

LIV. *A Letter from Mr. B. Wilson, F. R. S. and Member of the Royal Academy at Upsal, to Mr. Æpinus, Professor of Natural Philosophy in the Imperial Academy of Sciences at St. Petersburg, and Member of the Academies of Berlin, Stockholm, and Erfurth.*

Read Dec. 23, 1763,  
and  
March 1764. **I** Have not had the favour of hearing from you since I sent you some experiments upon *Gems* similar to those produced by the *Tourmalin*, which induced me to conclude, that *the electric current moves always along the grain thereof*. It was the more agreeable to communicate these new experiments to you, because from the least hints the greatest discoveries have been made; and what may we not expect from that curious observer of nature, who first discovered these extraordinary qualities in the *Tourmalin*, which have since excited the attention of so many Philosophers.

Your treatise upon this stone, published in 1762, seems to be the same you formerly mentioned in a letter to me. The *remarks* at the end of that work interest me in a particular manner, as they contain objections to several parts of the letter to Dr. *Herberden*. I am obliged therefore to say something in the defence of my own experiments and deductions, which I hope will merit your attention, and remove your difficulties.

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In repeating such of the experiments with the *Tourmalin* as were most proper to answer your objections, I accidentally observed an appearance which has given rise to some new experiments, very simple, and of great consequence in electric researches. These discoveries you shall be acquainted with, before I conclude this letter, that I may have the pleasure of hearing your sentiments concerning them, the sooner.

I am glad to find we \* agree in admitting what are called the two species of electricity, one whereof consists in the augmentation of the electric fluid, and the other in it's diminution. But still, notwithstanding the experiment made with the *bent tube* in order to determine that interesting question, you seem to doubt which of these is the *plus*, and which the *minus* electricity.

You say † I have proved the opposition of the two species, by the knobs of light appearing at the *upper*

\* Mr. Wilson reconnoît de même que moi, l'existence de deux espèces d'électricité, dont l'une consiste dans l'augmentation, et l'autre dans la diminution, de la quantité naturelle du fluide électrique. C'est une question qui doit intéresser chaque physicien, de démêler, quelle de deux électricités, ou la vitrée, ou la résineuse de Mr. du Fay, est la positive, et quelle en est la négative ? Quant à moi j'ai déclaré, il y a long tems, dans mon *Sermo de similitudine electricitatis et magnetismi*, que je ne connois aucune expérience, propre à décider cette question. Je suis maintenant encore du même sentiment, car la belle expérience de mylord Charles Cavendish, rapportée ici et perfectionnée par Mr. Wilson, me semble laisser pareillement la chose indécise.

† Quant à l'opposition des deux espèces d'électricité, je conviens qu'elle est fort clairement prouvée par l'expérience de Mr. Wilson, et par ce phénomène, que la lumière électrique est beaucoup plus vive qu'ailleurs dans le vuide, aux surfaces supérieures des colonnes du mercure, (jusqu'à y former comme des boutons

*per surface* of the quicksilver, when one electrifies with glass; and at the *under surface*, when one electrifies with wax, or sulphur; but that, in your opinion, is all which can be concluded from it. You then declare, that was you disposed to argue against me, it would be in this manner. “ It is easy to

de lumière) quand on électrise avec un tuyau de verre; et que ces boutons se trouvent au contraire aux surfaces inférieures du vif argent quand on se sert d'un bâton de cire d'Espagne ou de soufre. Mais selon moi c'est tout ce qu'on en peut conclure. Voilà comme je raisonnais, si j'avois envie de disputer contre Mr. Wilson. Il est facile de concevoir, que, quand un fluide (et sur tout un fluide élastique, ou dont les parties se repoussent mutuellement) sort d'un corps, le fluide, dis-je, doit être plus dense proche de la surface d'où il sort, que là, où il trouve plus de liberté de se répandre. La surface donc, proche de la quelle la lumière électrique est la plus vive, doit être celle, de la quelle sort le fluide, qui cause ces apparences lumineuses.

Ces raisonnemens, n'ont ils pas autant de probabilité, que ceux de Mr. Wilson? Mais ne prouvent-ils pas directement le contraire de ce que Mr. Wilson a avancé? Ne prouvent-ils pas, dis-je, que l'électricité résineuse est la même que la positive, et la vitrée la même que la négative?

Pour dire le vrai, le raisonnement dont je me suis servi, me semble presque avoir plus de vraisemblance, que celui de Mr. Wilson. Il est incontestable, que la matière électrique entre très librement, et sans éprouver une résistance considérable dans les métaux et autres corps non électriques, comme je le prouverai clairement tout à l'heure. Le raisonnement de Mr. Wilson semble, par conséquent, être fondé sur une hypothèse gratuitement admise, savoir, que la matière électrique s'accumule à la surface du vif argent, parce qu'elle n'y peut entrer librement.

Je ne pretends pourtant pas prouver par tout cela, qu'effectivement l'électricité résineuse soit la même que la positive, et la vitrée au contraire la même que la négative. Mon unique but est de faire comprendre, que cette hypothèse est au moins aussi probable, que celle, pour laquelle Mr. Wilson s'est déclaré.

“ conceive that when an elastic fluid issues from a  
 “ body (as from the quicksilver in the bent tube) it  
 “ will be denser at the surface from whence it issues,  
 “ than it is where it finds more liberty to expand  
 “ itself. And therefore the surface near which  
 “ the electric light is the brightest, should be that  
 “ from whence the fluid issues, which causes those  
 “ luminous appearances.”

This reasoning appears intirely new to me, and I am at a loss to comprehend why an elastic fluid confined within a tube, whose sides are supposed parallel, will be denser at the surface of a body from whence it issues, than in any other part; since its expansive force must in that case be limited by the sides thereof. As you have not given any particular experiment to prove what you assert in this case, you will therefore give me leave to differ from you in opinion. If, as you say, there is really more liberty for an elastic fluid to expand in any other part of the exhausted tube, than at the surface itself, you must produce some evidence in favour of that opinion, before it can be admitted. On the other hand, I should have thought, if all *resistance* is supposed to be removed from within the tube, the liberty, for the fluid to expand itself, will be equal in every part; reckoning from the surface of one column of quicksilver through the whole void, to the surface of the other column of quicksilver.

