

It may farther be observed, that the whole surfaces of these two stones, except the parts of them where they lay in contact with each other, or where they continually pressed upon the lower surface of the urethra, or bed, which they had formed for themselves in this chanel, are rough, and have several inequalities, or eminences arising from them.

Fig. 2. represents the inferior smooth parts of the two stones, as they appeared when separated from each other, as well as the smooth or polished surfaces of the ends of these stones, which lay in contact with each other, and upon motion rubbed against each other.

XXXV. *Experiments on the Tourmalin: by Mr. Benjamin Wilson, F. R. S. In a letter to Dr. William Heberden, F. R. S.*

S I R,

Read Dec. 6. 1759. **I** Have the pleasure to communicate to you some experiments made upon the *Tourmalin*, or *Ashstone*, which you were so kind as to procure me, together with some others, respecting the vitreous and resinous electricities, as they are called, and the observations I have made thereupon.

The more I am acquainted with electricity, the more I admire a wonderful simplicity which seems to prevail in nature, at least in this part which abounds with phænomena of a very curious kind; whereof many that have passed under my examination of late are so extremely nice, that I avoid venturing to relate them, because I would not willingly subject myself to the censure of incautious observers.

I cannot enter into this particular subject, without first settling a dispute amongst electricians, which subsists at this day, concerning the two electricities; because some consequences, drawn from the several experiments I have to produce, greatly depend upon it.

Polished glass, upon being rubbed properly, has been supposed to *give* an electricity to bodies, and those bodies that receive it from the glass, are said to be electrified *plus*. Whereas wax, amber, &c. upon being rubbed in the same manner, have on the contrary been supposed to *receive* an electricity from bodies, and those bodies which part with it are said to be electrified *minus*. But no experiment, that I know of, has yet appeared to determine which of these electricities does really electrify *plus*, and which of them does really electrify *minus*; though it happens that the fact turns out just as they have all along supposed.

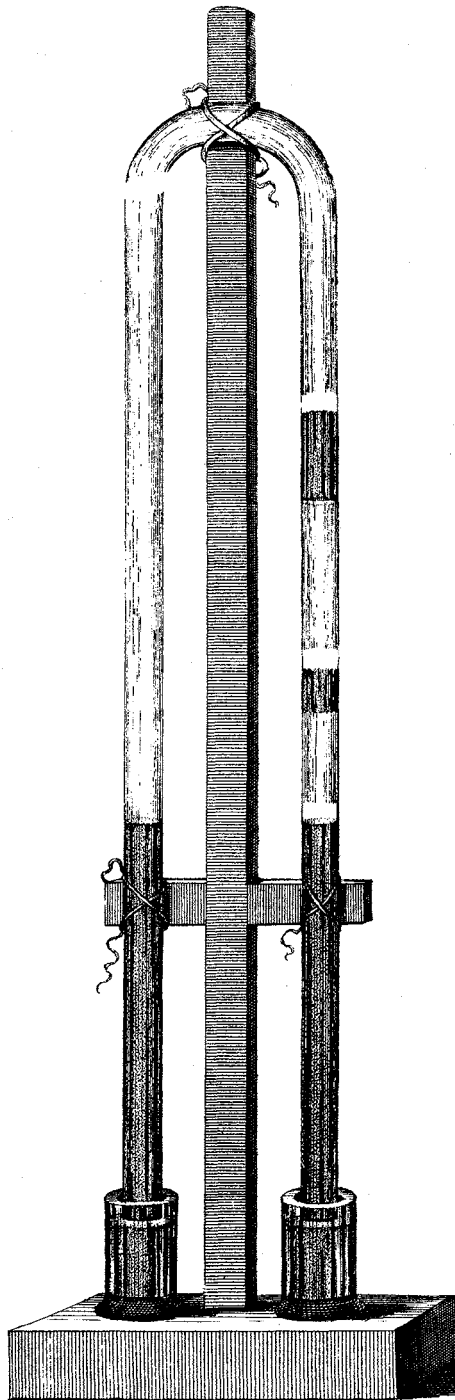
In a second treatise upon the subject of electricity, that I published in the year 1748, several experiments were produced, to shew that all bodies are surrounded with a *medium*, which is *of an exceeding elastic nature, and extends but to a very small distance from the body* when it is not disturbed by heat, or other causes. Since that, other experiments of the like kind have been published in a work, wherein my late worthy friend *Dr. Hoadly* was concerned with me. Among the proofs therein given, is a curious one, which I observed in the *Torricellian vacuum*, where the appearance was remarkably sensible. And what is more singular, that same appearance not only proves the existence of a medium, at or on the surface of bodies, but at the same time determines which
of

of the electricities is truly *plus*, and which of them is *minus*.

You must remember, *Lord Charles Cavendish* first observed the luminous appearance in one continued stream throughout the whole *vacuum* of an exhausted tube. It is a fine experiment, and affords more information than I at first imagined, therefore I shall beg leave to recite it from the *Philosophical Transactions* *, before I give you any farther particulars about it.

“ This apparatus consisted of a cylindrical glass
 “ tube of about three tenths of an inch in diameter,
 “ and of seven feet and half in length, bent some-
 “ what like a parabola, in such a manner, that thirty
 “ inches of each of its extremities were nearly
 “ straight, and parallel to each other, from which
 “ an arch sprung, which was likewise of thirty in-
 “ ches. This tube was carefully filled with mer-
 “ cury; and each of its extremities being put into its
 “ basin of mercury, so much of the mercury ran
 “ out, until, as in common barometrical tubes, it
 “ was in equilibrio with the atmosphere. Each of
 “ the basins containing the mercury was of wood,
 “ and was supported by a cylindrical glass of about
 “ four inches in diameter, and six inches in length;
 “ and these glasses were fastened to the bottom of a
 “ square wooden frame, so contrived, as that to its top
 “ was suspended with silk lines the tube filled with
 “ mercury before mentioned; so that the whole of
 “ this apparatus without inconvenience might be

* Vol. xlvii.



