity of this fluid, forced thus into the air in the first experiment, was sufficient to electrify the two bars *plus*, that were applied to its ends on repeating that experiment.

3° That none of this fluid was dissipated in the air, and so lost, because on withdrawing the excited tube, and leaving the three bars to themselves, they shew no signs of being electrified at all.

It should seem therefore, that when a single bar is thus electrified *minus*; the fluid thrown out of it into the air stands in that portion of air that surrounds the ends of the bar all the time the excited tube continues acting; and that, when the tube is withdrawn, it re-

turns

turns again into the bar, till such time as the resistance at the ends is a ballance to the force, with which it endeavours to flow in. But as the bar remains electrified *minus* after the experiment is over, it is evident that all that was thrown out does not return in again; and therefore that the remainder forms itself into a kind of atmosphere every way surrounding the bar with a nearly equal degree of density, from the nature of the two elastic fluids, air, and the electrical fluid, and of the resistance that fluid meets with in its endeavour to dilate itself either into the air, or into the bar.

4° We have likewise seen, that when a single bar was electrified *plus*, there was likewise a sufficient quantity of this fluid to electrify two bars applied to its other end.

It follows therefore, that when a bar is electrified *plus* by itself, and continues so after the tube is withdrawn, the fluid that was thrown out of the bar must remain there, and form the same kind of atmosphere round the bar, as it does in the other case, when it was electrified *minus*.

Whenever therefore a Body is electrified either *plus*, or *minus*, and remains so after the experiment is over, there are similar atmospheres of the electrical fluid surrounding them, that are ready to expand themselves into any body that approaches, that resists less than the air: and this is the reason why bodies give very nearly the same signs, when they are electrified either *plus* or *minus*.

I say, very nearly, because when we come to examine the signs these bodies give (on being electrified in these different ways) with greater accuracy, we shall see a sufficient difference in these signs to enable us to say which were electrified *plus* and which *minus*.

It will be worth while to stop here a little, and consider the different circumstances the electrical fluid is in, that forms these similar atmospheres around a body electrified *plus*, and one electrified *minus*.

When a bar is electrified *plus*, the * atmosphere formed round it is extended in the air to a considerable distance; but the air resists its entrance into it more forcibly than the bar naturally does: and the bar, by being overloaded by electrification, resists its return into it, (even when the excited tube is withdrawn) more forcibly than it does naturally; and by being thus circumstanced, its endeavour to expand is naturally exerted outwards from the axis of the bar on every side, and it gradually dissipates itself into the air, and whilst it is doing this, and no longer, the bar will remain electrified *plus*.

When a bar is electrified *minus*, the atmosphere formed round it, which during the action of the tube that electrified it, ** is extended in the same manner in the air to a considerable distance from the bar, will,

* The electric atmosphere is not to be understood as lying between the air and the bar, as it is express'd in the former edition, but extended in the air all the way from the bar.

** This alteration was in consequence of the former.

on that tube's being withdrawn, exert its endeavour to dilate itself in a contrary direction, *viz.* from the surrounding air on every side inwards to the axis of the bar; and it will gradually flow into the bar, and reduce the electrical fluid there to its natural degree of density; and whilst this is doing, and no longer, will the bar remain electrified *minus*.

When therefore two balls are both of them electrified *plus*, suspended by two silk lines, and brought near one another; they repel each other, and stand for some time at a distance from each other; because the two atmospheres, each of them exerting their endeavours to expand into the air, want more room to do it in; and when the weight of the balls is not sufficient to prevent it, must naturally drive them asunder, till these atmospheres are dissipated, and the weight of the balls takes place again.

And when two bodies are both of them electrified *minus*, suspended by silk strings, and brought near one another, they likewise repel each other, and stand for some time at a distance from each other; because the condensed electrical fluid in the air, in order to force itself in at the surfaces of the balls between their two centers, crowds in, and forces them asunder, till the atmospheres get all into the balls, and their weight then takes place again.

But when two balls are in the same circumstances, one electrified *plus*, and the other *minus*, and brought near one another; they will gradually come together

and unelectrify each other. Because the atmosphere of the ball electrified *plus*, is endeavouring to dissipate itself from the center of the ball outwards; and the atmosphere of the ball electrified *minus*, is endeavouring to dilate itself from the air inwards to the center of the ball. The common atmospheres therefore of the two balls, thus brought near together, exert their forces in one and the same direction between them; the flow of the electrical fluid into the ball electrified *minus*, is facilitated by the endeavour of the electrical fluid to get out of the ball, electrified *plus*; and *vice versa*: and so the two balls and the air between them very readily return to their natural states. *

Thus then it appears, that when we only know that a body is electrified, without knowing in what manner it was electrified, there is no criterion to form our judgment upon whether it was electrified *plus*, or *minus*: because the common appearances are the same in both cases, when we unelectrify it.

* The reader is desired to take notice, that though two electrical atmospheres seem necessary in this experiment to restore the equilibrium in the balls, yet he is not to conclude therefore that they both enter the balls, because the electrical signs of both disappear at the same time. But this seems to follow from the principles we have already deduced, that a quantity, equal to what was in the ball electrified *plus*, is at the end of the experiment diffused in the balls electrified *minus*; and the remaining quantity which form'd the atmospheres is diffused in the air, or the bodies from whence it at first flow'd, in order to crowd into the body electrified *minus*. For if this was not the case such bodies, or the air, would remain in an unnatural state, because the equilibrium of the electric fluid in such circumstances would not be restored.

But

But by pursuing the same train of reasoning; we shall readily obtain a certain method to know whether a body is electrified *plus*, or *minus*, even without unelectrifying it.

Fasten two cork balls one at each end to a piece of thread about eight inches long, and, doubling the thread over the bar before it is electrified, make the balls hang as near to one another under the bar, as they can; and now the bar, the threads, and the balls, should be considered as one body equally ready to be electrified either *plus*, or *minus*.

1^o We will suppose this bar, &c. to be electrified *minus*, and in consequence of their being electrified at all, the balls to repel one another, and hang at a greater distance from each other, than they did naturally.

Now let an excited tube be brought to a certain distance under these balls in these circumstances, and they will at first repel each other more; because the force of the excited tube will condense the atmospheres around the balls still more, on account of the resistance of the air, till the resistance at their surfaces is overcome, which will take up some little time, during which, their atmospheres being encreasing, they will repel each other more than before the tube was brought near them.

But so soon as ever this resistance is once overcome, the excited tube drives the atmospheres into the balls, &c. and consequently begins to unelectrify them, *i. e.* to render them less forcibly electrified *minus*: and on
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withdrawing the tube, the balls will hang very visibly nearer together than they did before the tube was first brought near them.

2° We will suppose the bar, &c. to be electrified *plus*, and in consequence of their being electrified at all, the balls to repel each other.

Now when an excited tube is brought to a certain distance under the balls in these circumstances, they will repel each other less forcibly and come nearer together; because the resistance of the air alone to the electrical fluid's escaping out of the balls electrified *plus*, has been seen not to be able entirely to prevent it, as an atmosphere is made, and supported round the balls, till they are reduced to their natural state. But now the excited tube acts in concert with the air, this atmosphere, which had prevailed against the air alone, must, on this additional force acting against it, retire again into the ball, and continue to do so for some small time.

And after it is all retired into the balls again, so long as the bar, &c. can be electrified more and more *plus*, this appearance will continue, whilst the tube remains in action: and when the tube is withdrawn, the balls will repel each other more forcibly than at first, because they will remain more forcibly electrified, *plus*.

Thus then it appears that though the balls repel each other when the bar is electrified either *plus*, or *minus*, yet when an excited tube is brought near them in this their repulsive state, they will in one case have their re-
pulsive

pulsive force encreased at first, but very soon after gradually diminished : and in the other case, diminished at first, but very soon after encreased.

And when we see the appearance in the first case, we may safely conclude the bar had been electrified *minus*; and when we see the appearance in the other case, we may conclude the bar had been electrified *plus*.

This, therefore, is an easy and ready way of trying, when there is any doubt about it, whether any body is electrified *plus*, or *minus*. For the string, with the balls affixed to its ends, may, with the assistance of a glass tube, be easily hung on the bar without unelectrifying it, supposing it to have been electrified at first without the string and balls.

I cannot help observing here, that in the first case, after the excited tube has been held so long near the balls, that on its being withdrawn they no longer repel each other; if it be again presented to them, it will electrify them and the bar *plus*, and they will be put into a repulsive state again.

Whence it was reasonable to conclude, that if the excited tube, in our first experiment, after it had been presented at a proper distance to the extended surface of the bar, and so had electrified it *minus*, had been brought nearer and nearer to the bar, so as at last to touch it, it would have electrified it *plus*.

And accordingly, when the experiment was tried, it succeeded, and the bar was electrified *plus*.

Now

Now the consequence of this is, that there must be some middle distance of the excited tube from the bar, between its situation, where it electrified it *minus*, and its situation, where it electrified it *plus*; at which middle distance, the bar will be reduced to its natural state, and not be electrified any more, than if the tube was not there.

This would appear a most amazing paradox in electricity, if it was told in general terms to any one, who did not know that bodies were capable of being electrified *plus* and *minus*: *viz.* the same excited tube brought near a body electrifies it; and after that, (without ever withdrawing it) brought nearer, ceases to electrify it; and after that, brought still nearer, electrifies it again.

Now as this was deduced from our way of reasoning, the event, answering on making the experiment, greatly confirms the truth of the reasoning.

But I have not yet done with the cork balls, as they will be very serviceable in proving, that the power of the excited tube (or other electrifying machine) is limited, and can electrify bodies but to a certain degree, at one and the same distance from those bodies.

This I hinted at before, and promised to shew more fully: and in order to do it, I shall use these balls as indexes to shew us when, and to what degree the bar, to which they are fixed, is electrified: which end they will exactly answer, because they will repel each other with more or less force, according to their greater or less degree of electrification.

Let

Let the balls, therefore, be hung at the end of the bar, and the excited tube be brought to a certain distance from the middle of it, and held there steady and firm, and let us suppose that at this distance it electrifies the bar *minus*.

Now I say, that some little time is taken up, during which the tube electrifies the bar *minus* to a certain degree; but that when it has once done this, it will produce no farther effect on it, though it remain at that distance ever so long, and be supposed to lose none of its force.

Now this is shewn to be true in fact by trying the experiment thus with the cork balls; for you will see, on bringing the excited tube to this distance from the bar, the balls will begin to repel each other, and continue to do so more and more, till they stand at a certain distance asunder, and then they will continue so, without ever getting farther asunder, how long soever the tube is held at that distance.

