OBSERVATIONS
ON A
SERIES
OF
ELECTRICAL EXPERIMENTS.

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Usque adeo magni refert cui quæ adjaceat res.
LUCRET. Lib. V.

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THERE is a very fine Fluid, of the ſame nature with Air, but extremely more ſubtile and elaſtic, according to Sir *Iſaac Newton*, every where diſperſed through all ſpace, which in his Optics he calls *Æther*.

This Æther is 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*.

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, agreably 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 agreable 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 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, there is 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

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 its 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 Glaſs, 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, 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

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 make a very great resistance to being unelectrified, 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 strogly; and that air likewise, when it is dry aud free from vapour, does 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 unelectrified: 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 return 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 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 disfavourable 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 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,

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the air clofes over the bar, and ferves to keep it electrified.

Now the fame way of reafoning will fhow us the eafieft way of unelectrifying it again.

For firft, I can take off the refiftance 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 confider what fhape I ought to give this body. If I make it end with a fharp point, it will with the greateft eafe receive the fluid that endeavours to get out of the bar, as foon as the equilibrium begins to be deftroyed by thus removing the refiftance of the air from off that point of the bar, the point of the body is oppofed to: and if I ftand upon the ground, whilft I do this, I give a free paffage to any quantity of this fluid the bar may have to part with, into the earth.

But laftly, I am to confider, that by thus making the difcharge of this fluid fo eafy, 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 defign was to produce: becaufe as this pointed body approaches the bar, there lies fo fmall a quantity of air between the point and the bar, that it hardly refifts at all.

Confequently, we fhould try the experiment with fome body that ends bluntly and is well polifhed, and bring it at laft with fome degree of quicknefs within the

fphere

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.

The question then arises, at what parts of the bar did this fluid escape? and the answer, according to our manner

ner 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, neither 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 by and by.

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 the only difference in the two experiments is this,

that

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.

After

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

ſame diſtance from the middle of the bar, as before; the bar, now its ends are thus fortified with glaſs, will be electrified *plus*, inſtead of *minus*: 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 twice the quantity of fluid to condenſe with ſufficient ſtrength to throw any 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

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 by and by.

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

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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 returns 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 lies between the air, [which reſiſts its entrance into it more forcibly than the bar naturally does,] and the bar, [which 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, lies in the ſame manner between the air, and the bar, will, 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

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, crouds 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.

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° We

1° 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, 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 withdrawing the tube, the balls will not repel each other near ſo much, and ſo 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;

ther; 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 repulſive 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

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 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

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 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

duce 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 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-

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 furface, from its inner furface through the vacuum, and darting directly to the metal and hand, where it finds the refiftance to its efcape is the weakeft.

Now though the refiftance is weakeft, where the brafs and hand is applied to the glafs; yet there is a refiftance; and confequently, the electrical fluid in the vacuum muft have fome additional force given to it from that part of the inner furface of the glafs which is neareft to the excited tube, *i. e.* muft be condenfed till it acquires a power fufficient to overcome the refiftance of the brafs, *&c.*

But this fluid within the exhaufted tube cannot be condenfed, without more paffes into it immediately from that inner furface, and confequently fo long as the electrical fluid is continually flowing from it into the vacuum, and fo into the brafs, *&c.* fo long will it be lofing its natural quantity, unlefs its lofs of this fluid be fupplied by a flow of more from the excited tube.

I fhall now, therefore, endeavour to fhew, that the lofs of the electrical fluid flowing from the inner furface of the glafs, is not fupplied by a fimilar flow of it from the excited tube to it, through the outer furface; and therefore, that the glafs is electrified *minus* at or near that part of its inner furface.

Now this will follow, plainly, if I can fhew, 1°. That no electrical fluid flows into the glafs fo far as to reach that

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 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

So far then the appearances in the experiment do not anſwer agreably 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, 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

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 excited bar (whoſe 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 excited 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 experiment can be ſtronger to the point, 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

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 excited 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 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

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 appearance of light ſhooting from the braſs 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 to that extraordinary effect has not hitherto been satisfactorily explained.

We

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.

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 paſſing through air at all: and therefore we have

have reaſon to believe the glaſs is put into the moſt favourable circumſtanccs 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 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, whereever

ever it lies, to expand itſelf equally every way with the 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 thown 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

no electrical fluid paſſed ſo far into the glaſs as to reach that ſurface; and therefore we may here imagine it 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 ſix 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 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: eſpecially as we know not yet how far the particular make of glaſs may contribute towards this effect.