As I am for supporting this opinion, let us examine it more particularly, and attend only to the appearances which glass affords in certain circumstances: because when the direction of the fluid,

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caused by glass, is once traced, that which is caused by wax, amber, or resin, follows of course. The electric fluid, when it is emitted from any smooth surface of metal without edges, or angles, appears, in certain circumstances, to issue from all parts of that surface equally. This fact, I apprehend, is so well established that it needs no further proof.

Now the column of quicksilver being confined by the sides of the glass, which are supposed parallel, the top of the quicksilver will answer to the smooth surface described. The electric fluid therefore that is to pass from it, into the void space, which is of the same diameter with the column of quicksilver, will move forward within the hollow of the tube to the next column of quicksilver. And since no resistance is supposed to be within any part of the vacuum, there can be no cause for any accumulation: consequently when the fluid is suffered to pass along the tube, the appearance ought to be the same at the surfaces, that it is in every part of the void space. But by my experiment there is a greater quantity of light seen at the second surface of the quicksilver, than in any other part, (when polished glass electrifies the first column) and that this light which appears so dense, extends itself about one tenth of an inch from the surface. Whereas the light extending all along the intermediate hollow of the tube, appears to be much thinner, and rarer, and of an uniform density. I conclude therefore that this luminous accumulation at the second surface is caused by a *resistance* exerted at, or near, the *surface* of the quicksilver: when the electric fluid, issuing from  
the

the glass that electrifies it, is pushing forward to enter the second column of quicksilver.

At the time I related this experiment with the bent tube in the letter to Dr. *Heberden*, I omitted certain phenomena, which attended the experiment, greatly favouring the doctrine here advanced. If when glass is electrified, and applied to the first column, we suffer the electric fluid to pass along the tube in small quantities only, and at short intervals, little luminous streams will be seen to move from the first to the 2d column of quicksilver, and consequently from the glass. The like appearances happen, but in a contrary direction, when resin or amber is made use of, and applied to the same column. Glass therefore electrifies *plus*; or fills bodies with more of this fluid than belongs to them naturally: and resin, &c. *vice versa*.

When you say, your reasoning appears to have as much probability as mine, I believe you do not include your observation, that the electric fluid enters with great freedom, and without *any considerable resistance*, into metals and other non-electric bodies. Because the words *any considerable resistance*, imply *some resistance*, which is all that is contended for: and a very small *resistance* will occasion very extraordinary appearances, as I shall be able to shew by and by. There is no occasion to trouble you with any further arguments to prove this *resistance*; of which yourself seem to entertain no doubt; and the *accumulation* caused by the *resistance* is evident.

In your second remark, respecting the *impermeability of glass*, you say you agree with Mr. Franklin \* as to the

\* Je me tourne vers un autre objet, sur lequel, il me semble, que Mr. Wilson a eu envie de savoir mon sentiment. C'est la question de l'imperméabilité du verre. On peut bien savoir par mon livre: *Tentamen Theoriae Electricitatis et Magnetismi*, ce que j'en pense, et que bien que je tombe d'accord avec Mr. Franklin, de l'existence de cette imperméabilité, je diffère pourtant beaucoup de lui, par rapport a plusieurs autres points. Il ne seroit donc pas à la vérité nécessaire, d'exposer ici de nouveau mon sentiment, néanmoins je me charge de ce travail, pour ne laisser rien à desirer a ceux qui liront ce recueil.

Qu'on suspende un fil de fer ou d'archal, quelque long qu'il soit, à des fils de soye, et qu'on en électrise un bout par le moy en d'un tuyau de verre, ou d'un bâton de cire d'Espagne. Dans moins d'un clin d'oeil non seulement le bout qu'on électrise, devient électrique, mais aussi le fil entier le fera d'un bout à l'autre, et le fera partout également. Qu'on touche après l'un des bouts, et l'électricité sera détruite dans tout le fil, d'un bout a l'autre, avec la même vitesse, qu'elle avoit été produite.

Qu'on suspende au contraire de la même façon un tube de verre bien sec, ou un cylindre de cire d'Espagne ou de soufre, et qu'on le traite de la même manière. Le succès en sera tout à fait différent. Ce n'est pas le cylindre entier, qui devient alors électrique dans un instant, mais seulement une partie de la longueur de quelques pouces, ou d'un pied tout au plus, acquiert cette force, et il faut travailler fort long tems, si on veut amener les choses, au point d'en rendre électrique une partie d'une longueur un peu considérable. Qu'on touche après la partie électrisée du tuyau, et l'électricité ne sera détruite, ni dans un instant, ni dans le tuyau entier, comme dans l'expérience précédente. Au contraire, encore que l'endroit touché perde son électricité, il n'en fera de même, des parties tout proches, qui conserveront plutôt leur électricité pendant fort long tems.

J'en tire la conclusion: que la matière électrique, traverse les métaux ou d'autres corps non électriques et se distribue en eux fort facilement et fort rapidement, mais qu'au contraire elle passe par le verre, la cire d'Espagne, et d'autres corps électriques par eux mêmes beaucoup plus difficilement et plus lentement. Cette règle ne doit pas

the existence of this impermeability, though you differ from him in many other points: and refer to your Essay upon *the Theory of magnetism and electricity*. You then relate two experiments, one with a wire, and the other with a glass tube, or cylinder of wax: and observe that the first may be easily electrified, and the latter also, though with great difficulty, to any considerable length. You then draw this conclusion from the two experiments, that the electric matter pervades metals and other non-electric bodies, and expands itself in them with the greatest ease and rapidity. But that on the contrary *it passes through glass, wax, and other electric bodies, more slowly and with much greater difficulty*.

This conclusion, instead of establishing the impermeability of glass, most evidently affirms the contrary: for though, according to your observation, the fluid passes *more slowly* and with much greater difficulty through glass than iron; your admitting *that it does pass at all through the glass*, ends the dispute, as to the point of *permeability*: and at the same time *establishes* my doctrine of *resistance*; at least in glass, and resinous substances.

In regard to my experiments upon the *Tourmalin*, you say\* that the first and second correspond with your discoveries.

pas être prise pour une hypothèse. C'est une loi, prise immédiatement et d'une manière incontestable, de l'expérience.

Cette propriété des corps électriques par eux mêmes, est selon moi la même que celle, que Francklin a appelée d'un autre nom, *impermeabilité*. Au moins, je ne fais consister l'impermeabilité en rien autre chose, qu'en cela.