This is a proof of my assertion, that the Power of the excited tube is limited, and therefore can act but to a certain degree in electrifying bodies, which will be greater or less, according to the circumstances of each particular experiment.

But this will be confirmed by the following very remarkable set of experiments.

Let a glass tube be hermetically sealed at one end, and have its other end properly armed with brass, cemented into it; and let proper contrivances be made in that

E

brass,

brass, so that the tube may be readily fixt to the air-pump and exhausted of air; and afterwards removed from it, and still remain exhausted.

1° Let this tube be fixt to the air-pump in order to be exhausted of air.

Now before this is done, the outer and inner surface of the glass tube are equally exposed to the air; and consequently, the powers of these two surfaces are in equilibrio, even supposing the air to affect them.

2° Let the tube be exhausted of air, as perfectly as it can.

When this is done, the outer surface remains, as before, exposed to the air; and the inner surface is exposed to the electrical fluid, which I will suppose to be naturally dispersed in empty spaces void of all gross bodies, (as the vacuum is thought very nearly to be) as well as in the pores of gross bodies.

And the equilibrium is still preserved between the powers at these surfaces: as we may conclude from there being no visible or perceptible alterations observed to be produced on drawing the air out of the hollow of the tube, either in the light, or in the dark.

Whence we may conclude that the air, when it has its natural quantity of this electrical fluid only in it, does not affect this tube at its outer surface, more than the fluid within it does at its inner surface.

3° Let the exhausted tube be taken off the air-pump: and let a person grasp the brass end of it with his hand, whilst he stands on the ground.

4° Let another person bring an excited tube near the outer surface of any part of this exhausted tube (suppose near the end that is hermetically sealed, for this reason only, that it will then be at a distance from the hand that grasps the other end of it.)

There will immediately appear lucid rays of light, very visible when the experiment is made in the dark, proceeding from the inner surface of that part of the tube nearest the excited tube, and darting through the vacuum to the brass, grasped by the hand.

5° And if the tubes are held steady for some length of time, so as to be kept at the same distance from each other; this lucid appearance will cease, and the light totally disappear.

6° And when the light has thus disappeared for some time; if the excited tube be withdrawn entirely, the rays of light will appear again; but they will now be seen darting from the brass end of the exhausted tube through the vacuum to the inner surface of that part of the tube, from which they had proceeded before: and this darting of the rays from the brass end of the tube will continue for a time only, as the other had done before.

7° And when the excited tube is brought nearer, and nearer to the exhausted one, as the light disappeared, there only appears a fresh darting of rays, similar to the first, which likewise continues for a time only: and on the removal of the excited tube, the same kind of light, somewhat stronger and in greater quantity, returns from

the brass end of the tube, as before, and disappears again after some little time. *

In this experiment there is the strongest appearance of the electrical fluid's shifting its place in order to preserve the equilibrium, by flowing immediately, when any force is offered to the outer surface, from its inner surface through the vacuum, and darting directly to the metal and hand, where it finds the resistance to its escape is the weakest.

Now though the resistance is weakest, where the brass and hand is applied to the glass; yet there is a resistance; and consequently, the electrical fluid in the vacuum must have some additional force given to it from that of the inner surface of the glass which is nearest to the excited tube, *i. e.* must be condensed till it acquires a power sufficient to overcome the resistance of the brass, &c.

But this fluid within the exhausted tube cannot be condensed, without more passes into it immediately from that inner surface, and consequently so long as the electrical fluid is continually flowing from it into the vacuum, and so into the brass, &c. so long will it be losing its natural quantity, unless its loss of this fluid be supplied by a flow of more from the excited tube.

I shall now, therefore, endeavour to shew, that the loss of the electrical fluid flowing from the inner sur-

* Mr. J. Canton has taken notice of this vanishing and returning of the light in the same experiment, referred to in page 10.

face of the glass, is not supplied by a similar flow of it from the excited tube to it, through the outer surface; and therefore, that the glass is electrified *minus* at or near that part of its inner surface.

Now this will follow plainly, if I can shew, 1° That no electrical fluid flows into the glass so far as to reach that inner surface; and, 2° How part of the electrical fluid naturally belonging to it can be driven out into the vacuum, without such a flow of the fluid from the excited tube through the glass to get at it immediately.

In order to prove the first of these, the best way seems to me to be, to suppose the contrary, that the electrical fluid does flow into it, and supply it continually, and to enquire what would be the consequence of such a supposition at the end of the experiment.

If upon this enquiry, we find that the appearances answer in fact to what we ought to expect from this supposition, there will be no reason to reject it. But on the contrary, if the appearances are against it, it must be given up.

Now if there was such a flow of this fluid from the excited tube through the whole substance of the glass to the very inner surface of it, so as to supply the very quantity it drives out through it; the glass at this inner surface must always have its natural quantity in it; and consequently, when the excited tube is withdrawn, remain in its natural state, equally capable, as at first, to resist the entrance of any more of this fluid into it.

And

And therefore, on withdrawing the excited tube, there would be no such return of light from the brass, &c. darting so particularly towards that part of the inner surface of the glass, as appears in the experiment.

So far then the appearances in the experiment do not answer agreeably to what we should naturally expect from the supposition.

Again, if the excited tube supplies the inner surface of the glass with as much of this fluid as passes through it into the vacuum; it will follow, that as long as the excited tube has any power, there must be a continuation of this lucid appearance in the vacuum; but this is by no means true in fact, that appearance vanishing long before the excited tube has lost its power; as was seen above.

Here then is another remarkable instance, in which the appearances do not answer on the trial to what was reasonably to be expected from the supposition.

And therefore we ought to conclude that no electrical fluid passes from the excited tube so far into the glass, as to reach its internal surface and supply it with any recruit of those particles of this electrical fluid, which are thrown through it into the vacuum: and consequently, that this inner surface remains electrified *minus* during the continuance of the excited tube near the glass, after the lucid appearance has ceased; and stands ready (on withdrawing that tube, or on its power's decaying) to receive a supply of what it had lost from the equilibrium being restored through the person, his hand,

hand, the brass and the electrical fluid that is in the vacuum.

But that no doubt may remain in an enquiry of so delicate a nature, I will propose making the experiment in a more accurate manner.

Every one knows that a bar of iron, suspended by silk strings in the air, may be kept equally electrified, or very nearly so, for any length of time, by the electrifying machine's being kept equally in motion, during that time.

Now if instead of approaching an excited tube of glass (which is continually losing its power) to the exhausted tube, as I did before; I now approach the exhausted tube to the electrified bar (whose electrifying force is constantly kept up at the same degree by the machine) to such a particular distance, as to make the lucid appearance begin; and then keep it there steady: I gain this advantage, that it continues during the whole time I keep it thus steady at one certain distance, exposed to one and the same degree of force in the electrified bar, or very nearly so.

And on trying the experiment thus, I found that the lucid appearance continues only for a limited time; and if the tube was to be held so ever so long, there would be no return of that appearance: but on removing the exhausted tube from the bar, the light returns again immediately, shooting then from the brass and hand to that part of the inner surface of the tube, that had been nearest to the bar.

No

No experiment can be more to the purpose, than this; and we may now fairly conclude, that the electrical fluid does not flow in so plentifully, or so forcibly as to reach the inner surface of the glass, and so supply it with a quantity equal to what is thrown off through it into the vacuum, on the first approach of the excited tube in the first way of trying this experiment, or on the first approach of the exhausted tube to the electrified bar, in the last way.

It remains therefore to be shown how this inner surface, and the part of the glass nearest it, can be so affected, as it is, without so strong a flow of this electrical fluid from the tube or bar.

In order to be more plainly understood, I shall observe, that the electrical fluid within the hollow of the exhausted tube is in the same circumstances with that in the first bar, when a second bar was brought into contact with it end to end: and that this second bar is represented in this experiment by the brass and the person holding it. Now as the excited tube, applied to the middle of the first bar, dilated the electrical fluid that lay nearest it with sufficient force to drive that at a distance into the second bar: so in this experiment, the excited tube has power enough to dilate the electrical fluid at the inner surface of the glass near it with sufficient force to drive the distant part of it in the vacuum, into the brass, and the person, who holds it: though it has not power to force any external electrical fluid into the glass itself.

But

But this power is limited, and the excited tube can dilate this electrical fluid in these circumstances but to a certain degree; and this it must do gradually: and consequently, during the time of doing this, there will be a flow of this fluid from the inner surface of the glass to the brass, &c. which manifests itself by a stream of light.

And when the electrical fluid in the brass, hand, &c. is sufficiently condensed to ballance the force of the dilating fluid in the vacuum; no more of it will be driven out, the flow of it will cease, and the lucid light will disappear.

So long therefore (after the light disappears) as the excited tube is held at the same distance, and acts with the same force; the electrical fluid within the vacuum and the brass, &c. will be kept in equilibrio, the electrical fluid nearest the excited tube being all that time forced to continue in that degree of dilatation by the continued force of that tube, and so to keep the more distant part of the fluid near the brass in the same degree of condensation. For surely the same power, that is able at first to put them into these different states, must be able to keep them in those states, so long as it acts with the same force in the same circumstances.

But on withdrawing the tube, the force of the condensed part of the fluid in the brass drives that in the vacuum back again to the inner surface of the glass, where the tube had been applied; and consequently, there must be a returning flow of this fluid from the brass to the glass, which will manifest itself by a similar

F

appearance

appearance of light shooting from the brads to the glass; and this will last no longer than till the equilibrium is restored.

And this effect is similar to what happens in the two bars when they are left to themselves after the tube is withdrawn, the equilibrium here too being very soon restored by the condensed part of the fluid in one bar's gradually flowing into the dilated part in the other.

And consequently these two experiments illustrate each other, and the reasoning thus on them both together is sufficient to convince us, that the electrical fluid within the substance of the glass at its inner surface is forced into a state of dilatation by the power of the excited tube at a distance: which it is in this case enabled to perform by there being no resistance of air at that surface to keep that fluid from dilating.

From these experiments we are naturally led to attempt an explanation of the appearances that occur in electrifying and unelectrifying a large pane of glass in a similar manner to that in which the *Leyden* bottle was *accidentally electrified* at first. Before the appearance, that happened on unelectrifying that bottle: no one imagined that glass could be electrified any other way, than by being *actually rubbed*. But experience since has taught us otherwise, and that in order to electrify glass so as to produce these appearances with considerable force, it is necessary there should be extended over the two opposite surfaces of the glass equal coverings of metal, or some such body as easily electrified; but how these coverings
contribute

contribute to that extraordinary effect has not hitherto been satisfactorily explained.