“ moved together. The *Torricellian vacuum* then
 “ occupied a space of about thirty inches. In make-
 “ ing the experiment, when the room was darkened,
 “ a wire from the prime conductor of the com-
 “ mon electrical machine communicated with one
 “ of the basons of mercury, and any non-electric
 “ touching the other bason, while the machine was
 “ in motion, *the electricity pervaded the vacuum in*
 “ *a continued arch of lambent flame, and as far as*
 “ *the eye could follow it, without the least divergen-*
 “ *cy.*”

I imagine *Dr. Watson*, who has described this experiment, did not, when he made the experiment, attend to a singular appearance of light upon one of the surfaces of the quicksilver, because he has taken no notice of it. However, I had a mind to make the experiment myself, and try whether I could not manage the quicksilver, so as to have more than two visible surfaces, in order that I might have that remarkable appearance repeated.

To do this, I let a very small quantity of air into the tube, by which means four columns of quicksilver were obtained, reckoning great and small together; and six visible surfaces, three of which I called upper, and three under surfaces. But the figure in the plate N^o. IX. will give you a better idea of the instrument, than I am able to do by writing.

When the column of quicksilver on the left hand was electrified, and the other on the right communicated with the earth, the stream of light was visible in a darkened chamber, and the general appearance, all the way through the *vacuum*, was a light of a seeming uniform density, excepting at the upper surfaces, for above one tenth of an inch, reckoning
 from

from each surface; and there the light was always considerably brighter; infomuch that a gentleman, then present, who was unacquainted with experiments of this kind, desired to know the reason of the three *knobs* of light appearing on the tops of the quicksilver. I mention this circumstance, because they were remarkably conspicuous; whereas the three under surfaces exhibited no such appearance, the light being rather less bright than even in the general appearance of the whole illuminated *vacuum*.

An electric current, setting in from the glass of the electrifying machine, and passing along the tube through the quicksilver and *vacuum*, and so to the earth, must have caused those bright *knobs* by means of the resistance the fluid met with at the upper surface of the quicksilver in endeavouring to enter it; because the appearances were alike on the three upper surfaces, and nothing of the same kind was seen at the under surfaces. Glass therefore electrifies bodies *plus*, or, in other words, *gives* bodies a quantity of electric fluid more than they have naturally.

I am now to acquaint you with the appearances that the *minus* electricity occasioned.

Instead of the glass cylinder to electrify with, I put a resinous one, preserving the communication, and every thing else, as in the former experiment. In these circumstances, the general appearance of the light in the *vacuum* was the same; but in this experiment the *knobs* of light were on the under surfaces, and not upon the upper.

From the knobs of light being at the under surfaces, and not upon the upper, I concluded that the flow of the fluid was the contrary way to that caused by

by glass; for in this case it appeared to come from the earth, then into the tube, and so on to the resinous cylinder. Those bodies therefore which have no communication with the earth, and from which the resinous cylinder is supplied, must be electrified *minus* *.

These luminous *knobs* (were there no other proofs) are a strong confirmation of the existence of a *medium* at or on the surfaces of bodies, which, to a limited degree, resists or hinders the entrance and exit of the electric fluid. But we have some other experiments to illustrate the same, and the *Tourmalin* does assist us not a little.

There is one thing more, which I am obliged to take notice of, concerning the impermeability of glass by electricity. Our friend, *Dr. Franklin*, seems to found a great part of his system on this opinion.

“ Glass, says he, has within its substance always
 “ the same quantity of electrical fire, and that a very
 “ great quantity in proportion to the mass. This
 “ quantity, proportioned to the glass, it strongly and
 “ obstinately retains, *and will have neither more*
 “ *nor less*, though it will suffer a change to be made
 “ in its parts and situation; *i. e.* we may take away
 “ part of it from one of its sides, provided we throw
 “ an equal quantity into the other.”

This doctrine I could never agree to, so far as it relates to the impermeability of glass, as appears by my letters in answer to the *Abbé Nollet* and *M. le Roy*, on their requesting to know my sentiments about it,

* The same cylinder made rough with emmery, and rubbed with flannel at one time, and with leather at another, will afford these different appearances, the first causing the *minus*, and the last the *plus* appearance.

in the year 1756, the former of whom it is well known could never come into it*. And I am still the less inclined to alter my opinion of this matter, from an acquaintance with the properties of the *Tourmalin*, which I think has furnished me with sufficient grounds for advancing that *Dr. Franklin* is mistaken. I have wondered that *Æpinus* did not take notice of some of the experiments which electrify glass either *plus* or *minus*, because the *Tourmalin* affordeth leading experiments towards it; and can ascribe it to no other cause than a favourable opinion he was willing to entertain of *Dr. Franklin's* hypothesis.