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

This therefore ſeems to be an experimental Proof, that there is ſome power remaining in the glaſs itſelf 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 any fluid within the ſubſtance of the glaſs of the ex-

 hauſted

hausted 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 encreased 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

diſtance from the middle of the middlemoſt bar: For 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 in conſequence of the glaſs's being electrified *plus* neareſt the upper ſurface, and *minus* near the under ſurface, there muſt, on withdrawing both the finger and the wire, be left two atmoſpheres ſtanding on theſe ſurfaces without the glaſs, now ſurrounded (covers and all) with air; for the ſame reaſons that the cork balls had ſuch atmoſpheres round them, when one was electrified

plus,

plus, and the other *minus*: and the atmoſphere about the ſurface, that is electrified *plus*, will endeavour to dilate itſelf from the glaſs into the air, whilſt that at the ſurface electrified *minus* is endeavouring to dilate itſelf from the air into the glaſs; as was ſhewn to be the caſe of the two cork balls. Whenever therefore theſe two atmoſpheres are permitted to communicate with one another, the former will drive the latter into the glaſs at the ſurface electrified *minus*: as we ſaw in the cork balls.

But the broad column of air, that is in immediate contact with all the ſurfaces of the pane of glaſs that were left uncovered, ſeparates theſe atmoſpheres, and prevents their communicating together: and we know in fact, that a pane of glaſs, properly covered, will remain electrified much longer than metals.

And this leads us into the reaſon of there being but one way of unelectrifying the pane of glaſs with violence, which is by opening a communication at once through this column of air between theſe two atmoſpheres, as we ſaw it done by the perſon's holding his finger 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 very ſtrong ſpark will appear, and the glaſs be unelectrified at once, or very nearly ſo.

Let us try another way of forming a communication between theſe atmoſpheres, ſo, that the pane of glaſs ſhall

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 to be faſtened to a piece of ſealing wax, and then be 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 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

to be done at once; and the violence was owing only to the encreafed velocity with which the fluid was forced to move into the finger at the upper furface? And that this encreafe of velocity was fo great, that what in one cafe was only a continued ftream of faint bluifh light, was in the other kindled at once into a ftrong fpark of fire?

This may feem a very minute way of confidering the caufes of thefe appearances, but it feems to arife from the appearances, and to be confirmed by the following farther obfervations on the fame fubject.

The firft obfervation is, that when the wire of communication with the machine is brought into contact with the upper cover, and the under cover is left expofed every where to the air; and the machine is put into action, it will never be able to electrify the pane of glafs, though its force be fuppofed ever fo great, becaufe no fluid can be thrown out of the under furface, on account of the joint refiftances of the glafs, the cover, and the air furrounding it.

But it may be urged, that this joint refiftance muft be limited, and confequently a power in the machine may be imagined greater, and confequently fufficient to electrify the pane of glafs. To this I anfwer, 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 confequently, the force of the machine on

that

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 into 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 atmoſpheres; they will of themſelves come together, 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 atmoſpheres lying thus at the two ſurfaces of the pane of glaſs, electrified one *plus* and the other *minus*, with the atmoſpheres ſurrounding 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 atmoſpheres, thus communicating together, act with no greater force at each upper covering, than they did before they communicated together.

Thus,

Thus, when two balls are brought together, when 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 atmospheres will neither of them be confined, but a free passage will be opened to them to dilate into the panes of glass, at those parts where they are electrified *minus*. The electrical fluid therefore 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 of the atmoſpheres on the different ſurfaces of the electrified pane of glaſs, with thoſe on the ſurfaces of 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

allowed to mean, that every body *resists* the passage of the electrical fluid either into them, or out of them, though *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

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 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 glaſs, 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,

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 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

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 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,

plus, or *minus*, in any of the experiments made on them by art, there is an atmoſphere of this electrical fluid formed round them, ſufficiently ſtrong to ballance 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, can not 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 electrical fluid sufficient to ballance the smaller force with which they are attacked; every way similar to the perceptible atmospheres at the surfaces of 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 the yield on being electrified, ſied, 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 rays of 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 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

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 ſo wax, even by the degree of heat raiſed by friction, ſeems to become a conductor of the electrical fluid, rather than any ſtop or impediment to its flow.

The ſame is true of brimſtone; and hence will appear the reaſon of the appearances in the following experiment.

If a glaſs globe be ſo placed as to communicate with one end of the bar of metal, ſuſpended on ſilk ſtrings in the air, and a ſulphur one be ſo placed, as to communicate with the other end of the bar; and both be ſuppoſed to be in equal order, and in equal motion, and equally rubbed, the bar will not be electrified at all; as appears from our not being able to get a ſingle ſpark from it.