We will suppose then an oblong pane of glass to be covered in the middle of its upper surface with leaf gold, somehow kept in contact with it, of the same shape with the pane of glass, but no where reaching within two, or three inches of its edges: and that the same be done on its under surface, so that the under-covering may lie directly under the upper-covering.

The use of the upper-covering is this. As every part of it, from the nature of metal, is equally electrified at the same time that any one part of it is; the whole extended surface of the glass immediately under it lies equally exposed to the action of the electrical fluid, when that cover is electrified.

Let us then suppose that a Communication is made by a wire from the electrifying machine to this upper covering: it will follow then, that, whenever the machine is put into motion, the electrical fluid will be directed down the wire into the upper covering, and when there, will exert its full force against its whole extent of the glass immediately under it: and that this force will be greater than the force of an excited tube on the same surface of glass would be, when it was only brought near it, because the flow of the fluid to the glass is less resisted by the metal wire and covering, than it would be in the other case by the air it must pass through to get at it; and this is another use of the covering.

FIG. 2. Let

Let us in the next place suppose a communication to be made by another wire between the under covering and the ground, it is plain on a supposition that the glass was electrified, that great part of the resistance to the escape of the electrical fluid through the under surface of the glass will be taken off, as metal resists so much less than air, and all the whole surface of the glass immediately over this cover can discharge its fluid into the wire at once, and so into the ground. This therefore is the use of the uncovering and its wire.

Let us next attend to the glass. We are to consider no more of it, as to its being electrified, than what lies immediately between its two coverings; for the rims of the glass, that are left uncovered and exposed to the air, serve to prevent any of the electrical fluid's passing through the edges or points of the pane of glass or from one cover to the other.

Now this parallelopiped of glass (when no communication is made with the ground) seems to be much in the same circumstances with the bar when its two ends were armed with glass; and we saw the bar was in those circumstances electrified strongly *plus* when the excited tube was brought into contact with it, and so had no air between it and the bar to resist the entrance of the fluid into the bar.

Though the resistance is not so entirely taken off, in the present case, as if an excited tube was in contact with the glass, yet it is greatly diminished by the electrical fluid's being conducted thus to the glass through metal,
without

without passing through air at all: and therefore we have reason to believe the glass is put into the most favourable circumstances to be strongly electrified, from every thing we have yet seen in electrifying of metals.

But experience has shewn us the contrary, and that glass cannot be so electrified; but that the resistance must be taken off from its under, as well as from its upper surface. It is therefore contrived, by placing the covers and wires as we have done, that the escape of the fluid may be as much promoted through the under cover by the wire that communicates with the ground, as the entrance of it into the glass through the upper cover is by the wire that communicates with the machine.

We shall now, therefore, propose an experiment on the pane of glass very nearly in these circumstances.

Let the pane of glass, covered thus on each surface, be supported by four drinking glasses, one under each corner; and let a finger be held within an inch of the under covering by a person standing on the ground; and let the wire, which communicates with the electrifying machine, reach likewise within an inch of the upper covering, and end with a knob, as large as the finger's end.

By thus bringing the finger, and the wire almost, but not quite into contact with the covers, we gain this advantage, that when any electrical fluid flows from the wire into the upper covering, or from the under covering into the finger, it must pass through an inch of air
and

and so give a visible proof of its doing so, by producing a spark.

If now the machine be set in motion, there will a spark be seen issuing from the wire, and at the same time an equal spark from the under cover; and then another and another from the wire, accompanied at each of the times with an equal spark from the under cover; but these sparks will continue thus succeeding each other from the wire and under cover for a certain time only, and then cease, notwithstanding the machine be kept equally in action for ever so long a time, and the wire and finger be kept in the same places.

Now from this experiment it should seem, that whatever was thrown in at the upper cover passed out again immediately in equal quantities at the under one, and therefore, that the electrical fluid has a free passage through the pores of glass, and meets with no resistance; and consequently, that the glass is not itself electrified in these circumstances; and indeed, if after these sparks have ceased appearing, and both the wire of communication and the finger are removed; a finger be brought to either covering at different times, there will none or very slight signs appear of the glass's being electrified.

But if any one, who argues from these facts that the glass is not electrified, will hold one finger in contact with one cover, and then approach a finger of his other hand to the opposite cover, he will receive so severe a stroke, as will convince him that the glass was very
strongly

strongly electrified ; though before it gave little or no sign of it, in the manner metals do ; which part with their overload readily wheresoever they are touched by a body that is not electrified itself.

The fact however is undoubtedly true, that as much of this fluid is thrown out of the glass at the under surface through the under covering into the finger, as is thrown into it from the wire at its upper surface through the upper covering ; but the reasoning upon this fact is evidently false, as it contradicts experiment.*

Let us see how our doctrine of resistances will enable us to solve this difficulty.

Let us call the resistance at each surface of the glass, six ; by which I mean that the upper resists the entrance of the fluid into the glass, and the under resists its exit out of it ; with the same force, equally to six : and that the machine acts with a force equal to nine.

Then will six of this nine be ballanced by the resistance, six, at the upper surface, and the fluid will be forced into the glass with the force, three only ; and consequently, when the machine has exerted this force, nine, and condensed the electrical fluid within the glass to such a degree, as the force, three, can condense it to ; it can do no more ; because the resistance at the surface, six, and the expansive power of this fluid, three, (which, from the nature of an elastic fluid, endeavours, wherever it lies, to expand itself equally every way with the

* See Dr. Franklin's letters upon this subject.

same force with which it is condensed) added together makes nine, *i. e.* is an exact ballance to the force of the machine.

Now this fluid, thus forced into the glass, endeavours immediately to expand itself every way, as soon as ever it begins to be thrown into it, towards the edge and corners of the glass, as well as towards the opposite surface.

But these edges and corners of the glass are every way so strongly guarded by the length of the rim that is purposely left exposed to the air, that the fluid is prevented to escape through them by the resistance of the air and the thickness of the glass it has to pass through; whilst the under surface is only supposed to resist with the force, six, and is purposely cut off from any assistance from the air: and consequently from the time, that any of this fluid is thrown into the glass at the upper surface till all is thrown in (that the force, three, can throw into it) as much will be thrown out at the under surface, where the resistance is least; or in other words, the resistance at this surface will gradually be reduced to three, and then there will be an equilibrium between all the forces.

Thus we have seen before, in the experiment of the exhausted tube, that the excited tube, when kept at a certain distance from it, had a power to drive the electrical fluid out at the inner surface of the glass, though no electrical fluid passed so far into the glass as to reach that surface; and therefore we may here imagine it possible,

possible, that the excited fluid penetrating thus to a certain distance within the upper surface may act at a distance on that near the under surface, and so force it out, till there is a ballance between the expansive force of this condensed fluid, and the resistance at the under surface: and thus the resistance fix at the under surface, will be reduced to three; just as the force, nine of the machine was reduced to three, by the resistance, six, at the upper surface of the glass: and all will remain in equilibrio.

Consequently, all the while the machine is condensing the fluid within the glass near its upper surface, it is likewise dilating the fluid, naturally in the glass, near the under surface; which it cannot do without forcing some of it out: and therefore it is reasonable to expect from this doctrine, as the wire and finger are not in absolute contact with the covers, that sparks of fire should continue to appear at both surfaces during this time, and no longer; and that the instant they disappear, the fluid within the glass exactly ballances the fluid without.

For the machine, never acting with more force than nine, has this force ballanced by six, the resistance at the upper surface, with the additional force, three, of the condensed fluid near it; and the condensed fluid acting the contrary way towards the under surface with the same force, three, is ballanced by the remaining force, three, at that under surface.

Now the only difficulty here arises from our want of conceiving how a quantity of elastic fluid can lie condensed

condensed in one part of the glass, whilst its neighbouring fluid in another part is attenuated; but when experiment is strongly on one side, and nothing but this difficulty of conception appears on the other, the latter ought to give way: however, that we may not rest too much upon this, we will pursue our enquiries farther, and try to remove this difficulty.

To explain how experiment is strongly on one side, we must consider the thing more carefully, and compare the present experiment with that of the exhausted tube.

In the experiment of the exhausted tube, we saw that the excited tube acted at a considerable distance on the glass of the exhausted tube, so as to produce a lucid appearance in the vacuum for a time; and on withdrawing the excited tube the lucid appearance returned again, but in a contrary direction.

Let us now repeat our present experiment, and try what will be the event, if we withdraw the wire of communication with the machine, as soon as the appearance of sparks ceases; keeping the finger still at the same distance from the under cover.

The event is this. No return of sparks is perceived.

This therefore seems to be an experimental Proof, that there is some power remaining *in the glass itself** that

* N. B. When at any time we mention the electric fluid as being condensed in the glass itself, I would be understood that such condensation may be made *at* or exceedingly *near* the surface; but, supposing the latter, whether near the inside or outside thereof, I cannot yet say. However, I do not despair of making some experiments which

that prevents the return of these sparks, or rather prevents the return of that fluid into the glass (which had before been thrown out into the finger) which would have produced the sparks.

It seems therefore that the force of the machine was the cause of the condensation of the fluid within the glass near its upper surface, notwithstanding the resistance at that surface; and that this condensed fluid was the cause of that at the under surface dilating itself and passing out into the finger, notwithstanding the resistance at the under surface.

Now if on withdrawing the wire of communication, this condensed fluid could dilate itself and pass out back again at the upper surface, the fluid would likewise return from the finger into the glass at the upper surface and a spark would appear: but the resistance at the upper surface, six, prevents this escape of the condensed fluid out of the glass, as it endeavours to dilate itself only with the force, three.

Consequently, the condensed fluid within the glass near its upper surface is the power, remaining in the glass, when the wire of communication was withdrawn, that prevents the return of the sparks.

Now in the experiment of the exhausted tube, the excited tube (having not sufficient power to condense

which will determine what the fact really is. I am the more particular in this part, as Dr. Franklin's notion differs considerably from this, as may be seen in his letters, page 72 to 75, for he supposes a kind of partition in the center of the glass, which being of a closer nature than any other part thereof, suffers the electric fluid entering at one surface, to pass no farther than to half the thickness of the glass, and is there condensed.

any fluid within the substance of the glass of the exhausted tube) must, when it is withdrawn, take away with it all the excited fluid it brought with it, standing as an atmosphere about it: and therefore the lucid appearance returned, and this is the reason of the difference between these two experiments.

Upon the whole, we know by experiment that the pane of glass in these circumstances remains electrified; and we have all the reason in the world from the equal appearance of the sparks, to believe that as much fluid passes out at one cover as passes in at the other; and therefore that the electrification of the pane of glass does not arise from its whole quantity being either increased or diminished, as it does in metals, and other bodies.