\* Je viens aux expériences de Mr. Wilson. La première et la seconde s'accordent tout à fait avec mes découvertes. Il n'en



discoveries; but that those from the 3d to the 8th do not agree with your notions and experience: and then

est pas de même, de celles qui suivent, depuis la troisième jusqu'à la huitième, qui demandent d'être discutées avec plus de soin.

J'ai établi comme une loi constante, de l'électricité de la *Tourmaline* : *Que cette pierre est toujours dans l'état contraire (c'est à dire, que son côté positif, est négativement électrique, et le côté négatif l'est positivement) quand un de ses côtés, quel qu'il soit, est plus chaud que l'autre; et qu'elle ne retourne dans son état naturel, qu'après que la chaleur s'est distribuée uniformément en elle.* Par les expériences de Mr. Wilson au contraire, il faut établir une règle tout à fait différente, savoir : *Que la Tourmaline, quand elle chauffée inégalement, des deux côtés, a l'espèce d'électricité, qui est naturelle au côté le plus chaud (c'est à dire, que la Tourmaline est positivement électrique des deux côtés, quand c'est le côté positif, qui est le plus chaud; et qu'elle l'est négativement, quand c'est le côté négatif, qui est le plus chaud) mais qu'elle retourne dans son état naturel, quand la chaleur s'est répandue uniformément.* Cette règle ne s'accorde en aucune façon avec celle que j'avois avancée.

Encore que je fusse tout à fait convaincu de la justesse de mes assertions, et de leur parfaite conformité avec l'expérience, je ne pouvois pourtant que fort difficilement me résoudre, à douter de l'exactitude des expériences faites par un aussi habile Observateur, que Mr. Wilson. J'ai donc mieux aimé supposer, qu'il y avoit, dans la manière, dont Mr. Wilson et moi avions procédé dans ces expériences, quelque différence, qui pût être la cause de la différence qui se trouve dans nos résultats. Je croyois à la vérité de trouver facilement une circonstance sur la quelle on pût fonder quelque soupçon. C'est que moi j'avois toujours posé la *Tourmaline*, ou sur un charbon ardent, ou sur une plaque de métal ou de verre chauffée, de façon, que l'un des côtés de la pierre, avoit toujours touché à quelque corps. Mr. Wilson au contraire, en chauffant la *Tourmaline*, y a procédé tellement que la pierre n'a jamais touché à quelque corps. Voilà une circonstance, qui semble assez importante, pour avoir pu causer une différence, sensible. C'étoit à l'expérience d'en décider.

C'est

then declare that you have established this, as a constant law, namely, that the *Tourmalin* is always in an inverted state ; that is, its plus side is electrified minus and the minus side plus, whenever either of the sides is hotter than the other : and that it does not return to its natural state, until the heat is distributed uniformly therein. My experiments on the contrary, you observe, lead one to lay down a rule intirely different. viz. That the *Tourmalin*, when its sides are unequally heated, exhibits the species of electricity which is natural to the hotter side, that is, the *Tourmalin* is plus on both sides, when the plus side is the hotter ; and minus on both sides, when the minus side is so : but that it returns to its natural state also, when the heat is uniformly distributed. To account for these different opinions, you think it more agreeable to suppose some difference in our methods of making the experiments, than to question the facts I have declared. Accordingly you observe one circumstance which would naturally give rise to such a suspicion. And then tell us, that you always placed the *Tourmalin* upon burning coals, or upon a plate of metal, or heated glafs: so that one side of the stone was always in contact with some (*non electric*) body. That I, on the contrary, in heating

C'est elle qui me force et m'autorise à declarer, que la regle, que j'ai avancée, est la seule, qui lui soit conforme. J'ai échauffé la *Tourmaline* de la même manière que Mr. Wilfon, et j'ai pris garde, qu'elle ne touchât à rien, mais pour le resultat, je l'ai, non obstant cela, toujours trouvé conforme à ma regle, et pas une seule fois à celle de Mr. Wilfon.

Je le repete encore ; il m'est extrêmement difficile, de supposer, que Mr. Wilfon soit tombé ici dans une méprise, néanmoins je suis convaincu, que de ma part, il n'y a assurément, aucune faute. Ainsi je ne sais qu'en juger.

the *Tourmalin*, never suffered it to be in contact with any (*non electric*) body.

Here, say you, is a circumstance that seems of consequence enough to cause a sensible difference; and then you appeal to the experiment which is to decide it: adding at the same time, *it is experiment that obliges and authorizes you to declare, that the rule advanced by you, is the only one that agrees therewith.* Now your experiment tells us no more, than that you heated the *Tourmalin* in my manner, taking care (as you express it) that it was in contact with *nothing*. But notwithstanding this, that *you have always found the event agreeable to your rule, and not one single time to mine.*

From your description of this experiment, your exact method of making it does not appear. I am inclined to believe some material circumstance has been omitted in the method; or that our apparatus's are essentially different; (though you seem to have had a regard to some of the necessary requisites for making the experiment properly :) otherwise, I cannot apprehend why you were not able to succeed: because the experiment always answers with me. Perhaps the *difference in the size of our Tourmalins* may have contributed towards causing the different effects. The *Tourmalins* I employed in the experiments with the *flame*, are *above five times larger than yours*: and must therefore *require a longer time in heating*. Now the degree of heat given to my *large Tourmalins*, was much below that of boiling water; and therefore I suspect, that the *small Tourmalins*, you employed, were made *hotter*; whereas they ought to have

have been at the most, but *about blood warm*. For *since a difference of heat, between the two sides, is absolutely necessary, it is of importance that this difference be increased as much, and as suddenly, as the nature of the circumstances will admit.*

If you follow this rule, I apprehend you will be more likely to succeed: but that you may be the better enabled to make my experiments answer, it may not be amiss to relate them again in the manner they were lately repeated, and frequently, with two different *Tourmalins*, before several *members of the Royal-Society of London*, who are well acquainted with enquiries of this kind.