Brimſtone is claſſed with glaſs, wax, reſin, *&c.* as naturally reſiſting being electrified with conſiderable force, but like wax does not reſiſt heat on friction ſo ſtrongly as glaſs; and therefore has the atmoſphere on its ſurface (by which it reſiſts being electrified) more eaſily reduced to the ſame degree of weakneſs with that of metals, by friction, than the atmoſphere on glaſs.

In

In the experiment, both the globes are ſuppoſed to be equally in motion, *i. e.* equally rubbed. So ſoon therefore as ever this rubbing the ſulphur globe has attenuated the atmoſphere on its ſurface by heating it, the globe and the machine, that moves it, become conductors of the electrical fluid that is thrown into the bar from the glaſs globe, and convey it away or give it as eaſy a paſſage into the earth, at if the bar itſelf communicated with the earth.

It is the heat therefore ariſing from the friction on the globe of ſulphur, that is the real original cauſe of theſe appearances, by diminiſhing the reſiſtance which ſulphur naturally makes againſt being electrified when left to itſelf unrubbed.

And accordingly, when the globe of ſulphur is kept, unrubbed, without motion; and the glaſs tube is put into motion, the bar will be electrified, and a ſpark will readily be obtained.

The globe of ſulphur, which naturally reſiſts electrification with force enough to be claſſed with glaſs, &c. whilſt it is left to itſelf, changing its nature thus on its ſurface's being heated by friction, and becoming a ready conductor of the electrical fluid through the machine, that puts it into motion, into the earth: is a very ſtrong confirmation of the truth of what we aſſert, that the reſiſtance to being electrified in all bodies is exerted at their ſurfaces, and that the cauſe of this reſiſtance is an atmoſphere (lying at theſe ſurfaces) of the ſame electrical fluid

fluid of different densities according to their different circumstances with regard to the nature and quantity of the bodies immediately surrounding them.

Because it shows us, as we before observed on glass and wax, that heat, arising from the rubbing, attenuates the electrical atmosphere on the surface of the sulphur globe, and so takes off the cause (in our way of reasoning) of the resistance, that sulphur naturally makes to being electrified.

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

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 electrified fluid does *not* always paſs from one body to another by the *ſhorteſt way*, 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

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, 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 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

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, it electrifies 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 at that distance, to which the power of the tube extends, it returns 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 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; 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

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 repell 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 both of the gold and the ſurface of the glaſs, ſo inſtantaneouſly, that no ſenſible heat remains in them, and they immediately after become incorporated, and form an enamel.

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

Air may juſt as properly be called ſound, as this fluid can be called fire. When ſound is produced, the particles

cles 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 expresly, 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 cohæsion 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 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 of 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 ariſing 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 parti-

cles;

cles; 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 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, moſt of which were known to thoſe converſant in theſe things, 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 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,

one, who is difpofed to a philofophical enquiry into any part of the works of the great Author of all nature.

"Omnis enim Philofophiæ difficultas in eo verfari videtur ut a phænomenis motuum inveftigemus vires naturæ; deinde ab his viribus demonftremus phænomena reliqua: et huc fpectant propofitiones generales quas libro primo et fecundo pertractavimus.

In libro autem tertio exemplum hujus rei propofuimus per explicationem fyftematis mundani.

Ibi enim ex phænomenis cœleftibus, per propofitiones in libris prioribus mathematice demonftratas, derivanter vires gravitatis, quibus corpora ad folem et planetas fingulos tendunt. Deinde ex his viribus, per propofitiones etiam mathematicas deducuntur motus planetarum, cometarum, lunæ, et maris.

Utinam cætera naturæ phænomena ex principiis mechanicis eodem argumentandi genere derivare liceret. Nam *multa me movent ut nonnihil fufpicer ea omnia ex viribus quibufdam pendere poffe, quibus corporum particulæ per caufas nondum cognitas, vel in fe mutuo impelluntur et fecundum figuras regulares cohærent, vel ab invicem fugantur et recedunt: quibus viribus ignotis, philofophi hactenus naturam fruftra tentarunt.*

Spere autem quod huic philofophandi modo, vel veriori alicui, principia hic pofita lucem aliquam præbebunt."

POST-

POSTSCRIPT.

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*.

FINIS

ERRATA.

Page 5, Line 23, for *ſtrogly*, read *ſtrongly*.
Page 47, Line 5, dele *to*.
Line 6, dele *be*.
Page 62, Line 8, for *at*, read *as*.