There remains therefore but one way of explaining whence this electrification arises, and that is, the continuation of the condensed fluid in the same situation at the upper surface, after the wire was withdrawn, and its continuing to act with the same force, as it did when the machine was in action; and therefore that the fluid must remain denser near the upper surface, and rarer near the under surface of the glass, than it does naturally in unelectrified glass: or to return to our former manner of expression, that the upper surface of the glass is electrified *plus*, and the under surface *minus*.

There is something similar to this state of the fluid, in the experiment of the three bars in contact with each other, when the excited tube was applied at a certain distance from the middle of the middlemost bar: For
whilst

whilst the excited tube continues there, the middle bar is electrified *minus*, the others, *plus*. May not the three bars, as they are in contact with each other, be looked on as one bar? and will not the fluid stand denser in the outer bars, and rarer in the middle one, than it did naturally, all the while the excited tube continues its action? So far then the disposition of the fluid in these bars, considered as one body, is similar to that of the fluid in the pane of glass.

But when the excited tube is withdrawn, the fluid immediately returns to its natural degree of density in all the three bars: because there is no remaining force to resist its doing so.

In the pane of glass, this return of the fluid to its natural degree of density within the whole glass, is prevented by some resistance remaining at each of the surfaces after the machine is withdrawn, and acting with sufficient force to ballance the endeavour of the fluid to do so. So long therefore as this resistance continues, so long must the fluid continue in this unnatural state.

* Now let us again compare (but more particularly) this unnatural state of the fluid, in the pane of glass, with the experiment of the exhausted tube, when exposed to

* The reader will please to observe, that in the other edition, the reasoning upon the electric atmospheres, is not altogether agreeable to experiment. I say, *not altogether*, because though there are really such electric atmospheres extended over each surface of the glass, yet they are neither so powerful nor so extensive, as they are there supposed to be. I have therefore chose to vary and illustrate that part, which relates to the experiment, by a farther analogy between the exhausted tube and the pane of glass, as it appears to be the clearest and most satisfactory method. And I do think if my worthy friend had now been living, he would readily have concurred in this alteration.

the

the excited one, which we have more than once mentioned before in this work, and we shall imagine have all the visible proof of their being in the same state that an experimental analogy is capable of affording us.

And first, I consider that when the excited tube is held at a certain distance from the exhausted tube, and where it exerts its full force against the fluid within, next the inner surface of the same; it is exactly analogous to that side of the pane of glass electrified *plus*, exerting its full force against the fluid on the opposite side thereof. In the first case the greatest quantity of light is seen to dart away through the vacuum without any return. And in the second case the opposite side is electrified *minus* without any return.

And farther, so long as the tube is held at the same distance from the exhausted tube, just so long the light ceases to be seen.

In the other, so long as the *plus* side continues in the same electrified state, just so long does the *minus* side continue electrified in the same degree.

Secondly, Weaken the force in the first case, by withdrawing the excited tube from the exhausted one, and a proportionable quantity of light returns.

Weaken the *plus* side of the pane of glass, by drawing away a little of the fluid with a pointed body, and the *minus* side will be weakened in the same proportion.

Thirdly, Increase the distance of the tubes from each other, and more light will return.

Draw more of the fluid from out of the *plus* side, and the *minus* will be weakened accordingly.

Fourthly,

Fourthly, Withdraw the excited tube intirely from the other, and the remaining quantity of light will return, but after that there will be no more such appearance.

Draw of all the *plus* electricity, and there will remain no sign of a *minus* electricity on the other side.

Fifthly, the same lucid appearances happen as you approach the exhausted tube with the excited one at the same distances, excepting that the light instead of returning, is driven away at each approach. And in this respect, it perfectly agrees with the electrifying of the pane of glass; for as one side becomes electrified *plus*, the opposite side is (as the virtue is driven of) electrified *minus*.

Lastly, Let the excited tube be brought very near the exhausted tube, and then draw it away quickly; in this case the greatest quantity of light appears to return.

So with the pane of glass, the greatest effect is produced by discharging the fluid on the *plus* side as suddenly. And experience has taught us the readiest method, which is done by opening a communication at once between the two surfaces: as we saw it done by the persons holding his fingers in contact with the under cover, and then bringing a finger of his other hand as quickly as he can into contact with the other cover, which he cannot do so quickly, but a spark will appear, and the glass be unelectrified at once or very nearly so.

And that the exhausted tube is in a *minus* state of electrification, at least in some part on the inside thereof, when the light is driven away, and therefore in the same
state.

state with the pane of glass, appears from the violent shock it occasions in the arms, when both ends are touched at the same time with each hand, after the tube has been exposed to a much stronger power of electrifying than the excited tube is capable of producing.

But now we are advanced so far with the Leyden experiment, let us pursue it farther, and try another way of forming a communication between these surfaces, so that the pane of glass shall be gradually and quietly unelectrified. If a wire be bent in a circular form, so that its two ends, when it is brought to the pane of glass, may reach within two or three inches of the covers. If its ends are tapered down into fine points, and the middle of it be fastened to a piece of sealing wax, and then brought by this sealing wax held in the hand into the situation we have described, the glass will quietly and gradually return to its natural state, the denser fluid escaping silently through this wire into the rarer: and what is worth notice, the wire afterwards will not be electrified, notwithstanding it is supported by wax, which confines the fluid within it more than a person's hand would; and notwithstanding that when the experiment is made in the dark, a very visible stream of light appears at each end of it, till the glass is unelectrified. May we not therefore conclude, that nothing more is done in this experiment, than letting the equilibrium be gradually restored within the glass? May we not likewise conclude, that the overplus of fluid in the condensed part was exactly equal to the deficiency in the dilated part; for

for otherwise the wire would have been electrified either *plus* or *minus*? and therefore, that during the electrification of the glass, as much of the fluid was thrown out of the glass, as was thrown in; which only seemed probable before this experiment was tried. May we not likewise conclude, that nothing more was done in the first way of discharging the glass, than in this, only it was obliged to be done at once; and the violence was owing only to the encreased velocity with which the fluid was forced to move into the finger at the upper surface? And that this encrease of velocity was so great, that what in one case was only a continued stream of faint bluish light, was in the other kindled at once into a strong spark of fire?

This may seem a very minute way of considering the causes of these appearances, but it seems to arise from the appearances, and to be confirmed by the following farther observations on the same subject.

The first observation is, that when the wire of communication with the machine is brought into contact with the upper cover, and the under cover is left exposed every where to the air; and the machine is put into action, it will never be able to electrify the pane of glass, though its force be supposed ever so great, because no fluid can be thrown out of the under surface, on account of the joint resistances of the glass, the cover, and the air surrounding it.

But it may be urged, that this joint resistance must be limited, and consequently a power in the machine may be imagined greater, and consequently sufficient to

electrify the pane of glass. To this I answer, that no more of the force of the machine can act upon the upper cover, than what it can communicate to the metal wire, and that all bodies can be electrified but to a certain degree, and consequently, the force of the machine on that upper cover is limited and but small in comparison to the joint resistance abovementioned.

The force of the machine therefore need only be strong enough to electrify this wire as much as it will admit of; all superior force, by irregularly electrifying the air about the wire, being of disservice to the experiment.

The second observation is, that if a bent wire (as described above) is thus applied at its two ends in contact, (or nearly so) with the two coverings, before the machine is put into action, the machine will never be able to electrify the pane of glass; notwithstanding the wire of communication with the machine, and the finger be each in contact with the upper and under cover; because the electrical fluid, conducted to the upper cover by the wire, has a freer passage through the bent wire to the under cover, than to the pane of glass, as metal resists less than glass.

This is a farther confirmation of the cause alledged for the bent wire's discharging the glass, when it was electrified; for surely the same cause that prevents an effect from being produced, is able to destroy that effect, when it is produced.

The third observation is this, that without such a wire to form such a communication between the two surfaces,

faces, the electric fluid on the plus side will of itself stream out, and spread over to the other side, whenever the covers of metal reach too near to the edges and corners of the pane of glass; the electrical fluid insinuating itself between the air and the glass, till it reaches either the corner and edges of the glass, or of the under covering; and so preventing the electrifying of the glass at all, or very soon unelectrifying it, if it is electrified to any degree.

This is the reason why I gave a particular caution, that two, or three inches at least of the under and upper surface of the pane of glass should be left uncovered.

The next thing observable is, the great resemblance there is in these two surfaces of the pane of glass, electrified one *plus* and the other *minus*, with the two cork balls, when one was electrified *plus*, and the other *minus*.

In order to shew this, suppose two panes of glass, supported each at the corners by four drinking glasses, to be electrified equally in the manner we have seen; and afterwards to be left to themselves.

When they are thus left to themselves, 1°. Let a wire form a communication between the two upper surfaces.

No alteration will happen, because the two surfaces, thus communicating together, act with no greater force at each upper covering, than they did before they communicated together.

Thus, when two balls are brought together, after they are each electrified *plus*, they do not unelectrify each other. They only repel each other; and so would the

panes of glass, if their repulsive force were strong enough, to do it as they are now situated.

2°. Remove the wire, and let it now form a communication between the two under covers.

No alteration happens, and for the same reason. Thus when two balls were brought together, both electrified *minus*, they did not unelectrify each other.

3°. But if with two such wires, a cross communication be made from the upper cover of the first pane to the under covering of the second, with one wire; and with the other from the upper covering of the second to the under covering of the first: the condensed fluid will no longer be confined, but a free passage will be opened to delate into the panes of glass at those parts, where they are electrified *minus*, and will circulate round, and reduce itself to its natural state both in the panes of glass, and in the wires, and not one of them will afterwards remain electrified either *plus* or *minus*.

In the same manner two balls, when one is electrified *plus*, and the other *minus*, and they are brought near one another, run together and unelectrify each other immediately.

It would be endless to produce all the experiments, that could be brought in confirmation of this way of accounting for the uncommon appearances, that occur in thus electrifying and unelectrifying glass: which agrees so exactly with all the other experiments we have produced; or rather, which takes its rise from them.

I shall therefore stop here, and only desire the reader not to carry this comparison of the effects on the different
surfaces

surfaces of the electrified pane of glass, with those on the cork balls, electrified *plus* and *minus*, too far. I mean only to compare them in general as similar effects; and do not here consider the comparative strength of the powers producing these similar effects.—Let us now return to the particular subject of this enquiry.