## EXPERIMENT.

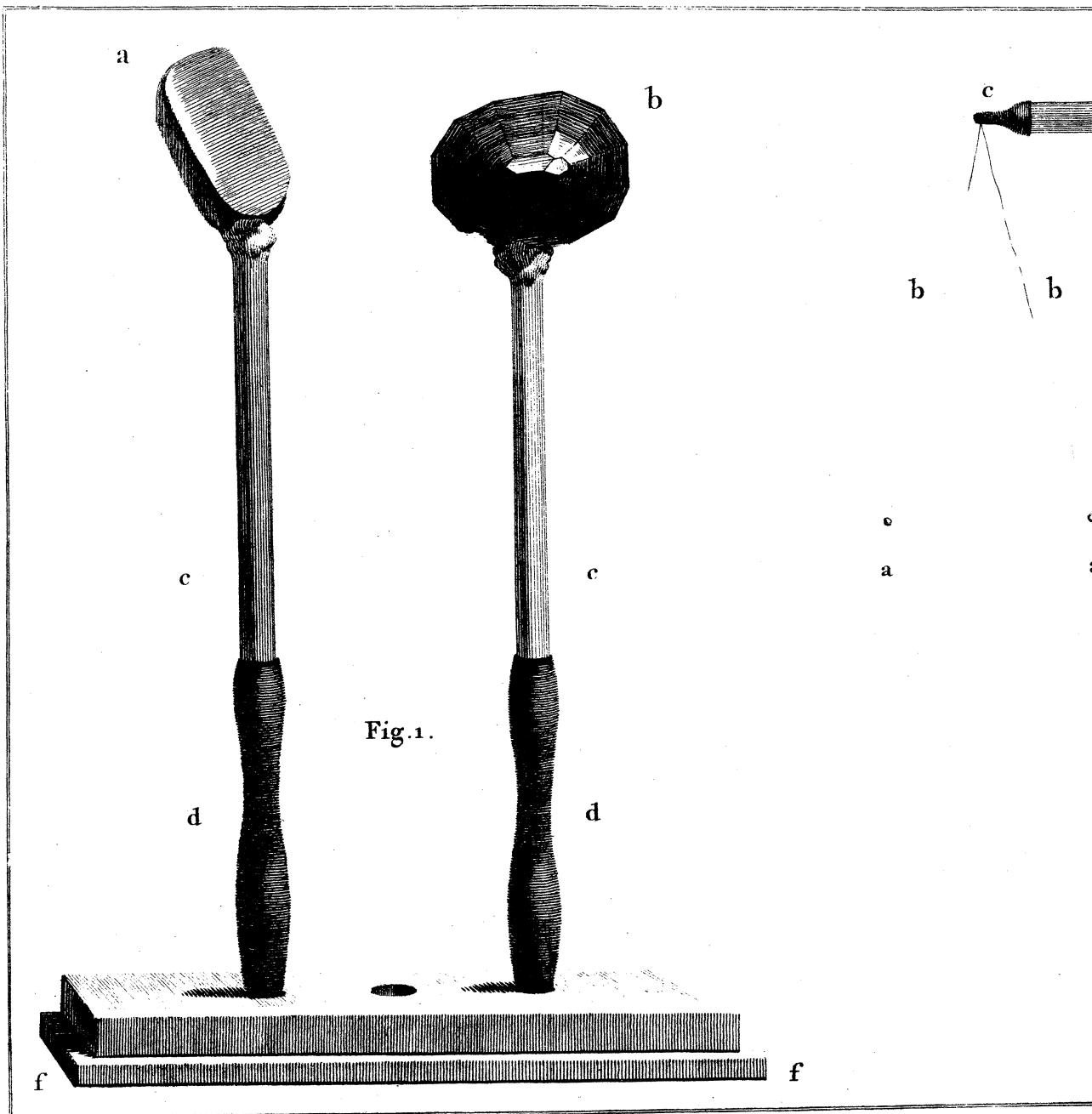
After the *convex side* of the *Tourmalin* (*b*, fig. 1. Tab. XXII.) has been held for a short time, about one tenth of an inch, from the *flame* of a candle, *both sides* thereof *are electrified plus*: and continue so for half a minute or more. And in a short time after, the same *Tourmalin*, without being heated a fresh, or disturbed by any other cause than that of the air surrounding, returns to its natural state; as you have called it; that is, the *plain side* changes to *minus*, and the *convex side* remains *plus*. These appearances continue whilst the *Tourmalin* is cooling.

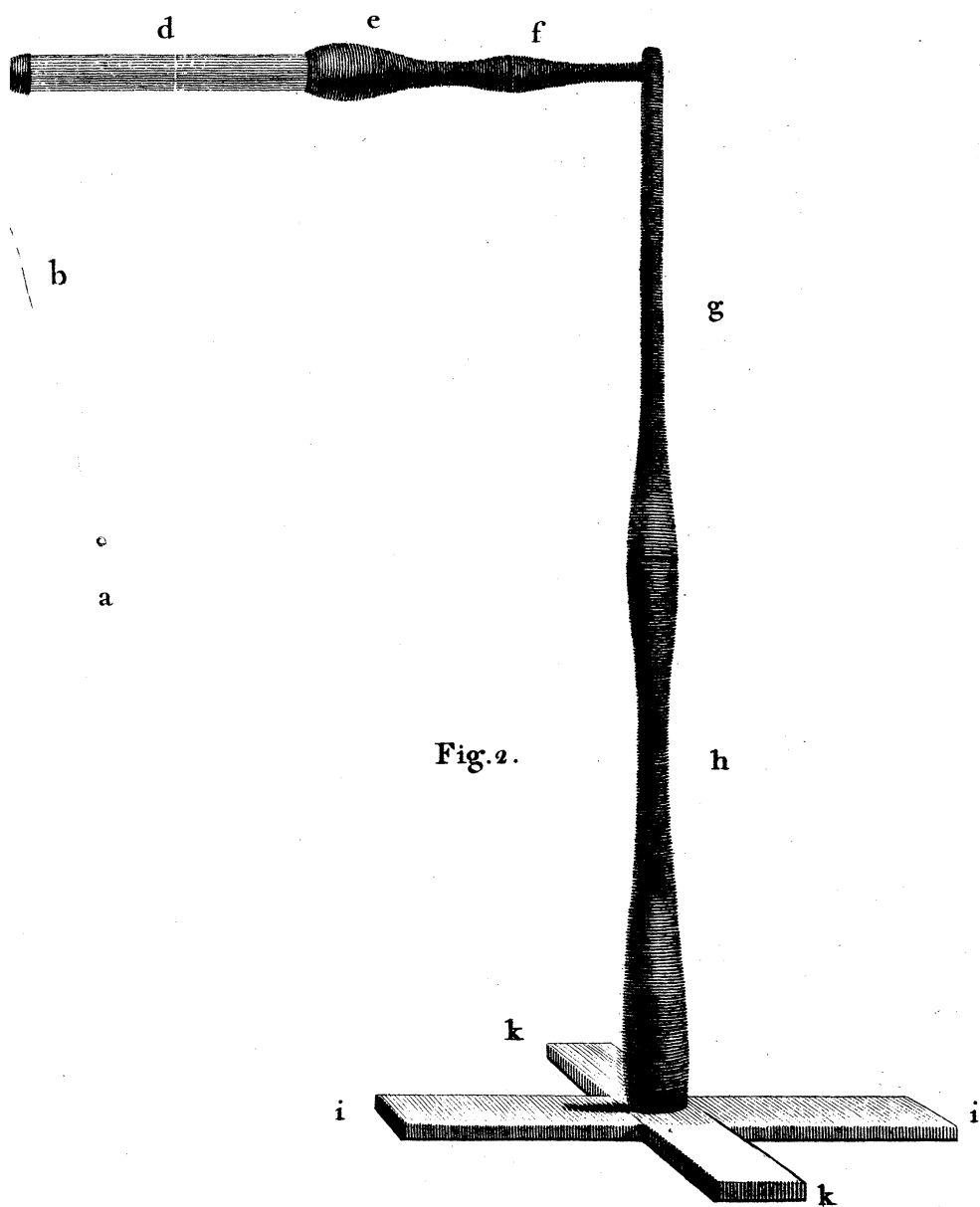
Upon heating the *Tourmalin* again, as before, excepting that the *plain side* was now next the flame, *both sides* thereof were electrified *minus*: and continued so, for half a minute or more; and in a short