To prove that glass is permeable by electricity, I took a very large pane thereof (I chose it large, that no objection might be made to the experiment) and then warmed it a little, so that it was free from moisture. After that, I held it upright by one edge, whilst the opposite edge rested upon wax, and rubbed the middle part of the surface with my finger. After this, I found that both sides were electrified *plus*. Upon repeating the experiment many times, with different glass and different degrees of rubbing, still both sides were in every experiment electrified *plus*. Now they could not both be electrified *plus* by this treatment, unless some part of the fluid had really passed through the glass; because the virtue could not get round to the other side over so great a surface: besides, the extremities of the glass were not electrified at all. As to electrifying glass *minus*, I find it may be done three different ways. But these and other experiments relating to the permeability of glass, I shall

* Vid. lettres sur l'électricité par M. l'Abbé Nollet, lett. iv.

forbear mentioning, till I have brought you better acquainted with the *Tourmalin*.

The first account I met with of the *Tourmalin*, and the remarkable properties belonging to it, was from a *memoir in the Berlin acts*, printed in the year 1758; wherein it appears, that *F. V. T. Æpinus*, professor of natural philosophy, made several very curious and judicious experiments upon it; the most material of which, prove a *plus* electricity on one side thereof, and, at the same time, a *minus* electricity on the other side; provided the *Tourmalin* is moderately warmed, and even by hot water. These appearances are the more extraordinary, as the like means employed in the same manner upon diamonds, glass, and all other electric bodies hitherto tried, produce no such appearances.

The *Duke de Noya*, who visited this kingdom in 1758, wrote a small treatise on the subject, and published it at Paris on his way to Italy: in this work he mentions *Æpinus's* experiments, but does not admit of a *plus* and *minus* electricity belonging to the *Tourmalin* when heated. On the contrary, he says that the two sides are electrified *plus*, but one of them is more so than the other; and that it is the difference between those degrees which has led *Æpinus* into the mistake.

I remember to have repeated most of the experiments mentioned in the *Berlin memoir*, soon after it appeared in England, with the *Tourmalin* belonging to our friend *Dr. Sharp*, which you fortunately recollected to have seen in his possession many years ago at Cambridge; for it was the only one known of here at that time: and though but a small one, compared

with *Æpinus's*, yet it was large enough to satisfy me that his opinion was well founded. Besides, the trials you made with the same stone before I had it, were a farther confirmation of the truth. However, the following experiments, which I made to procure more *data* towards attaining some simple explanation of these curious phænomena, will sufficiently prove, that one side of the stone is really electrified *plus*, and the other side *minus*. And had the *Duke de Noya* made farther experiments, and pursued the same method I have done, I think he would have been of the same opinion.

The largest *Tourmalin* I had from you, and with which I made the following experiments, weighs above 120 grains. It is of an oval form, and polished: the greatest diameter measures an inch and a quarter, and the least one inch. One side is *plain*, the other is *convex*, but cut into several small planes or *facettes*, something like a rose diamond; the thickest part of which is near one third of an inch. This shape does not seem to me to be the most convenient for making experiments, but I would not alter it, lest the stone should break; for there are several cracks in it; and I fear it will be difficult to meet with another of the same size.

To make experiments with the *Tourmalin* requires the greatest attention, as the appearances sometimes are scarcely sensible, insomuch that I have been obliged to employ the tenderest kind of apparatus, and even interpose a sort of skreen to prevent my breath, or other like motions of the air, from disturbing the experiment.

My apparatus for making many of these experiments, consists of two very small balls made of the
pith

pith of elder, and suspended by two linnen threads of the finest kind: the ends of these threads I fasten to a slip of wood about three inches long, and half an inch broad: then upon a stick of sealing wax nine inches long, fixed upright on a table, or any other convenient place, I fasten the slip of wood, from one end of which the threads &c. hang down five inches, so that the pith balls are about four inches from the table. These balls are always supposed to be electrified *plus*, except where the contrary is mentioned: but with no greater power, than to make them recede from each other about one inch in every experiment.

I prefer the wax stand to a glass one, as the latter when cold acquires moisture very soon, and therefore becomes a conductor: whereas wax, when it is once in good order, will continue a non-conductor for a long time.

Before I set down my experiments, I must mention three truths that are commonly known, and which, for the present, I shall call

GENERAL LAWS.

Two bodies equally electrified *plus* recede from each other, or are repelled.

Two bodies electrified *minus* recede also from each other, or are repelled.

One body electrified *plus* and another body electrified *minus*, to an equal degree, accede, or move towards each other, or, as it is generally expressed, are attracted.

EXPERIMENTS upon the TOURMALIN.

Exp. 1. One edge of the *Tourmalin* being properly fastened to a long stick of sealing wax, I dipped the

stone into boiling water, and continued it there near one minute. On taking it out, and presenting the *convex* side thereof near the pith balls, they immediately *receded* from it, but not very strongly.

On turning the *plain* side towards the balls, it caused them to *accede* to it, but rather at a greater distance in this case, than they *receded* from it in the last. When the *stone* was colder, these appearances were considerably stronger; but, on cooling still more, they were less and less.

Exp. 2. I repeated this last experiment, with the difference only of having the balls electrified *minus* instead of *plus* *. And in this case (as might be expected) the effects were reversed; the balls *acceding* towards the *convex* side, and *receding* from the *plain* side.

These appearances in the two experiments agreeing with one another, sufficiently prove, that *when the stone has been heated by water, one side is electrified minus, and the other side is electrified plus*; which is the first law laid down by *Æpinus*. This state has not improperly been called its *natural state*; because the *heat*, which disposed the *Tourmalin* to afford these appearances, was *uniform* in every part of its surface, and the *water* itself an *uniform* conductor.

Exp. 3. I presented the *convex* side to the flame of a candle, but not so near as to touch the flame, and held it there about one minute, during which time the stone acquired a *plus* electricity on both sides, for

* I did the same thing in every experiment where there was occasion to use these balls, to be more certain of the conclusions.

the balls *receded* from them, but rather with a greater force from the *convex* side, than from the *plain* side.