Thus have we seen that the variety in the resistances made by different bodies to being electrified, has led us into the knowledge why bodies act so differently in one situation from what they do in another, according to the nature and quantity of the bodies they are contiguous to; and enabled us to explain the most amazing appearance of all, that of the *Leyden* bottle, which has excited the attention of all the philosophers (I may almost say) in the known world.*

This our success in pursuing this train of reasoning is surely sufficient to encourage us to enquire farther, where this resistance is exerted, and from what power within the body it arises.

Now from our knowing by experiment that every body more or less resists being electrified; which is now allowed to mean, that every body *resists* the passage of the electrical fluid either into them, or out of them, though

* N. B. The Leyden experiment is not confined to glass only, for it succeeds with other substances, though perhaps, not so well; this hath been observed by several gentlemen. And it is very remarkable, that the experiment will not succeed with some particular substances, *unless they are heated to a certain moderate degree; but why heat should make this difference, and in certain bodies only, is another inquiry.* The account of these last curious facts, was sent me in a letter along with some other experiments, by Edward Delaval, Esq; which I communicated to the Royal Society in March last.

Some do it with a *greater* force than *others*; we may very reasonably conclude, that when this fluid is endeavouring to get into any body, the resistance it meets with is exerted at that particular surface, at which the attack is made: and when this fluid is endeavouring to get out of any body, the resistance it meets with is exerted at all its internal surfaces at once; and, if the force with which this fluid endeavours to get in or out, is superior to the force, which endeavours to keep it out or in, at any place or physical point of any of its surfaces, it must make its way more easily there, and consequently will always pass that way either in or out.

And there is an appearance in glass that is exactly analogous to this, with regard to the reflection of the rays of light, by which it is evident, that a pane of glass resists the entrance of light in at one surface; and when it has got in, it as strongly resists its going out at the opposite surface; because at both these surfaces, it reflects or drives off great numbers of rays, that without such resistance would have passed readily through the pane of glass.

It is likewise known, that the rays reflected thus back into the glass, upon their endeavour to get out, and returned again to the first surface, through which they had once got, meet again with an equal resistance there to their passing out, and are many of them reflected into the glass again.

It is shewn farther by Sir *Isaac Newton*, that this force begins to exert itself on the rays of light before they arrive into contact with either of these surfaces.

And

And hence we have reason to believe, that the resistance we have experimentally shewn to be made by all bodies against being electrified either *plus* or *minus*, is of the same nature with that, which not only prevents the entrance and exit of the rays of light in glafs, but throws them off with the same velocity, great as it is, with which they endeavour to get into it, or out of it: and therefore most probably arises from the same cause.

And consequently, that this resistance in bodies to being electrified is exerted, as the other is, before the electrical fluid comes actually into contact with the surface of any body it endeavours to electrify.

The experiments therefore in *Sir Isaac's Optics* serve greatly to confirm the opinion, we have endeavoured to establish by electrical experiments, that every body resists the entrance and exit of this electric fluid; and that this resistance is exerted at some distance, before the fluid, that endeavours either to get in or out, arrives at the surface, where the endeavour is made.

Our next enquiry is, whence this power arises, that is thus exerted at the surfaces of bodies.

In order to answer this enquiry, I shall first propose the examination of the following experiment, agreeably to the way of arguing we have hitherto made use of.

Let a person standing on wax electrify a tube, and let another person standing on the ground take as many snaps, as he can from the tube. There will soon be a time, when the person on the ground will not be able to get any more snaps.

But

But if, when this happens, the person on the wax sets one of his feet on the ground, and keeps it there; the other on the ground may take snaps from morning to night, if he pleases.

From which plain experiment, the following consequences may very evidently be drawn.

1°. That there is a quantity of this electrical fluid naturally in the person, standing on the wax, that by the contrivance in making the experiment is thrown into the person, standing on the ground.

2°. That there is but a certain limited quantity of this fluid in the person, standing on the wax. Because the cessation of the sparks shews, that there remains no more of this fluid, that at first produced them: or at least, that if there does remain some in the person, it has not force enough to push out, and pass into the other, that stands on the ground, and endeavours to take a snap, as he did before.

3°. Therefore the cause of the fluid's passing out, and producing the snap, is the condensation of it at that part of the person, which touches the tube in rubbing it; and so long as this condensation of it can be made with a degree of force that is superior to the resistance made against its escape, it will continue to produce snaps: but no longer.

4°. Now the person standing on the ground must be considered as one body with the earth, and what little fluid is thrown into him cannot produce any sensible encrease of density in the immense quantity on the whole
surface

surface of the earth : and therefore he will not be sensibly electrified, how many snaps soever he may have taken.

5°. When the person on wax does but touch the ground with his foot, he likewise must then be considered as one body with the earth, and consequently, that the degree of density in his whole body cannot be sensibly altered, though ever so many snaps are taken ; because whatever passes out of him, passes into the other, *i. e.* in their present circumstances, passes into their common stock in the earth.

6°. In these circumstances therefore neither of these persons ought to continue to be electrified, according to our reasoning : and it is true in fact that they do not.

7°. Whence it seems to follow that every animal that stands on the earth, every vegetable that grows on it, the whole watery element that flows upon its surface, all minerals, and metals within the earth, as well as the air without, partakes of this common stock in the general course of nature : without being sensibly electrified.

Whence therefore arises their resistance to being *sensibly* electrified ? In order to answer this ; we have seen, that whilst any body continues to be sensibly, or perceptibly electrified, whether it be *plus* or *minus*, in any of the experiments made on them by art, there is an atmosphere of the same kind of fluid which naturally belongs to, and is formed round them, sufficiently strong to balance every power, that endeavours to electrify them above a certain degree : otherwise they might be electrified more and more *without limit*.

This atmosphere therefore, thus surrounding the surfaces of bodies, when artificial force electrifies them, is what resists their being electrified *more*; and when it absolutely prevents it, must be equally dense and powerful with that electrical fluid that flows from the excited tube, or machine, which endeavours to force its way through these atmospheres into the bodies, in order to electrify them *more*.

In the ordinary and quiet manner, in which the imperceptible works of the Author of nature are carried on among the component particles of the gross bodies on or near the surface of the earth; this subtile, active, electrical fluid, which not only surrounds each gross body, but pervading its pores, surrounds every component particle of it, where it is not in absolute contact with its neighbouring component particle; this active fluid, I say, cannot be idle, but must be in action, though that action be imperceptible to our senses: and it must in an imperceptible degree be varying its condition in all the parts of bodies whatever, *i. e.* in our present way of expressing of ourselves, be electrifying them *plus* or *minus*, though not so forcibly, as to give sensible signs of it.

We may therefore not unreasonably conclude, that all bodies whatever in their natural situation, and all their component particles, have surrounding their surfaces, not in absolute contact with other surfaces, an imperceptible atmosphere, of elastic particles, sufficient to ballance the smaller force with which they are attacked; every way similar to the perceptible atmospheres at the surfaces of
bodies.

bodies electrified forcibly either by art, or the violent explosions in nature.

In these atmospheres, which surround the surfaces of all bodies, is placed the power, which occasions the resistance found experimentally to be made against those bodies being electrified to a higher degree, than they are naturally before we attempt to electrify them perceptibly to our senses : and the power is the elasticity of this subtile electrical fluid, every where dispersed indeed where gross bodies are out of the way, but likewise confined within bodies differently, according to their different situations and neighbourhood to other bodies.

And these atmospheres may be encreased, or diminished to a certain degree many ways by art ; and when this is done with violence, the natural contexture of the bodies is altered in proportion to the violence.

Thus we see even, by the small force of our electrical machines, very manifest tokens of the electrified bodies not only parting with their natural share of the electrical fluid, but of many of their component particles ; which may be perceived by the smell they yield on being electrified, and the rays of light they throw out, which, mixing with the air, occasions *real Fire*.

These are proofs of the atmospheres being encreased, as they only happen on the sudden dissipation of them by art, after they were thus encreased. Before they were thus dissipated, therefore, they kept the body, they enclosed, *compact* and *entire* ; and if the force, which had encreased them, had gradually withdrawn itself, these

atmospheres would gradually have returned to their natural degrees of density, and the bodies to their natural state, both with regard to their component particles, and their natural share of this fluid and of the light within them, which were all disturbed and in action before.

With regard to the encrease of density in these atmospheres, what has been said may be little esteemed as arising from theory: but it must be remembered that this theory has been very carefully raised from experiments, and can only be destroyed by shewing some fallacy in our reasoning, or some experiment in contradiction to it.

When this happens, this theory must either give way, or be improved.

With regard to the diminishing the degree of density in these atmospheres, I shall produce an experiment or two, very well worth attending to, as I think they will amount to an experimental proof, the best proof, of the whole doctrine I have advanced in these sheets.

Glass, we have observed, and seen in experiments, is the most difficultly electrified of any of the common bodies we try our experiments upon: and therefore has the most resisting atmosphere on its surfaces, according to our way of reasoning, by which it so strongly resists electrification, *i. e.* the densest atmosphere.

Metals resist being electrified with much less force, and consequently have, in the same way of arguing, atmospheres on their surfaces which resist but weakly, *i. e.* not the densest atmospheres.

Heat

Heat is known to rarify all bodies, both fluid or solid; and if a glass be heated to a certain degree, even below melting, it will conduct, as it is called, the electrical fluid, *i. e.* it will resist its entrance into it, or its exit out of it, no more than metals do: and when melted, no more than water.

A subtile and elastic fluid most undoubtedly must be rarefied sooner than so dense a body as glass, and therefore it seems most probable that the resisting atmosphere around its surfaces is rendered equally rare, or weak by the heat, with that of metals, and therefore resists the passage of the electrical fluid through the glass no more than brass or any other metal does.

And this seems to be confirmed by the glass's resisting this passage through it more and more as it gradually cools: till at last when it is quite cold, it resists as forcibly as ever against any entrance of the electrical fluid into it.

Wax is a substance, which resists electrification perhaps as much as glass, but certainly more than metals, whilst its surface is every where smooth and polished: but wax is very easily melted with a small degree of heat; and so its atmospheres are very readily brought down to the same degree of rarity with those of metals: and therefore resists the passage of the electric fluid no more than gold or any other metal.*

But

* In the other edition, there is an experiment or two with a sulphur globe, which was taken from the works of Dr. Franklyn: But as the experiments were never repeated either by Doctor Hoadly or myself, and the

But to make it still plainer that the resistance to electrification is made at the surfaces of bodies, I will shew in the following very curious experiments, that the electrical fluid in passing from one body to another will always take that way, in which it meets with the fewest surfaces to break through.