time after, the same *Tourmalin*, without being heated afresh, or disturbed by any other cause but that of the air surrounding, returned to its natural state again; that is, the *convex side* changed to *plus*, and the *plain side* remained *minus*.

One of the *Tourmalins* that were employed in these experiments, (*marked b*) belongs to Dr. *Heberden*, and is the same I formerly made use of: but the largest of them was put into my possession by the favour of Dr. *Morton* Secretary to the Royal-Society, of *London*, who received it, with several others, from Mr. *Loton* F. R. S. late governor of the Island of *Cylon*. This stone weighed about one hundred and eighty grains: but the form thereof not being the most favourable for experiments, I had permission to fashion it into such shape as would answer the purpose best. For this end, alterations were made therein; and at, one time, the electric poles (if they may be so called) were pretty near in the longest direction of the stone, and at another time, in the shortest. These trials terminated in the form and size, nearly, as represented by the letter *a*, fig. 2: and the direction of the poles is now in the shortest line that can be drawn between any of the opposite sides.

I have to observe, that when this *Tourmalin* is held between the eye and the light, and viewed in the direction though which the electric fluid is found to pass, it appears of a *darker* colour considerably, than when it is viewed at right angles to the former direction. This appearance obtains in many other *Tourmalins*, especially if they happen to be as conveniently shaped.





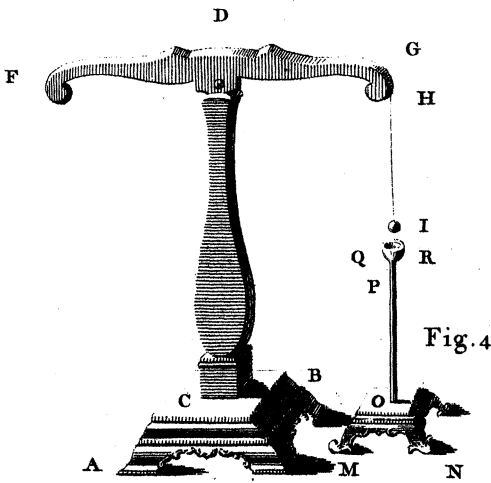


Fig.3.



Fig.5



In regard to the other experiments with the *beated iron*, and *Tourmalin*, which you have only hinted at, it is enough to recommend the utmost care, to avoid the least degree of friction; and the iron itself must be so managed, that there may be *as easy an electric communication with the air, from it, as the flame of a candle is found to have.*

Give me leave, in this place, to make some observations on the *apparatus* you have made use of in these experiments, as it does not in my opinion, seem the best calculated for such nice purposes; at least in our climate, which is certainly more moist than yours.

The instrument first described, \* consists of a *balance* 12 inches long, fixed upon a small stand; from one arm of which, that moves upon a hinge, is suspended, by a silk string, a small cork ball: but there is

\* Le premier de ces instrumens est représenté par la Fig. I. Sur un pied quarré A B. je fis dresser un bâton C D. environ d'un pied de hauteur. Ce bâton avoit au haut une charniere près de D. au moïen de laquelle le levier à bras égaux F G. étoit un peu mobile. Chaque bras de ce levier F D. et D G. avoit la longueur de 7. pouces à peu près. J'employai cet instrument pour pouvoir suspendre commodément le pendule H I. Le levier F G. étoit un peu mobile, afin de pouvoir au besoin hausser, ou baisser un peu le pendule. Pour faire le pendule H I. je pris un fil de soie cruë, j'y attachai par le bas un petit morceau de liège arrondi de la grosseur à peu près d'une lentille. La longueur du fil H I. étoit de 5 à 6. pouces environ, et j'attachai le pendule avec de la cire à un des bras du levier F G. Sur un autre petit pied quarré M N, qui est représenté dans la Fig. II. je mis dans le milieu un tube mince de verre O P, à peu près de 6. pouces de longueur, dont le haut se terminoit dans un hémisphère O R, de deux lignes environ. Il est très-facile de faire ce tube en soufflant une petite boule de verre, comme on fait en faisant un Thermomètre, et en brisant ensuite cette boule jusque'à la moitié. Je me fers dans les expériences de cet instrument pour y poser la *Tourmaline*, afin qu'elle puisse agir librement.

no mention of what the *stand* or *ballance* is made ; though a difference in the materials will, I apprehend, make some difference in the experiment. And if the cork is, as you say, the size of a *pea*, it will be a considerable objection with me : and the more so, if it is to be moistened with water. For the force employed to move it, in some of my experiments, is too inconsiderable. And I should imagine the *ballance* itself could not be very nicely adjusted ; because such an increase in the weight of the cork, by moistening it (as you say) from time to time, ought to make it not only preponderate, but unsteady ; as an evaporation of the moisture is constantly making some alteration in the weight. My *apparatus* contrived for the same purposes, hath already been described, except that part of it which respects the size of the pith balls, and each of these are about the *thirtieth part of a pea*. This smallness, nevertheless, does not require any moisture to retain the electric fluid, as the balls communicate with the finest flaxen threads, and the threads with a slender piece of wood, about one inch long, and the greater part one tenth of an inch thick ; with the angles rounded off, and polished. This is fixed upon the end of a cylinder of amber, properly supported. But that there may be no mistake in the construction thereof, I have given a drawing and description of the one I use at present, see fig. 2. When I at any time want to examine the state of the *Tourmalin*, it is brought slowly towards the electrified balls in an horizontal direction.