This appearance proves an *increase of the power in the stone*; because it continued to act for a time like other bodies electrified *plus*. And in regard to the different forces of the two sides, that will be particularly considered hereafter.

Exp. 4. After a short time, the *Tourmalin* being colder, and the remaining heat more equally diffused, it changed its last state to a *plus* and *minus* one; for the *plain* side made the balls *accede*, and the *convex* side made them *recede*.

This change seemed to arise from some alteration, on or near its surfaces, by having the heat equally diffused throughout the stone: if it was not so, I see no reason but that the *Tourmalin* should continue to be electrified *plus* on both sides, whilst any electrical signs remained.

Exp. 5. I now held the *plain* side as near the flame as I had done the *convex* one; and instead of both sides being electrified *plus*, they were electrified *minus*, for each side caused the balls to *accede*.

From this experiment, I had reason to believe that the *stone* was *emptied* (if I may so say) of its electricity; because it continued to act for a time like other bodies electrified *minus*.

Exp. 6. After the same length of time as in the fourth experiment, the *Tourmalin* being colder, it changed its state also; for the *convex* side made the balls *recede*, and the *plain* side continued to make them *accede*, as they had done before.

This

This change seemed likewise to arise from some alteration on the surfaces of the *Tourmalin*, by having the heat equally diffused throughout the stone: if it was not so, I see no reason but that the *Tourmalin* should continue to be electrified *minus* on both sides, whilst any electrical signs remained.

I was naturally led next to examine at which surface of the *Tourmalin* the fluid entered (if any did), whilst it continued heating.

Exp. 7. *Flame* being improper for my purpose, because the electric fluid is readily dissipated by its presence, I made choice of an iron rod, at the end of which was a round knob. This was heated, and afterwards brought to a certain distance from the balls, in order to see if they were moved by it: but not perceiving the least motion, I interposed the *Tourmalin*, with the *convex* side next the balls. They *acceded* a little, and when I removed the heated iron, they returned to their place again. I then brought the iron nearer to the *Tourmalin* than before: the balls in this case *moved* with vigour towards the stone, and continued in *contact* with it for a considerable time; and, after that, they *receded* from it.

On examining the balls, I found they had lost all their *plus* electricity, and were electrified *minus*. I also observed that the *stone* itself was *minus* on both *sides*.

I gathered from this experiment, that the *electric fluid* flowed from the balls towards the stone; because they not only lost their own *plus* electricity, but were electrified *minus*: and, as the *Tourmalin* was *minus* also on both sides, a quantity of electric fluid must
have

have flowed from it towards the iron. That this was really the fact will appear presently.

Exp. 8. When I heated the *convex* side in the same manner as I did the *plain* one, the balls were not moved towards the *Tourmalin*, but from it, and continued in that state. In this experiment they were electrified *plus*, and the stone also was *plus* on both sides.

I gathered from this experiment that the *plus* electricity in the stone did not flow from the balls, because they lost none of their virtue; but it must be *from the iron itself*.

It was now necessary to examine the iron: but I found it very difficult to do this for many reasons, and such as might perhaps be thought too tedious if I mentioned them.

I therefore had recourse to another expedient, where there was a probability of meeting with better success. This was to make use of a tube of glass about two feet long, one end of which I heated red hot and tried the experiments again: observing at the same time the state of the glass after each experiment.

Exp. 9. When the *plain* side of the *Tourmalin* was exposed to the heated end of the glass, in like manner as it was to the knob of iron in the seventh experiment, I observed that about *three inches* of the heated part of the glass was *electrified minus*, and beyond that distance it was *electrified plus*, and continued so even when the glass was very near cold.

This *plus* and *minus* state in the glass, must be caused from the action of the fluid flowing from the

balls and *Tourmalin* towards the glass: because I found that a current of the electric fluid acting against the natural quantity of the fluid in the glass will produce the same effect: for having rubbed a tube of glass, I applied it to the heated end of another tube, and the appearances were exactly alike; that part which was heated being *minus*, and the part beyond it *plus*.

This seems a leading step, towards discovering the truth we are in search after: for the current of the fluid seems to be fairly traced in these circumstances, the heat having disposed the *Tourmalin* properly to let the fluid pass *from the balls, through its substance, towards the glass*.

Exp. 10. I was now eager to try what would be the event, when the heated part of the glass was next the *convex* side of the *Tourmalin*. Upon making the experiment, I found that the tube was electrified *minus* above one foot in length, without the least appearance of a *plus* electricity beyond the *minus* one, as in the last experiment. And this *minus* appearance continued also when the tube was nearly cold.

Now because the *Tourmalin* was *plus*, the balls *plus* also, and the heated glass *minus*, the electric fluid *must have flowed from the glass to produce a plus electricity in the stone and balls*.

Thus you see, Sir, I have discovered *two currents of the electric fluid* passing in contrary directions, the *one of them electrifying both sides of the Tourmalin plus*, and the *other both sides minus*.

MY next step was to put the *Tourmalin* into its natural state (as *Æpinus* calls it,) in order to make farther experiments.

Exp. 11.

Exp. 11. To answer this purpose best, I separated the *Tourmalin* from the wax, and placed it in boiling water for a short time, where it was surrounded on all sides with a conductor and an *equal degree of heat*: then, taking it out of the water, I laid the *convex* side (after it was dry) upon the slip of wood, supported by wax, to which the pith balls were suspended; but no appearance happened, for the balls continued at rest.