Let a *Leyden* bottle, that has a hook in its coating, be electrified and set down on glass, and left to itself. Let one end of a clean chain (such a one as is commonly used for a jack, but rather lighter) be fixed on to the hook in the coating; and let a person grasp the coating of the bottle with one hand, and with the other hand bring the other end of the chain and his finger and thumb that holds it, at the same time into contact with the wire of the bottle. Here then are two ways offered for the electrical fluid to pass from the wire to the coating, either through the person, or the chain: and if the links of the chain hang loosely on one another, it passes through the person, and he is shocked very nearly as much as if the chain was not there. But if the chain be stretched by any contrivance, so that its links are all in absolute contact with each other, the fluid will pass through the chain, and the person will feel no shock at all. And this will be the case, let the chain be ever so long.

Whence it follows, that the electric fluid does *not* always pass from one body to another by the *shortest way*,

the reasoning upon them was liable to objections, and not necessary in the present case, I have chose to leave that part out in this 2d edition.

but

but on the contrary it will go about, and pass that way in which it meets with *least* resistance.*

Now if the chain alone forms the communication between the wire and coating of the bottle, and it is spread so on a table, that the links of it scarce seem to touch one another; there will not only be a spark seen on the approach of the end of the chain to the wire of the bottle: but a number of them will appear very visibly, when the experiment is made in the dark, *viz.* at every place, where the links do not absolutely touch one another. But when the chain is stretched tight enough to have every link in absolute contact with its neighbouring links, there appears but one spark on bringing the end of the chain to the wire of the bottle; the whole chain then forming one continued metal.

By the appearance of these sparks, or their non-appearance, we judge whether the electrical fluid passes through the chain or no.

Now in these experiments, we have seen that when the links of the chain were loose, the person was shocked, and the fluid passed through him; and that it did not pass likewise through the chain appears from the experiment, because no such sparks appear between the links.

Whence we may fairly conclude, that the resistance made to the passage of the electrical fluid through the chain, arises from the sum of the resistances at the different surfaces of the links, it was to break its way through,

* Doctor Watson and Doctor Franklyn are therefore mistaken in regard to the Leyden experiment, where they advance that the electric fluid always makes the *shortest circuit*.

in passing through the chain. Because when the links were forced into contact with each other, and so the whole chain was made one continued metal, this resistance is entirely taken off, the person is not at all affected on the discharge of the bottle, nor do any sparks appear between the links.

Now if the experiment be repeated in the following manner, this consequence will be still plainer. Let one end of a wire, ever so long, be fastened to the hook at the coating of the bottle, as well as the chain, and let a person, grasping the bottle, as before, bring the other ends of both the wire and the chain into contact with the wire of the bottle, and let the links of the chain lie loose on the table; or let the chain not be stretched.

Here are plainly three ways, the electrical fluid may pass on the discharge of the bottle, through the person, through the chain, or through the wire: and the event of the experiment shows that it does not pass through the person, because he feels no shock; and that it does not pass through the chain, because no sparks are seen between the links; and therefore that it passes through the wire, where it meets with but one surface to break through; *viz.* the surface at the end that is brought to the wire of the bottle, the other end of it being supposed to be in contact with the hook at the coating of the bottle.

I shall observe, by the by, that no one, who had not made the experiment, would imagine with how much force the chain must be stretched before the experiment will

will answer, and the electrical fluid pass through it without producing a spark at any of the links, *i. e.* before the links can be brought into absolute contact with each other: which one would naturally think their weight alone would be sufficient to do.

But it appears that their weight will not do this, but that some additional force, independent of themselves, or their weight, is required to bring them into absolute contact.

This, therefore, is a strong confirmation of Sir *Isaac Newton's* assertion, that when a convex glass is laid upon a plane glass, they do not absolutely touch; but that they must be squeezed together with some force before they can be brought into absolute contact, so as to produce the effects visible in some of his nicest experiments, which depend on their absolute continuity.

From all these observations, and experiments, we may conclude, that the power which produces the resistance we find in bodies to be electrified either *plus* or *minus*, is the elasticity of these small atmospheres of the electrical fluid, which are formed at all their surfaces by the actions between the particles of this fluid (both within and without bodies) and the component particles of the bodies: and therefore must be different at the surfaces of different bodies.

We shall in the next place endeavour to explain, how light bodies at considerable distances from an electrified body, are driven to and from that body.

We have seen, that on bringing an excited tube near the middle of a bar, is electrified it *minus*, and consequently, puts the electrical fluid within it into a state of dilatation; whence we may conclude in general, that this fluid every way surrounding the tube in the air to a certain distance, is in the same manner put into a state of dilatation; and in consequence of this, that beyond that distance, it must be put into a state of condensation: or more properly, that this fluid is rarer, than it naturally is near the surface of the excited tube, and grows gradually denser and denser, till as that distance, to which the power of the tube extends, its return to its natural density again.

Whence it will follow, that any light body, that is within this distance, will be forced from the denser to the rarer part of this fluid surrounding thus the excited tube; and so seem to be attracted by it.

But as soon as this light body in passing thus from the denser part of this fluid through the rarer, during its approach to the excited tube, becomes sufficiently electrified to have an atmosphere collected round it, of the same nature with that of the excited tube, it must, as has been shewn before, be driven back again from the tube: and that its being thus driven back again, is owing to its having acquired such an atmosphere, is evident from this, that whenever afterwards it comes near any body more easily electrified than itself, and communicating with the ground, its electrical atmosphere is dissipated;

pated ; and it immediately returns to the tube again ; for the same reason, as at first.

Thus have we gone through the most interesting of the electrical experiments, and from the various appearances they afford, it appears, that the electrical fluid is as universal and powerful an agent at or near the surface of the earth, as that fluid, which Sir *Isaac Newton* in his *Optics*, calls æther ; that it is as subtile and elastic in its nature, as æther is ; and as æther does, that it pervades the pores of all bodies whatever, that we are conversant with ; is dispersed through whatever vacuum it is in our power to produce by art ; and from the natural phœnomena of Thunder, Lightning, &c. seems to be extended to very great distances in the air.

We shall make no scruple therefore now to affirm, that these two fluids are one and the same fluid ; as it is much more philosophical to do so, than to suppose two such fluids, each of them equally capable of producing these effects, and equally present every where ; which would be multiplying causes, where there is no manner of occasion.

The word *electrical*, is of too confined a meaning to be a proper epithet for a fluid of so universal an activity as this is found at last to be, from the experiments we have been considering, because it expresses its power but partially.

Electricity means no more than the power we give bodies by rubbing them, to attract and repel light bodies

that are near them, in the same manner as amber does, when it is rubbed. But this fluid not only makes light bodies, that are near an electrified body fly to and from that body, and so appear to be attracted and repelled: but it heats them by putting their component particles, and the particles of air and light within them, into a vibrating motion; and makes them throw out the rays of light that before lay hid, and part with their sulphureous and volatile component particles, which, with the rays of light, on mixing with the air, burst out into sparks of real *culinary* fire, as the chemists express themselves; nay, more in passing through animals, it occasions convulsions, tremors, pain, and death sometimes: and in passing violently through leaf gold, held tight between two pieces of glass, makes a fusion of the gold, or a separation of its parts, so instantaneously, that no sensible heat remains either in the gold or glass, and they immediately after are so closely united, that Aqua Regia itself, has no effect upon the metal.*

It is likewise improper to call this fluid, *Fire*.

Air may just as properly be called sound, as this fluid

* I have made a small alteration in this part, because upon making the experiment (which we had not done before, but only related what seemed to be the fact when Dr. Franklyn first made the experiment) it has not hitherto appeared that the gold is vetrified, or even driven into the pores of the glass, as was supposed by him; but I suspect that the metal is driven so near into contact with the surface of the glass, by the violence of the explosion, that it is held there by an exceeding great force, and such a one as we have been endeavouring to show is exerted at the surfaces of all bodies whatsoever.

can be called fire. When sound is produced, the particles of the air are put into so regular a motion as to convey such sensations by means of the ear as raise the idea of sound. But air is not therefore sound. In the same manner, when a body has all its component particles thrown into such agitations in the air, by the force and action of this fluid within it and without it, that it grows hot, and shines, and glows and consumes away in smoak and flame, we say the body is on fire, or burns; but this fluid is not therefore *fire*: nor can it, without confounding our ideas, have that name given to it; nor indeed can *fire* be called a *Principle*, or *Element* in the Chemist's sense of the word, any more than *sound* can.

Sir *Isaac Newton*, at the end of the *Principia*, in the second edition, *anno* 1713, describes this fluid and its effects in the following words, and says expressly, that it is the cause of the electricity.

“ Adjicere jam liceret nonnulla de spiritu quodam subtilissimo corpora crassa pervadente et in iisdem latente: cujus vi, et actionibus particulæ corporum ad *minimas distantias* se mutuo attrahunt, et contiguæ *factæ* cohærent: et corpora *electrica* agunt ad *distantias majores tam repellendo quam attrahendo corpuscula vicina*: et lux emittitur, reflectitur, refringitur, inflectitur, et corpora calefacit: et sensatio omnis excitatur, et membra animalium ad voluntatem moventur vibrationibus scilicet hujus spiritus per solida nervorum capillamenta ab externis sensuum organis ad cerebrum, et a cerebro ad musculos propagatis. Sed hæc paucis exponi non possunt; neque adest
sufficiens

sufficiens copia experimentorum, quibus leges actionum hujus spiritus accurate determinare et monstrari debent."

No one, we think, can read this paragraph, after having considered the appearances in the experiments described above, without recollecting instances in some one or other of them, of almost all the effects of this fluid, enumerated in it: and agreeing with us, that the other appearances among electrified bodies, as well as that of their repelling and attracting light bodies that are near them, may all of them arise from the force and action of this fluid, on the component particles of the bodies; on the rays of light within them; and on the air they are in; and the reaction of these upon the æther.

When a flint and steel are struck together with sufficient force and velocity, a spark of fire, as we call it, is produced, which readily fires gunpowder, or lights tinder: but soon cools, if left to itself.

Now if such a spark be caught on a sheet of paper, and examined in a microscope, it will be found to be a piece either of the flint or of the steel, struck off, so exactly spherical, and polished, that the windows of the room may be seen in it in the same manner as they are in a large polished sphere of metal or glass: and they could not be so spherical, and well polished, as they are found to be, if they had not been melted and kept in this form by the cohesion of their component particles.

In either of these cases, a piece of flint or steel is evidently separated from the body, and its component particles put into such agitations among each other, as to
 throw

throw off the rays of light that were among them, and shine and melt, and afterwards cool in a spherical form : by the action of the æther on light and air, and these component particles ; and the reaction of these upon the æther ; on their being all put into action at once by the briskness of the stroke.