Your *glass stand* seems liable to an objection, unless the air be extremely dry : because the mere breathing, if not properly guarded against, in very delicate experi-

experiments, is sufficient to cause an unfavourable alteration ; by the moisture condensing upon it.

We are not told of what materials your *pincers* \* were made, but I suppose of some electric substance ; otherwise in removing the *Tourmalin* from time to time, they may interrupt the experiment, by conducting away the fluid. And even supposing them made of an electric substance, the unavoidable friction may possibly disturb the experiment. I have therefore always preferred a different method, which will appear presently.

I shall now comply with the promise I made in the beginning of this letter, to acquaint you with some simple and interesting experiments upon the *Tourmalin* : most of them were made during the frost in Nov. last, in consequence of an appearance which I then observed accidentally.

With me it has been always found most convenient to fix the *Tourmalin* at the end of a long stick of the hardest kind of Sealing-Wax, and when I am not using it, to put the other end into the top of a candlestick, or other suitable stand, that the stone may be the less exposed to any kind of friction (See fig. I.) And it is a rule with me never to take hold of the Sealing-wax, or even to touch it, but by that end (d) the farthest from the *Tourmalin*. One day on

\* J'emploie encore de petites pincettes ABC, pour ne pas toucher la *Tourmaline* avec les doigts, et je la prens toujours par les côtés, comme il est indiqué dans la Fig. III. afin que la pierre soit touchée le moins qu'il est possible par des corps non électriques. Il faut encore avoir un Tube de verre et un bâton de cire d'Espagne tout prêts, pour qu'on puisse examiner de la manière que nous décrivons plus bas l'espèce d'Électricité produite par la *Tourmaline*.

removing

removing the *Tourmalin* into another room, to repeat the experiments we have here been treating of, I observed *it was electrified*; though no cause for its being so then appeared: the friction arising from the air, in such circumstances, being not sufficient to produce that effect. And I must here take notice, that many months before this fact was ascertained, I frequently suspected the like appearance: but it happened at those times that the effects were so uncertain, and appeared so accidental, that I did not think they deserved attention †.

In tracing out the cause of this appearance, it seemed most necessary to observe the changes in the air with respect to *warmth*: for, it is well known, those changes cause manifest alterations in the *expansion* and *contraction* of bodies.

## EXPERIMENT I.

In a room with a south aspect, and where no fire had been for sometime, *Farb. Therm.* stood at 42. The *Tourmalin* was in the same room, and had continued there some hours, undisturbed, without shewing any signs of electricity. On removing the *Tourmalin* into a warm room carefully, and the *Therm.* along with it, in less than 3 minutes (the *Therm.* having risen to 47) the *convex* side of the stone shewed

† N. B. The experiments upon the *Tourmalin*, by Mr. *Epinus*, which respect the heating and cooling of its sides *equally*, when occasioned by *violent and artificial means*, are, it is apprehended, very different from the ten following experiments; tho' they also respect an *equal* heating and cooling of the stone; because the *degree of heat* employed, is not only extremely different, but the *means* of obtaining it, is so likewise. The one being *natural* and the other *artificial*.

*a minus*, and the *plain side a plus*, electricity. These signs increased for a time, and then decreased, till they entirely disappeared. When this happened, the *Tourmalin* appeared to be of the same temper, in respect to warmth, with the room; and the *Therm.* was raised to 58.

The same degree of warmth, or nearly so, was continued in the room for thirty minutes and more, without causing any alteration; for the *Tourmalin* afforded no electric appearance whatsoever.

## EXPERIMENT II.

I then removed the *Tourmalin* with the *Therm.* into the *cold room* and with equal care\*. The *stone*, some little time after, shewed signs of electricity again: but then, those signs were *contrary* to what had been observed before. For, in this case, the *convex side was plus*, and the *plain side minus*, in nearly the same time; and these signs encreased also for a time, and then decreased, till they entirely vanished. When that happened, the *stone* appeared also of the same temper, in respect to coldness, with the room. And the *Therm.* was fallen to about 42°. During half an hour, or more, after that time, the *Tourmalin* shewed no electric signs whatsoever, nor did the *Therm.* fall any lower.

\* It is indifferent which side of the *Tourmalin* is moved foremost, provided it be done *slowly*.

## E X P E R I M E N T   I I I .

Not content with this last discovery, I removed the *Tourmalin* and *Therm.* into the *open air*. In about three minutes, the *Therm.* having fallen from 42 to 39, the *Tourmalin* was electrified again, in the same manner as in the last experiment. But when the electric signs disappeared, the *Tourmalin* and the air were in this case also of an equal temper: at which time the *Therm.* had fallen to 34. The *stone* was continued in the open air for half an hour or more, but no further electric signs appeared.

## E X P E R I M E N T   I V .

On returning into the room where the first experiment was made, the electric signs were stronger than in any of the preceding trials, and contrary to the two last; for the convex side was *minus*, and the plain side *plus*, which agrees with the appearances in the first experiment.

## E X P E R I M E N T   V .

When the *Therm.* shewed the state of the outward air to be considerably less warm, than what answers to the degree at which water is fixed, the same changes happened, by carrying the *Tourmalin* from it, into a room, where the Quicksilver stood at 34: and afterwards, from thence, back again into the open air.

Sir

Sir *Isaac Newton* carried two *Thermometers*, properly prepared, out of a cold place into a warm one, in order to shew that the warmth was conveyed through the *vacuum*, by the vibrations of a much subtiler medium than the air : and had these last experiments upon the *Tourmalin* at that time been known to him, he must have been agreeably surprised to find them tending so strongly to establish the existence of that *subtile medium* \*. This doctrine receives a further confirmation from the experiments that follow.

## EXPERIMENT VI.

About the middle of this month, December, the wind being full south, and the air loaded with a thick fog, which you know is the worst of weather for electric experiments, the *Tourmalin* afforded the same appearances as before, by removing it from one room to another ; and even into the open vapourous air ; notwithstanding the unfavourable season : but then, the appearances were weaker.