Exp. 12. But when the *stone* remained on the wood a little time longer, the balls separated to a considerable distance, sometimes near two inches; and remained so for more than one minute.

In this state they were electrified *plus*, as appeared by their acceding towards amber when it was rubbed and brought near them. From the balls being electrified *plus*, the flow of the fluid from the *plus* side of the *Tourmalin* must have caused some part of the wood itself to be *minus*, and the balls *plus*; because the same effects happened in *Exp. 9.* Besides, electrified glass opposed to the same wood produced similar appearances.

Exp. 13. If, whilst the stone continued resting upon the wood, I brought my *finger* near the *plain* side of the *Tourmalin*, the balls *receded farther* from each other; and when I repeated the approach, it every time affected the balls and made them recede a little more, unless the *Tourmalin* was become too cold; in which case they approached nearer, but still continued to be electrified *plus*.

In this experiment, the *finger* did nothing more than supply the stone with the electric fluid, more readily than the air itself would have done.

Exp. 14. Upon removing the stone by degrees from off the wood, the balls approached nearer and nearer: but when it was taken away entirely they receded again, and in this case were electrified *minus* instead of *plus*.

This is a farther confirmation that the fluid flowing from the *stone* electrified the wood *minus*, by forcing part of the natural quantity in the wood into the balls, and so made them *plus*; but because some part of the fluid was forced likewise out of the balls into the air whilst they continued *plus*, therefore when the *stone* was taken away, they were just as much electrified *minus*, as the force of the fluid flowing from the *stone* was able to drive out of the balls: and I have formerly shewn * that the balls will recede from one another in this *minus* state as they do in a *plus* one, by the crowding in of the fluid from the air &c. on all sides to restore the *æquilibrium*; but being retarded in some degree in endeavouring to enter the balls by the *medium* on their surfaces, an accumulation is formed, and consequently atmospheres similar to the *plus* ones; with this difference only, that one is tending from the body and the other to the body.

Exp. 15. After heating the stone again in boiling water, I laid the plain side upon the wood. In this case the balls continued at rest as they did in the eleventh experiment, when the *convex* side laid upon the wood.

* See exp. and obs. by Hoadly and Wilson.

Exp. 16. But, after a little time, the balls separated an inch or more, and remained so for some time. In this state they were electrified *minus*; for they *receded* from the amber.

The *Tourmalin*, in these circumstances, was supplied with the electric fluid from the wood and balls, as appears from the preceding experiments, when the heated glass was applied; so that the balls must have been in a *minus* state, and some part of the wood in a *plus* one: because of the like resistance at the surface to be overcome, where an accumulation of the fluid must have been caused before it could pass to the *Tourmalin*; as appears by Lord Charles Cavendish's experiment.

Glass electrified *minus*, and applied in the same manner, produced the like effect. As to the method of electrifying glass *minus*, it will be shewn presently.

Exp. 17. Upon bringing my *finger* near the *convex* side, the balls *receded farther from each other*, as they did in the thirteenth experiment: and, on repeating the approach, the balls *receded* a little more, unless the *stone* was become too cold.

The approach of the *finger*, therefore, conducted the fluid from the *stone*, more readily than the air surrounding it.

Exp. 18. On removing the *Tourmalin* the least from the wood, the balls approached nearer each other, and continued to do so as the *stone* was removed farther off; nevertheless they were electrified *minus*, though in a less degree.

Exp. 19. I then removed it intirely, and the balls *receded*, but to a greater distance than at any time before

before in the preceding experiments ; and, instead of being *minus*, they were now *plus* ; for the amber caused them to *accede*.

By the balls *receding* to a greater distance in this last case than in any other experiment I have yet produced, and the flow of the fluid, during the *natural state* of the stone, being from the *minus* to the *plus* side, we have produced another proof, that the *resistance is least at the minus side of the stone* : and from this cause, the tendency of the fluid from the balls towards the *stone* must be greater, than when the fluid tended from the *stone* towards the balls. But there is a resistance appertaining to the wood, that was observed before, and which must be taken into the account, though it is the same in each experiment. Nevertheless, since the tendency of the fluid is different when different sides of the *stone* are exposed, different degrees of the electric fluid will be accumulated. Hence we see the reason why, upon removing the *stone* from off the wood, as in this last experiment, *more* of the accumulated fluid must have *flowed in*, than *flowed out*, because the balls were electrified *plus*.

HAVING endeavoured to explain these *three different states* of the *Tourmalin*, caused by different applications of *heat*, I shall now offer some farther experiments in which *friction* is concerned, and compare them with other experiments of the vitreous and resinous kind, in order to observe how far they agree with one another, and whether the principles here advanced are constant and uniform.

Exp. 20. The *Tourmalin* being again fixed to the wax, I gave the *convex* side one slight rub with my
finger,

finger, and observed, that both sides were electrified *plus*.

When the *Tourmalin* was put into its *natural state*, so as to electrify *plus* and *minus*, I gave the same side another slight rub; and in this case both sides were electrified *plus*.

Upon repeating these two last experiments with the *plain* side instead of the other, the *Tourmalin* was electrified *plus* on both sides likewise; but with this difference, that now they were considerably more electrified than before.

This is a farther argument, that *the resistance is less on the plain side than on the convex side*; and that *the fluid passed through the stone*.

And because so *slight a friction* occasioned such a sensible alteration, we are sufficiently cautioned from *touching* the stone at any time, but when the experiment requires it. The same caution is to be observed with glass, amber, silk, &c.

These experiments put me upon trying the experiment with the pane of glass, mentioned in the beginning of this letter; and, upon finding that the *electric fluid* not only *passed through the glass*, but electrified it *plus*, I had a mind to try, whether I could not electrify it *minus*.