There would have been no such spark produced, if any of these had been wanting ; and consequently, they are all necessary, though perhaps not equally so, to the producing this effect ; the æther seeming to be as powerful an agent as any amongst them ; without which the intestine motion among the component particles of the piece struck off, could not have been kept regularly up, even for the very small time in which these changes are made in that piece.

In the same manner are the appearances of light in these electrical experiments, whether in faint streams of different colours, or in bright and active sparks, to be considered ; as rising from smaller parts of gross bodies separated from them, and carried off by the activity of the excited æther, passing from one body into another ; which parts, though imperceptible to us, must have their component particles put into agitations amongst themselves, and, in being decomposed, part with the light (that before lay hid within them) and their most volatile particles ; and so shine, and smell, and explode in passing through the air.

And not only these appearances of light, sparks and explosion, but the effects of them on bodies, exposed
to

to them in electrical experiments, seem all to be explicable by the mutual action and reaction of the æther, of the component particles of the small parts of bodies thrown off in these experiments, of the particles of light within these, and of the air, one upon another, when they are once made active by friction.

A more minute, or exact explanation of every particular appearance of this kind in each electrical experiment, we were to consider, was never designed in this enquiry; as has been said before. Our intention being only to shew from a number of experiments, that whatever fluid was the cause of the very surprising effects produced in them, must be of the same nature, and as universal, and as powerful, as the æther which Sir *Isaac* in his *Optics* suspects even to be the cause of gravity.

These experiments, therefore, seem to us so many confirmations of the existence and properties of such a subtile, elastic fluid every where dispersed about the earth; and though they should not be thought absolutely to prove its existence every where, they may be fairly added to the number of those experiments, that cannot be satisfactorily explained without it: and by putting us in a right track, may perhaps enable us to obtain a sufficient plenty of these sort of experiments to make us certain there is such a fluid actually existing every where; and what the laws of its action are.

If

If the laying these experiments in the order we have done, and our reasoning upon them, shall any way conduce to so valuable an end ; we shall think our time very well employed, and our purpose answered.

We are very sensible, that there remains to be explained, how rubbing a tube rarifies the æther around it ; nay, more, how the tube once excited by rubbing will retain this virtue for a considerable time after the rubbing has ceased : but this we must leave to another opportunity, the explanation of it necessarily leading to greater lengths, than our present design seems to require.*

We shall only add, with regard to Sir *Isaac Newton*, that this opinion of his was no new one, taken up in his latter days, in order to obviate the trifling objections of foreign philosophers against the use he made of the words *Attraction* and *Cohesion* ; but was the result of his experiments in *Optics*, the greatest part of which he made long before the *Principia* was published : and in order to shew this, as well as the strict adherence to the truth of appearances he observed in all his philosophy ; we shall conclude with a sentence or two out of the Preface to the first edition of the *Principia*, printed in the year 1686 : recommending them to the consideration of every one, who is disposed to a philosophical enquiry into any part of the works of the great Author of all nature.

“ Omnis enim Philosophiæ difficultas in eo versari videtur ut a phænomenis motuum investigemus vires naturæ ; deinde ab his viribus demonstremus phænomena reliqua

* See a Treatise of electricity, by B. Wilson, where this enquiry has been attended to, page 99, prop. 16.

et huc spectant propositiones generales quas libro primo et secundo pertractavimus.

In libro autem tertio exemplum hujus rei proposuimus per explicationem systematis mundani.

Ibi enim ex phænomenis cœlestibus, per propositiones in libris prioribus mathematice demonstratas, derivantur vires gravitatis, quibus corpora ad solem et planetas singulos tendunt. Deinde ex his viribus, per propositiones etiam mathematicas deducuntur motus planetarum, cometarum, lunæ, et maris.

Utinam cætera naturæ phænomena ex principiis
 Nam *multa me movent ut nonnihil suspicer ea omnia ex viribus quibusdam pendere posse, quibus corporum particule per causas nondum cognitatas, vel in se mutuo impelluntur et secundum figuras regulares cobærent, vel ab invicem fugantur et recedunt: quibus viribus ignotis, philosophi hætenus naturam frustra tentârunt.*

Spero autem quod huic philosophandi modo, vel veriori alicui, principia hic posita lucem aliquam præbebunt."

P O S T.

THE following letter to Dr. Hoadly was written at his request, to give him my notions upon electricity, and of all that I had done therein. It is a kind of history of several appearances I have at various times observed in the course of my electric enquiries, and in some respects is nearly the same with the matter contained in the preceding work. Yet as it is not only shorter, but requires less attention, in the reading, to understand, I have been advised to publish it, and at the same time subjoin two other letters which were never before published, and are there referred to, as they chiefly relate to a particular fact that I observed in the year 1746, concerning the electric fluid as collected (in a great measure) from the earth itself.

The following letter to Dr. Hoody was written at
 his request to give him my notions upon electricity
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 some years past it nearly coincides with the manner
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 never before published, and are thus related to as they
 chiefly relate to a particular fact that I observed in the
 year 1746, concerning the electric fluid as collected in
 a great measure from the earth itself.

P O S T S C R I P T.

WE cannot help bespeaking the reader's candour with regard to the difficulty of expression on so nice a subject, as well as the difficulty of describing with accuracy all the circumstances in these nice experiments which are necessary to the production of the appearances we argue from; and must take upon us at the same time to assure beginners, or those not very conversant in making these experiments, that they must not conclude they are not true, because they cannot make them succeed.

The many and nice precautions in making all sorts of electrical experiments with accuracy, may be seen very carefully enumerated at the beginning of Mr *Wilson's* treatise on Electricity, and to the same treatise we refer for the experiments, from which we deduce the circumstances in which bodies differ in their degrees of resistance to being electrified and unelectrified; mentioned page 3 and 4 of these *Observations*.

To

To Dr. HOADLEY.

30 Nov. 1755.

THE honour you have done me, in desiring a brief account of the most material discoveries I have at any time made in electricity, demands my immediate compliance.

I began my experiments and observations in the year 1745, and in the same year presented part of them to the Royal Society. What my notions were then, you will find in those papers which were afterwards published; the substance of which, however, I will here set down.

That what we call electricity or electric matter, is a fluid exceedingly subtile and elastic.

That it readily pervades all bodies, and is diffused every where; so that the earth itself, as well as the air, is filled with it; but in such manner, that its density may be encreased or lessened by different degrees of heat and other causes, as the air is.

That any one body under certain circumstances, cannot be electrified without *putting more* of that matter into it, than originally belonged to it. And so much of the electric matter as that body hath *gained*, must be taken away from the original quantity of some other body or bodies near it, or from the air, earth, or all of them together.

And

And on the contrary any one body under certain other circumstances, cannot be electrified without *taking away* or *lessening* that quantity of electric matter which originally belonged to it. And so much of the electric matter as that body hath lost, must be gained by some other body or bodies near it, or by the air, or earth, or them all together.

That the electric effects, such as the attraction, repulsion, sensation, &c. arise from this loss or gain, and are always in proportion; that is to say, the more any body has gained, the greater are the effects on its rushing out again, in order to restore its equilibrium. And *vice versa*, the more any body hath lost, the greater are the effects on its rushing in again, to restore the equilibrium.

The air which is an elastic fluid is governed by this law. I was of opinion, therefore, that from this law, all the electric phenomena, or most of them, might be explained.

But an elastic fluid of any given density, being actually detained within a bar of iron for any given time, without something to *confine* it there, was a very difficult problem with me. So likewise was the singular property of glass, wax, &c. compared with iron and other metals, &c. the first obstructing the passage of the electric fluid through them into other bodies, and the last suffering it to pass most freely and readily through them into other bodies of the like kind. My difficulties continued for sometime. But new experiments (which I published

published in the year 1746) and Sir Isaac Newton's optics induced me to believe.

That all bodies have a very dense subtile medium spread over their surfaces, which extends to a very small distance from the body: and is of the same nature with what is called electric fluid.

That the difference between electrics and non electrics, is intirely owing to the different densities of this medium, as it resists the exit or entrance of the electric fluid equally; so that bodies cannot be electrified plus, without this resisting medium to confine the electric fluid in the bodies when once they are electrified; no more than bodies can be electrified *minus* without the same resisting medium to hinder the electric fluid entering into the bodies.

Upon these principles I proceeded to make electric bodies non electric; or in other words, to render all bodies capable of receiving the electric fluid equally. And here I hop'd to dispell as it were the cloud with which this part of natural philosophy had for so many years past been envelop'd. I considered in the first place that if I rarified the air, or my other elastic fluid that was not confind, the resistance arising from such a diminution of density would be less than if it was not rarified. And therefore, if I exposed amber, glass, wax, pitch, resin, and even diamonds to any given degree of heat, I should rarify the medium upon the surface of any one of those bodies, and consequently lessen its power of resistance at any given distance. The event proved the truth of the
conjecture

conjecture, because they were all in such circumstances as readily electrified as iron or any other metal.

Having satisfied myself that there really was something upon the surfaces of bodies which actually had a power of resisting both the exit and entrance of the electric fluid, it was natural to conclude, that any two surfaces, or mediums belonging to the same body, would resist more than one surface or medium of the same body, and three more than two, and so on. I therefore made the Leyden experiment with a chain; and considered each link thereof as having two surfaces at least: so that lengthening or shortning the chain in each experiment occasioned different resistances; and the effects proved accordingly. To confirm this law of resistance I made the experiment with one wire only, the surfaces of which were no more than two; and the effect answered my expectations, as the resistance in that case was least of all. But to leave no room for any doubt I caused the chain to be stretched, that the links might be brought nearer into contact, the event then was the same as with the wire. And this last experiment I looked upon as an *experimentum crucis*.

I was the more induced to prosecute this doctrine of resistances, from what Sir *Isaac Newton* has delivered in his opticks, concerning the prisms and convex glasses, where he observes that light is more readily transmitted through those parts, which are in closer contact by *pressing*, than in
the

the other parts where they are not so much pressed.* The perfect analogy between these experiments greatly confirms the truth of the reasoning.

I then made some farther experiment, to shew that what is called electric fluid is of a very heterogeneous kind, and that it was probable we should be nearer the truth, if we understood it thus.