## EXPERIMENT VII.

In the most wet season, and during frequent heavy showers of rain, I repeated the first, second, third, and fourth experiments. And though the electric power was not very strong, yet they always succeeded so well as to ascertain the facts.

\* See Newton, Opt. page, 323.

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

After being acquainted with the preceding experiments, you will not wonder that the *Tourmalin* afforded the same appearances on removing it, in the open dry air, from the sun-shine into the shade; and again, from the shade into the sun-shine.

If these small differences, in the degrees of warmth, are capable of causing such appearances; well may the greater differences; and such more particularly as Mr. *Brown* and *yourself* have experienced in freezing of *Quick-silver*: and therefore *I cannot now agree with you in calling that the natural state of the Tourmalin, which arises from the heat given it by boiling water.*

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

It appears by the preceding experiments, that when the *Tourmalin* was of the same temper with the air in the different rooms, there were no electric signs to be observed. From which we may understand, if the heat of the air should be increased, even beyond that of boiling water, a *Tourmalin* exposed therein for a time, would afford no electric signs; that is, whilst the *stone* continues of the same temper with the air. I have lately caused the heat of the air to be increased, in a convenient room, beyond the degree of vital heat, even to 108: and then placed *two Tourmalins*, in the same room, very near the *Thermometer*, without being able to observe any electric effects; that is, after they had remained therein a short time.



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

Upon a very nice examination, and during some favourable opportunities, *I have observed the Tourmalin to be feebly electrified, when the Therm. varied, up or down only one degree.*

The smallness of the force here required to cause those manifest effects, and even them by *natural means* only, is a new discovery, and, perhaps, deserves the attention of philosophers.

In my first and second letters upon the *Tourmalin*, there are experiments that give us assurances of a *flux* and *reflux* of the electric fluid, or *æther*, at different times, even without artificial means to occasion it. And I did not scruple to advance that doctrine, as appears from a passage in the opticks which I quoted in the second letter, somewhat favouring the same opinion. This you see has happened to be a right conjecture; for these last experiments, are I apprehend, so clear and satisfactory, that there is no room left for a doubt about it. And I do hope they will lead to some useful discoveries. For these forces, however small they may appear, are probably sufficient to answer very great purposes in nature.

Upon considering the effects which heat and cold occasion in the *Tourmalin*, it may not be improper here to observe, that all bodies we are acquainted with, are *dilated* by heat, and *contracted* by cold: and when they acquire the same temper with the air, whether it be hot or cold, the same state of dilatation, or contraction, continues unaltered.

The

The *Tourmalin* we find, when uniformly disturbed on all sides, by changing the temperature of the air ; is not only electrified, but shews two opposite and contrary effects. That is, in passing from a great, to a less, degree of warmth, it is electrified in one manner ; and in passing from a less to a greater degree of warmth, it is electrified in another manner ; which evidently shews, that there is some *power*, belonging to the stone, which is differently affected by such contraction and dilatation. The same thing appears from other different effects it affords, when its two sides are *equally* warm. But the *Tourmalin*, affording no electric appearance whatever, when the whole mass is of the same temper with the air, agrees with the observation that *all bodies cease in that state, to contract or dilate* : and is a manifest indication, that the *fluid*, which causes these electric appearances, is in such circumstances, in *equilibrio* ; and must ever remain so, unless disturbed by violence.

The importance of this last observation bespeaks your attention, as it greatly tends to throw more light upon this curious subject.

From the experiment with the bent tube, mentioned in the former part of this letter, it was proved, that there is a *resistance* exerted at, or near, the surface of the *Quicksilver* where the light is accumulated. This *resistance*, which I apprehend is essential to all bodies, merits a further illustration ; because the electric phænomena in general greatly depend upon it.

When a *bladder* is well blown up, and secured properly, it will yield or give way, and change its form, in that part against which any given pressure is exerted.

exerted. And upon removing the pressure, the bladder will immediately recover its first form.

This yielding or giving way of the form, and then afterwards recovering it, proves an elastic substance existing within the bladder, and between the two sides where the pressure is employed.

In like manner when two glass prisms, or the object glasses of two long Telescopes press upon each other with their own weight only, philosophers know, by the phenomena of light, that they do not touch : and that there must be something between the glasses to keep them at a distance. They also know, by the like phenomena, that more pressing is required to bring them nearer to a contact ; and that when the pressure is removed, they immediately recover their first distance.

Now this yielding or giving way, and the recovery of the distance between the glasses, proves the existence of some *elastic substance* between them respectively. Since we find the effects of applying, and removing, the pressure, exactly similar to the case with the bladder.

Hence it is evident that the same *elastic substance* or *medium* causes prisms, and convex glasses, when pressed against each other, to exhibit several rings of different colours ; by having its density varied : and that it occasions all bodies to act upon light at a distance, by reflecting, refracting, and inflecting it ; and light to act upon bodies, at a distance, by causing a motion of their parts, and heating them.

*This is the medium then which gives rise to the resistance found in electric experiments.*

For

For when a quantity of the electric fluid is forced into the *apparatus*, which supports the two balls, we should from its elastic principle, expect it to pass out again immediately: whereas the fact is, that it passes out by slow degrees; and takes a considerable time in evacuating the *apparatus* effectually. Some *power* therefore must hinder the fluid, at least, in some measure, from escaping: and that *power* must be exerted at, or near, the surface of the body.

To say it is detained by an attraction of the body, will not answer the purpose: for the *power* which is supposed to draw the fluid into it, must certainly be sufficient to hinder it from passing out. Now by the experiment, the fluid does pass out, though slowly: this *power* therefore, which resists its passing out, can be no other than what arises from the *medium* we have proved to be spread upon the surfaces of bodies.

The evidence in favour of this doctrine is greatly strengthened by the following experiments, the three first of which, are well known to electric enquirers.

When glass is properly electrified, and held over the wooden part of the *apparatus* (c) at the distance of six or eight inches, and there *continued* for a time, the balls are separated to a considerable distance.

But upon taking away the glass, the separation is at an end, and there are no electric signs remaining in the balls.