Exp. 21. For this purpose I made use of the same glass, and, when it was a little warmed, I held it within two feet of the prime conductor, which was electrified *plus*. By this method, that part of the glass, which was opposed to the conductor, became electrified *minus on both sides*; but beyond that, a considerable part all round the *minus*, was electrified *plus on both sides*. This effect is of the same kind with

that mentioned in the 9th experiment. In a few minutes, the *minus* electricity disappeared, and the *plus* continuing, diffused itself into the place of the other ; so that now the *whole* was electrified *plus*.

Exp. 22. The experiment so far succeeding, induced me to make use of a less piece of glass, that I might have the *whole electrified minus*. Upon making the experiment, it answered accordingly.

These advances led me to observe the power of electrifying this small piece of glass and the *Tourmalin* at *different distances*.

Exp. 23. I exposed the small piece of glass to the prime conductor, at the distance of two feet, which was the same as before in the 21st experiment, and observed a *minus electricity at both surfaces*.

Exp. 24. As I moved the glass *nearer, to a certain distance*, it was more sensibly electrified *minus*; and after that, on moving it still nearer, the *minus* appearance was less and less sensible, till it came within the distance of about *one inch*, and then it was electrified *plus on both sides*.

These last experiments are farther confirmations of the *permeability of glass*.

Exp. 25. This *plus* electricity in the glass I found might be changed to a *minus* once again, by removing the glass, and holding *it for a time at a greater distance*: which is another proof of the *repulsive power of this fluid*.

Exp. 26. The *Tourmalin* afforded like appearances when in the same circumstances ; with this difference, that they were caused at *greater distances* than those of the glass; and particularly the *plus* electricity was acquired at the distance of *one foot*, or more.

From

From this difference in the power at different distances, I inferred, that the *Tourmalin* resists the exit and entrance of the fluid considerably less than glass, or even amber; for the distances requisite to cause changes in these, were less, than the distances which caused changes in the *Tourmalin*: and since, by exposing glass, and the *Tourmalin*, to an electrified body, at considerable distances, they are rendered *minus*, and at a nearer distance *plus*; it is no inconsiderable argument that their *general laws are the same*; and that the *Tourmalin* differs in nothing from other electric bodies, but in acquiring an electricity by heat. And, in regard to this remarkable effect, the experiments I formerly made, which rendered electrics non-electric, as likewise Mr. Delaval's curious experiments upon earthy substances, are other instances how particular bodies may be so altered, as to suffer the electric fluid to pass through them or not, according to the different degrees of heat employed in the experiment.

But to proceed with our observations on rubbed glass.

Exp. 27. Having by me a pane of glass, one side of which was rough, and the other smooth, I rubbed it slightly on the rough side; upon doing which, both sides were electrified minus.

Exp. 28. I treated the other side in the same manner; after which, the *minus* electricity was changed to a *plus one*, on both sides.

Now, because the same glass afforded different appearances when different sides thereof were rubbed, and there being no other difference in the circumstances of the two experiments, except that in the surfaces themselves, the one being rough, and the

other smooth; it follows, that the power of electrifying *plus* or *minus* arises from one and the same fluid.

Exp. 29. I then had a curiosity to try, whether I could not, by rubbing, make one side of this glass *plus*, and the other *minus*, at the same time. This I effected, after both sides were made *plus*; for, by rubbing the rough side of the glass less, than I rubbed the smooth side, that became *minus*, and the smooth side continued *plus*. I rubbed the rough side less, because I found from experience, that *rough glass* required a less power to electrify it *minus*, than *smooth glass* did to electrify it *plus*; and therefore I concluded, that the *medium on the different surfaces* has *different powers, the greatest belonging to the smooth, and the least to the rough surfaces*; as Sir *Isaac Newton* has shewn concerning *light* falling upon polished and rough glass.

I remember an observation of the like kind, which Mr. Short made, upon having occasion to heat one of his *metal speculums*, behind which was fastened a wooden handle. This *speculum* he placed near a strong fire, with the polished surface towards the same, where it continued above an hour, *without receiving the least degree of warmth*. That power therefore, which *reflected the heat*, must certainly be of the same nature with that, which occasioned the *knobs* of light in *vacuo*, mentioned in the first experiment. I have frequently endeavoured to cause alterations in that power, with a view to be better acquainted with its laws; and, amongst other attempts, I rubbed electrics against electrics.

Exp. 30. The first trial was with the *Tourmalin* and amber, which produced a *plus* electricity on both

sides of the *stone*, and a *minus* one in the amber. Afterwards I electrified the amber, and held it near the *Tourmalin*; still both sides were *plus*: and if I rubbed the *Tourmalin* whilst the amber was electrified, it continued *plus*. Then I rubbed the *stone* with glass; notwithstanding which, both sides of the *Tourmalin* were *plus*, and the glass *minus*.

Exp. 31. But when the glass was electrified *plus*, and held near the *Tourmalin*, as I had done before with the amber, in this case, both sides were electrified *minus*.

These experiments seem to shew, that *where electric appearances are produced, by the rubbing of any two polished bodies together, that body, whose substance is hardest, and electric power strongest, will be always plus, and the softest and weakest, always minus.* It was from this theory, with which I had the pleasure to acquaint you, that I was desirous of trying to electrify the *Tourmalin minus* by rubbing, not having at that time been able to do it. I fixed upon a *brilliant diamond* for this purpose, as being the *hardest* body, and *strongest electric*, that I was acquainted with; and, upon rubbing the *Tourmalin* with it, my expectations were answered; for both sides of the *Tourmalin* were electrified *minus*, and the diamond *plus*.