That there is a very gross matter driven off from the surface of bodies by the violence and vigour of the action of the electric fluid, which when forced into the air, kindles into flame. But the pure and simple electric fluid, which produces the wonderful effects of attraction and repulsion in electricity, and every other principal

* There is a curious phænomenon, which I have observed in the Torricellian experiment, made by the Right Hon. Lord Charles Cavendish, and mentioned in the transactions, vol. 47, p. 370, which affords us a kind of ocular proof of the existence of this resisting medium upon the surfaces of bodies; for during that experiment, the streaming light which appears to flow from the quicksilver in one of the

last columns, through the *vacuum* to the quicksilver in the other column, is of an uniform density; *excepting at that surface where it enters the quicksilver*, and there the light appears to be much brighter, for near one tenth of an inch, and consequently denser. Now this increase of density (I suppose) arises from the resistance at the surface of the quicksilver, where it endeavours to enter, and from no other cause. And that it will continue to increase till the force arising from its density, be sufficient to overcome that which is exerted against it at the surface of the quicksilver. If the experiment be reversed, so that the flow of the electric fluid shall be from the other column, then the same appearances happen, with this difference, that they are now upon the reverse surfaces: and when either column is electrified *minus*, the appearances are the same.

phænomenon,

phænomenon, excepting the luminous appearances, is far more subtile than even light itself.

From this its exceeding great subtilty and elastic power, I conjectured that it was diffused not only throughout the whole earth and air, but perhaps through the whole solar system, because we do not know of any confinement to limit its expansive power; and experiments have taught us that the electric fluid always flows where it meets with the least resistance, whenever its *equilibrium* has been disturbed, in order to recover its natural state.

As this wonderfully active instrument attracted all kinds of bodies indifferently, I flattered myself that there seemed a perfect analogy between it and the gravitation of bodies. And that the reason why we were not able to produce greater effects, in moving larger bodies, by accumulating the electric fluid, was, that the surrounding medium not being strong enough to detain the necessary quantity, our electric power was in some measure limited.

It was a considerable time after this, that Mr. Franklin, of Philadelphia, published his curious discoveries in regard to lightning, and the *plus* and *minus* electricity of a phial charged as in the Leyden experiment, in which he mentions a difficulty concerning the *minus* electricity which he could not account for, it was this. Why bodies electrified *minus* should have electric atmospheres round them like those electrified *plus*?

Here

Here I perceived a new field of enquiry open before me, particularly as I believed the Leyden experiment could not be explained satisfactorily without a previous solution of this difficulty. Therefore after examining the subject a fresh, I drew up the first packet of papers you had from me, where you know I endeavoured to remove the difficulty we laboured under, both in regard to the Leyden experiment and the different repelling atmospheres of the *plus* and *minus* electricity. The method I proceeded upon to discover this *defideratum*, was much of the same kind with that I made use of when I first found that the electric fluid did not come from the glass but the earth itself. For if (as I observed in a letter to Mr. Ellicot)* the glass or wax hinder the electric fluid from passing into the earth, they ought also to hinder its coming out again. So in this. If the medium surrounding a body electrified *plus* hinders the electric fluid from escaping out of that body, the same medium ought also to hinder the electric fluid from coming into a body electrified *minus*. Consequently atmospheres round two bodies electrified *plus* and *minus* must necessarily be formed and be of equal densities &c. provided the air surrounding the bodies be of the same driness and density, and the bodies of the same shape, kind and size.

I concluded my enquiries with giving a more general explication of the Leyden experiment, the particulars of which you received in my second packet. So that I need not repeat in this letter the method I proceeded upon to solve this last and surprising problem. Neither need I tell
you

* See the following Letters.

you that Mr. Franklin's method was far from pleasing me. I know you yourself was not satisfied with his reasoning about it. All I have now to add is, that you will fulfil your promise and methodize those papers for me. I care not how many objections are made by a good philosopher, because if I should not be able to answer them, I shall perhaps gain somewhat from them that may contribute to improve my hypothesis, and which I should not have thought on had there been no objection made.

In the detail of this account there are a few things I have omitted, particularly the finding that the electric shock in the Leyden experiment was always proportional to the points of non electric contact on the in and outside of the phial. But this and other experiments, however remarkable in themselves, not being connected with my general system, are therefore not of moment enough at this time to merit your attention.

I have omitted the references to save myself trouble, but if you desire them they shall be sent you.

I am, &c.

B. WILSON.

An

An Extract of a letter to Mr. Ellicot, dated from Chester,
17 Sept. 1746.

S I R,

I SHALL be very glad to know if you have tried the experiment I mentioned to you in London, concerning a large glass bottle filled with warm water, and touching the surface all over with a non-electric, in order to see if the effect is not in that proportion.

There is another experiment which I have since thought of, which I wish you would also try. I have suspected that the excited effluvia are not from the glass globe but the body that rubs it, or however the greatest part. You may be satisfied of this by holding in your hand a long dry bottle by the neck, so that the bottom thereof, to which one of your leather cushions is fixed, shall press upon the globe after the common method. In this situation you know the glass between the cushion and your hand will intercept any communication of these effluvia with the earth (if there naturally are any such in non-electric bodies) for the same reason as it does electrified bodies when fixed upon resin &c. I fancy you will produce little effect. If it should be the same as before, then it is plain that the effluvia come from the globe of glass.

I am, &c.

B. WILSON.

An

*An Extract of a Letter to Mr. Smeaton, dated from
Dublin the 6th of Oct. 1746.*

DEAR FRIEND,

I SHALL defer giving you any account of this city, &c. till I write again, because I have only time to acquaint you, that since my arrival in this kingdom, I have had an opportunity of trying some electrical experiments, which I had before thought of in England. And I have the pleasure to inform you, that I find the electric effluvia do not come from the globe of glass, but from the earth itself, and all non-electric bodies, which are round about the apparatus. To try this, stand upon a board when placed on four bottles, or upon a cake of wax, and endeavour to excite a tube of glass; after that observe the effect. Then get off from the board or wax, and excite the tube again, and you will perceive a very great difference: for the effect produced in the last circumstances, will be considerably greater than in the former circumstances.

Another experiment which I proposed to try, was with the *Leyden* bottle, whether it observed any law in the accumulation of electric matter. And I found that it was always in proportion to the thinness of the glass, the surface of the same, and that of the non-electric in contact with it on the in and outside thereof. The experiment I made, was with water a little warmed, which was poured into the bottle, and the outside was immersed
in

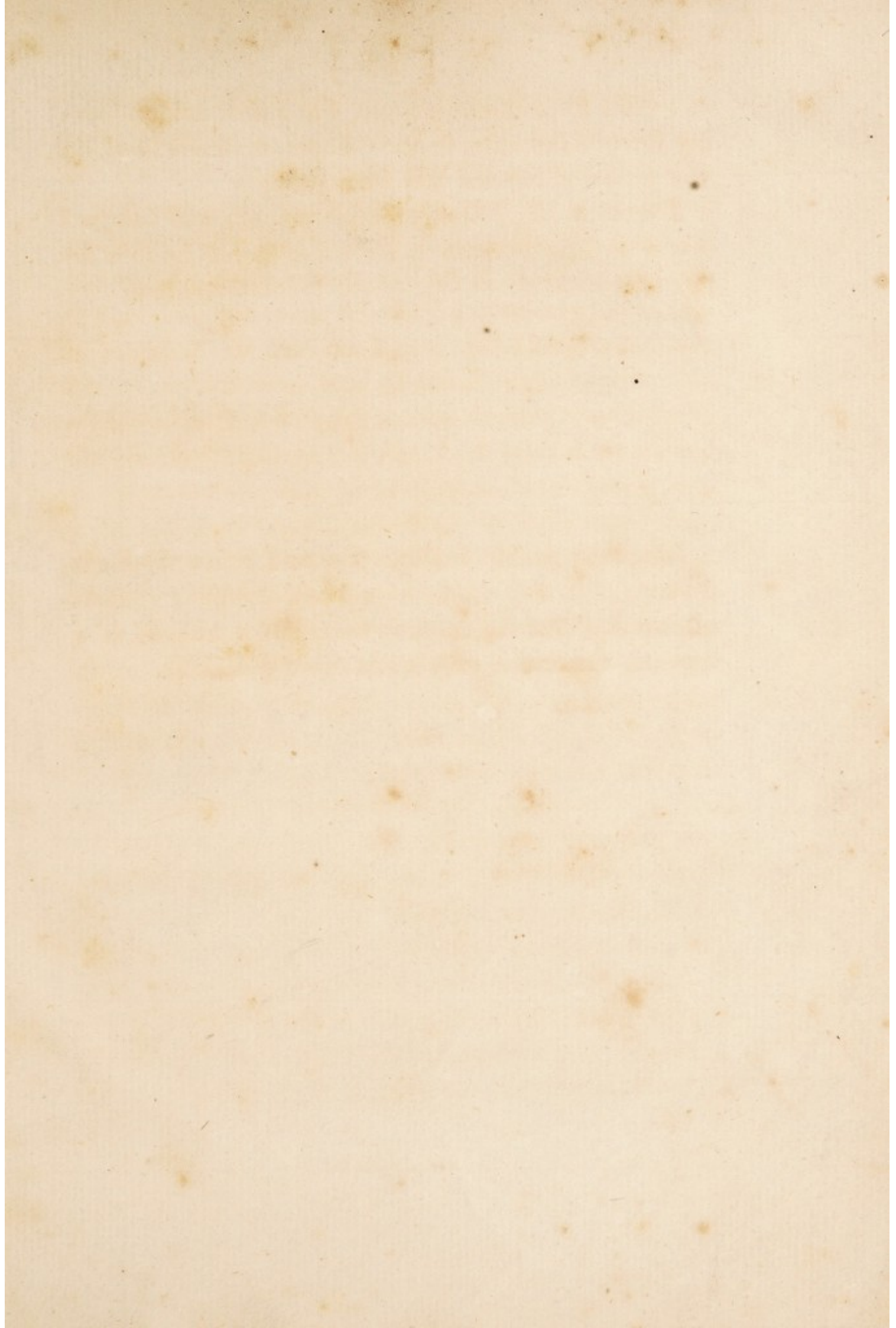
in a vessel filled also with water, but a little colder, leaving three inches only, or thereabouts, uncovered, which was preserved dry and free from dust.

I wrote to Mr. Folkes by the last packet, and informed him of these experiments, in order that he may lay them before that honourable society, of which he is President.

I am, &c.

B. WILSON.

The letter to Mr. Folkes, was read before the Royal Society, Oct. 23, 1746, as appears by their minutes of that day, but the original being lost or mislaid, is, I suppose, the reason why it was never published.



In a well filled with water, but a hole being made
in the middle of the hole, the water was found to
be contained in the hole and not in the water.

I wrote to Mr. Palmer on the subject, and informed
him of the result of the experiment, and that he should be
very kind to forward a copy of what he had written.

Yours truly,
J. W. WILSON

The water in the hole was found to be
contained in the hole, and not in the water,
and the result of the experiment was
that the water was contained in the hole
and not in the water.