These appearances therefore argue, that no part of the electric fluid, appertaining to the excited glass, passed from it into the wood. And that the cause, which obstructed its passage, is a *resistance*, exerted at or near, the surface of the wood: because we know, from a variety of experiments, that the *repulsive power*

of this fluid acts at great distances, and in gross bodies particularly ; without the fluid being able to enter them. There can then be no doubt that the separation of the balls, in the present circumstances, entirely depends upon this *repulsive power* ; which drives the natural quantity of the fluid belonging to the wood, or part of it at least, towards the balls. And though the *repulsive power* is sufficient to force the fluid from the wood into the balls, and there occasion the effects of a *plus* electricity, (as is found upon trial ; ) \* yet the experiment shews, it is not sufficient, in the same circumstances, to force it out of them, as they cease to be electrified on removing the *power*.

If this is not the case, and the fluid from the glass is supposed to enter to the wood ; I would ask, why the balls do not retain the fluid, or at least some part of it, and continue separated when the glass is taken away ? It would be unphilosophical to say, the glass actually suffered a quantity of the electric fluid to pass from it, into the wood and balls ; and then, on removing the glass, that it took it away again ; or attracted it back : because when the same glass is brought near enough to the wood, the balls will be electrified, and separated so effectually, as to *continue* in that state of separation, after the glass is removed : which proves clearly, that the *repulsive power* is not only great enough to overcome the *resistance* of the balls ; but even to force out some part of the fluid contained therein : therefore in this case the balls are electrified *minus*. And this *minus* may be increased,

\* For the proof of this, see the Essay by Dr. Hoadly and myself, page 13.

by bringing the power gradually nearer, and then removing it quickly.

We may then very justly conclude, that this separation of the balls, is occasioned by the expansive power of the electric fluid, or *æther*, *crowding from without*, and through the air, to enter the balls, and restore the *equilibrium*. And in like manner that the *plus* electricity causes a separation of the balls in consequence of the same electric fluid, or *æther*, *crowding from within* to get out of the balls, and passing through a like quantity of air, in order to restore the *equilibrium*; *for the same medium which appertains to the surfaces of bodies, must resist the exit and entrance equally*: and therefore the one case will be always the converse of the other.

We have seen that on bringing the electrified glass near enough to the wood, the balls are electrified *minus*. If now the circumstances are changed, by bringing the same glass *considerably nearer* to the wood, and much quicker, the balls are electrified *plus*; and continue so for a considerable time after the glass is removed\*: which is the strongest confirmation that this effect entirely depends upon the *resistance* of the wood being overcome, and the electric fluid entering the apparatus, by the nearer approach of the glass.

There are then two different methods of causing a *plus* appearance in the balls. The one, we find, depends upon an actual entrance of the fluid, from the glass, into the wood, &c. And the other upon a quantity of the fluid, originally in the wood, being forced

\* The glass in this experiment must not only be brought quickly towards the wood, but it must likewise be removed from it as suddenly.

from it into the balls, by the *repulsive power* of the fluid appertaining to the excited glass.

In these experiments we also learn, that when the glass is held nearer to the wood, than in the *minus*, and farther from it than in the *plus* appearance of the first experiment, it does not produce any electric signs whatever in the balls. Which shews a kind of *balance* subsisting between the *power* of the glass, and the *resistance* of the wood, &c. For, if we deviate the least on either side from this intermediate distance, electric effects, of the one kind or other, immediately take place.

I shall produce another experiment in favour of these forces, and of the *balance* obtaining between them, in certain circumstances. In the experiment I am about to mention, it is necessary, first to electrify the wood and balls; by properly rubbing that end of the sealing-wax, amber, or glass, to which they are affixed.

You know then, that, if we touch the balls with the hand, we immediatly unelectrify them, and the wood; but not the amber: and that these balls with the wood, will continue unelectrified. But if I *blow* ever so gently *against* that *part* of the *amber* which is *electrified*, the balls will be separated to a considerable distance, and continue in that state. On the other hand, if the *amber* has not been rubbed, no electricity can be produced by the same force of blowing, or even by a blast six or eight times greater: but if the blast is considerably increased, the amber will be electrified\*. By which it appears that in the first case, the *electric power* in the *amber* receives such an additional force from

\* See the letter to Dr. Heberden, page 332.

the slight *friction* of the *breath*, as enables it to destroy the *ballance* and *overcome the resistance at the surface of the wood*, in that part, where it is joined to the amber.

The *Leyden* experiment depends also upon a certain *ballance*, which obtains between the *mediums* at the opposite surfaces of the glass, by the *power of repulsion*\*: but this enquiry, being of a very extensive nature, would lead me too far for the business of a letter; I must therefore refer you to the works quoted last. In selecting the experiments above related to prove the *resistance*, I have purposely confined myself to a few, and those such as appeared to be the most simple and most worthy of attention. You will therefore do me the honor to examine, and consider carefully these experiments and observations, as I think they have sufficiently *established a resistance appertaining to bodies*, independent of the gross matter they contain: and that it arises from the same *elastic medium* which we before proved to exist between the convex glasses.

London Dec. 20, 1763.

I am, &c.

B. Wilson.

\* The consideration of this experiment was particularly attended to, in a former work, by Dr. Hoadly and myself. See also the *Phil. Trans.* Vol. LI. Part II. and Pages 898, 899.

Expla-



Explanation of Fig. 1. TAB. XXII.

- a, b,* Two *Tourmalins* fixed upon
- c. c.* Sticks of the hardest kind of sealing-wax.
- d, d,* Two handles of wood, in which the other ends of the sealing-wax are secured.
- f, f,* A stand of wood, with holes to rest the handles therein, when the *Tourmalins* are unemployed.

Explanation of Fig. 2. TAB. XXIII.

- a, a,* Two very small balls of the pith of Elder, suspended by
- b, b,* The finest flaxen threads about six inches long. The balls and threads together weigh about the *fiftieth* part of a grain.  
These threads are neatly fastened in a small hole on the under side of the thin end, and so as to touch the wood
- c,* Which is mahogany. Every part of this small piece of wood is well polished, and neatly joined to
- d,* the cylinder of amber; which is about four inches long, and near half an inch thick. It is finely polished also, and the other end slides into
- e,* which is part of the arm: *e* is joined to *f* by a screw; and the other end of
- f,* slides into the upper part of the stand *g*. One end of the upright *b* screws on to *g*, and the other end, into the cross pieces *i, i,* and *k k*; which are let into each other by a mortais, and thus

thus secured. The whole *apparatus* takes to pieces easily, for the conveniency of packing up in a case six inches and half long.

The stand is made of Cocoa-tree, without angles, or edges, and well polished.

The size of my *apparatus* is about twice as large as the drawing before you.