Exp. 32. These experiments succeeding, I rubbed glass against glass, and found, that *they electrified each other*; but one of them was *plus*, and the other *minus*.

Exp. 33. Two pieces of amber, treated in the same manner, were also electrified *plus* and *minus*.

Exp. 34. When I rubbed glass against amber, the former was *plus*, and the latter *minus*.

Exp. 35. Two *Tourmalins* being rubbed against each other, one became *plus*, and the other *minus*.

From all which it appears, that some alteration was made in the *medium* on their surfaces; otherwise these opposite effects could not have been produced: and in regard to the same bodies producing different effects, it is not improbable but they may differ in degrees of hardness, polish, or of their electrifying power.

Now, as *electrics*, rubbed against *electrics*, occasioned *electrical appearances*, I was encouraged to try what would be the effect of *air*, if I rubbed, or rather forced, it against *electrics*; for I supposed the particles of air to be surrounded with a *medium* of the same kind as grosser bodies, which is the cause of their being so elastic.

Exp. 36. To do this, I only made use of a common pair of *bellows*, and having brought the *Tourmalin* near to the end of the pipe, I found, after it had received about *twenty blasts*, it was electrified *plus* on both sides.

Air therefore seems to be less electric than the *Tourmalin*.

Exp. 37. Into the place of the *Tourmalin* I brought a pane of glass, and blew against it the same number of times as in the former experiment. When I examined both sides, they were electrified *plus* also, but less than the *Tourmalin*.

Exp. 38. Amber, treated in the same manner, was electrified less than the glass.

Exp. 39. I had recourse next to a smith's *bellows*. The difference these occasioned was only a much stronger

stronger electricity in the *Tourmalin*. Amber was still weaker than the glass, and the glass weaker than the *Tourmalin*.

Still having in view the *medium* on the surfaces of the particles of air, I considered, that *heat* would rarify it; by which means, air, having its resistance lessened, would more readily part with the electric fluid, and of consequence electrify more powerfully.

Exp. 40. The *pipe* of the *bellows* being made red-hot, I blew against the *Tourmalin* twelve times only, which was eight blasts less than in the former experiments with cold air. In this experiment likewise the *Tourmalin* was electrified *plus* on both sides, but to a considerable degree more, than was done in the 36th and 39th experiments. The hot air had the same effect upon glass; but electrified it less than the *Tourmalin*: and amber, though, like the other bodies, it suffered an increase of power by the same treatment, was electrified the least of all.

From the air electrifying more powerfully when it is hot, than when cold; and the *Tourmalin* being electrified more than glass, and glass more than amber, as appears by these last experiments, we seem to have obtained a proof, that the whole atmosphere is constantly promoting a flow of the electric fluid, by the alternate changes of heat and cold: and further, that air is not only less electric than the *Tourmalin*, but less than glass, and even amber.

Exp. 41. When the *Tourmalin* had received the same number of blasts against the *plain* side, whilst my *finger* touched the *convex* side, it afforded different appearances; for the plain side was electrified *plus*, and the other *minus*. After a short time, *both sides*

sides were plus; and some time after this the *stone* recovered its *natural state*, the *plain side* being *minus*, and the *convex plus*.

What appeared singular in this experiment, was, that the *middle state of the stone* should be *plus on both sides*. But we no more wonder at this, when we consider, that there were two causes to produce these effects: the first was *heat*, which put the *stone* into an *unnatural state* (as *Æpinus* had observed before); for, upon cooling, it would recover its *natural state*, and consequently afford different appearances.

But the second cause, which electrified both *sides plus* when the *stone* was in an intermediate (or, as *Æpinus* calls it, *neutral*) state, between the two extremes, was, that then the effects of the air itself took place, and electrified *both sides plus*, as it had done before in the 35th experiment.

Exp. 42. The convex side was now presented to the *bellows* in the same manner, and received an equal number of blasts. In this experiment *both sides were plus*, but weaker than in the last experiment; and, after a time, the *stone* returned to its *natural state*, affording a *plus* and *minus* appearance.

This property in the air, of electrifying glass, amber, &c. will, in all probability, account for an experiment I met with in *France*, made by *M. Le Monnier**, who shewed me, at *St. Germain en Lays*, two or three long wires, which he had suspended horizontally from the palace to his apartment, above

* See the *Memoirs of the Academy of Sciences*.

thirty feet from the ground (the same experiment that was made by my friend the *Abbé Mazeas*, and communicated, with several others, to the Royal Society *), in order to observe the *electric effects during thunder storms and cloudy weather*. These wires he frequently found *electrified in a small degree, when the day was clear, and without the least appearance of a cloud*. Might not this effect therefore arise from the *friction of the air against the wires?*

From considering all these things, and what these last experiments have taught us concerning the different effects produced by *hot and cold air*, it seems probable, that many of the curious operations in nature arise from a *constant flux and reflux of the electric fluid*. And if the observation be true, that air, free from moisture, during tempests and hurricanes in the night, frequently affords a faint kind of light, resembling what is seen in an exhausted receiver, through which the electric fluid is caused to flow, as also it occasions a more general clearness in the heavens, than would appear were there no such violent agitations; it is reasonable to imagine, that a *flux of the electric fluid in the air*, is the cause of such appearances; for the same reason that *our artificial blasts* produced the electrical effects I have just now mentioned.

But it remains to be inquired into, whether the *Tourmalin* is so disposed by nature, as to suffer the electric fluid to pass through it only in one direction, like *magnetism* through the *loadstone*, or is indifferent which way it flows.

* See the Transactions of the Royal Society, Vol. XLVIII.

Exp. 43. Upon examining a *Tourmalin* which was flat on both sides, and polished, except upon its edges, part of the edge appeared *plus*, and another part opposite to it *minus*; so that a line, drawn from the *plus* part through the centre of the stone to the other side, would pass through the *minus* part.

Exp. 44. Two smaller *Tourmalins*, that were flat also, and polished, like the other, exhibited the same appearances.

Exp. 45. Another *Tourmalin*, which was also flat, but unpolished, afforded a fourth instance of this kind.

Exp. 46. The first of these *Tourmalins* was afterwards polished, as well at the edges as the surface; in order to see, whether that would make any alteration; but I found it still retained its former electrical state.

Exp. 47. I experienced the same with another *Tourmalin*, which had been rough likewise.

Exp. 48. I then, with a little emery, made that edge, which was *plus*, rough again, preserving all the rest smooth; but I could not perceive, that any alteration was made by it. I did the like with the edge of the other polished *Tourmalin*, without being able to observe any difference.

Exp. 49. As to the small *Tourmalin*, that is plain on one side, and a little convex on the other, which you observed to be a very good one, it is *plus* on the plain side, and *minus* on the convex; which is contrary to the large *Tourmalin* described in the beginning of this letter.

Exp. 50. I had an opportunity of trying another *Tourmalin*, now in the *British Museum*, which afforded another instance of the singular disposition of this

this *stone*. The *Tourmalin* I now speak of, is *plain* on one side, and *concave* on the other; with a small flat border round it. It was square, but one of the corners had been broken off. When I examined it, the broken part was *plus*, and the corner opposite to the broken part *minus*: so that here also the electric current ran through the stone in a diagonal line.

Exp. 51. Every one of these *Tourmalins*, except that in the *British Museum*, I greased all over, and, whilst they were warm enough to preserve the grease liquid, I tried each *Tourmalin* separately, but found, no alteration in the virtue of the stone, except weakening it a little; though it is well known, that moisture, of any sort, readily conducts the electric fluid; and therefore, if the *Tourmalin* had not a fixed kind of electricity, the *plus* and *minus* observable on the two sides of the stone, must, by this treatment, have united, and destroyed each other: the *plus* side parting with as much of the fluid, as the *minus*, on the other side, wanted, to restore the *equilibrium*.

Upon the whole, all these experiments do most clearly prove, that the *Tourmalin* suffers the electrical fluid to pass through it only in one direction, and so far it bears some analogy to the loadstone. And as the loadstone loses its virtue by being made red-hot, I was desirous to see what would be the event in the *Tourmalin* under the same treatment.

Exp. 52. I therefore put one of the flat *Tourmalins* into a strong fire, for half an hour; but could not, afterwards, perceive the least alteration. I made the same experiment upon another *Tourmalin*, with the same success.

Exp. 53. Lastly, I heated the stone again, and, whilst it was red-hot, I threw it into water; by which treatment the *virtue of the Tourmalin was intirely destroyed*, and it had the appearance of being shivered in many parts, without breaking.

IN regard to the internal frame of the *Tourmalin*, we can say nothing; yet so much I have learned, by these experiments, that there are *three different methods of heating the Tourmalin*, which produce different electric appearances; that *different degrees of heat*, afford different appearances; that *friction* has the same effect upon it, as upon glass; and that the *Tourmalin*, when it is heated properly, suffers a current of the electric fluid to pass through it *in one direction only*: so that the *Tourmalin* hath, as it were, *two electrical poles*, which are not easy to be destroyed or altered; and farther, *that there is not any substance in nature, which we are acquainted with, that the electric fluid does not readily pass through*; that there seems to be a *constant flux and reflux of it in all bodies, as well in the air as in vacuo*, occasioned by the alternate changes of *heat and cold* in every part of this globe. All which things do very much confirm the opinion I formerly entertained, and attempted to prove by experiment, that the *electric fluid is diffused throughout the whole earth*, as well as in *the air* surrounding it*.

How far this flowing of the electric fluid may be concerned in the ordinary operations of *nature*, by keeping up that motion, which seems so necessary in the several parts of the *grand machine*, I leave to others, who may be more successful in their researches.

* See a Treatise on Electricity; the 2d Edit. by B. Wilson.

By these advances, we have likewise attained to a more certain knowlege of that *medium*, which seems to *surround the surfaces of all bodies*, and which, being of a greater or less density, or exercising (if I may so say) a greater or less resistance, produces different effects, as the *electric fluid* (or *æther*, if you please) *passes in and out of bodies*, whenever they are disturbed by any external violence; and, were it not for this *medium surrounding all bodies*, the electric fluid, I apprehend, could *neither be accumulated, nor detained, in any body* whatsoever.

This principle is very simple, and seems to be of a very general nature. Our great *philosopher* adopted it from many experiments: he supposed, that the wonderful phænomena of *nature*, particularly those of *light*, were not to be explained without it; and therefore did not scruple to propose it, as a principle, to be farther inquired into.

I shall think my time well employed, if, by these inquiries, I have at all contributed to shew, how much we are indebted to this happy *Interpreter of Nature*, and have afforded fresh occasion, by the light of his labours, to admire, and adore, the *first cause of all things*. But whatever may be the success of my endeavours, they have at least been attended with a satisfaction, which not a little increases that desire I have ever had for pursuits of this kind. I am,

S I R,

Your most obliged humble servant,

Great Queen-street, London,
Nov. 9th, 1759.

Benja. Wilson.