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 agreable 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. *Heberden*. 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.

In

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 intereſſer chaque phyſicien, de demêler, 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 declaré, il y a long tems, dans mon *Sermo de ſimilitudine electricitatis et magnetiſmi*, que je ne connois aucune expérience, propre à decider cette queſtion. Je ſuis maintenant encore du même ſentiment, car la belle expérience de mylord Charles Cavendiſh, rapportée ici et perfectionée 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 colomnes du mercure, (juſqu'à y former comme des boutons de

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 tuyeau 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 ſouffre. Mais ſelon moi c'eſt tout ce qu'on en peut conclure. Voila comme je raiſonnerois, 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 lumiére 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 reſiſtance conſiderable dans les metaux et autres corps non électriques, comme je le prouverai clairement tout à l'heure. Le raiſonnement de Mr. Wilſon ſemble, par conſequent, être fondé ſur une hypothèſe gratuitement admiſé, ſavoir, que la matiére 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 hypotheſe eſt au moins auſſi pro, bable, 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 limitted 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, 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 phænomena, which attended the experiment, greatly favouring the doctrine here advanced. If when glaſs is electrified, and applyed 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 applyed 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. Francklin, 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 tuyeau de verre, ou d'un bâton de cire d'eſpagne. Dans moins d'un clein d'oeil non ſeulement le bout qu'on électriſe, devient électrique, mais auſſi le fil entier le ſera d'un bout à l'autre, et le ſera 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 cilindre de cire d'eſpagne ou de ſouffre, et qu'on le traite de la même manière. Le ſuccès en ſera tout à fait différent. Ce n'eſt pas le cilindre 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 améner les choſes, au point d'en rendre électrique une partie d'une longueur un peu conſiderable. Qu'on touche aprés la partie électriſée du tuyeau, et l'électricité ne ſera détruite, ni dans un inſtant, ni dans le tuyeau entier, comme dans l'expérience précedente. Au contraire, encore que l'endroit touché perde ſon électricité, il n'en ſera 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 metaux ou d'autres corps non électriques et ſe diſtribue en eux fort facilement et fort rapidiment, 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 regle 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 appellée d'un autre nom, *imperméabilité*. Au moins, je ne fais conſiſter l'imperméabilité en rien autre choſe, qu'en cela.

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

 eſt

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 troisieme jusqu'a la huitieme, 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 toûjours dans l'état contraire (c'est à dire, que son côté positif, est négativement electrique, et le côté négatif l'est positivement) quand un de ses côtés, quel qu'il soi,, est plus chaud que l'autre; et qu'elle ne retourne dans son état naturel, qu'après que la chaleur s'est distribuée uniformement en elle.* Par les expériences de Mr. Wilson au contraire, il faut établir une regle tout à fait différente, savoir: *Que la Toarmaline, 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 etat naturel, quand la chaleur s'est répandue uniformement.* Cette regle ne s'accorde en aucune façon avec celle que j'avois avancée.

Encore que je fusse tout à fait convaincû de la justesse de mes assertions, et de leur parfaite conformité avec l'expérience, je ne pouvois pourtant que fort difficilement me resoudre, à douter de l'exactitude des expériences faites par un aussi habile Observateur, que Mr. Wilson. J'ai donc mieux aimé supposer, qu'l y avoit, dans la manière, dont Mr. Wilson et moi avions procedé 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 toûjours posé la *Tourmaline*, ou sur un charbon ardent, ou sur une plaque de metal ou de verre échauffée, de façon, que l'un des côtés de la pierre, avoit toûjours toûché à quelque corps. Mr. Wilson au contraire, en échauffant la *Tourmaline*, y a procedé tellement que la pierre n'a jamais touché à quelque corps. Voilà une circonstance, qui semble assez importante, pour avoir pû 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 glaſs: 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. Wilſon, et j'ai pris garde, qu'elle ne toûchat à rien, mais pour le reſultat, je l'ai, non obſtant cela, *toûjours* trouvé conforme à ma regle, et *pas une ſeule fois* à celle de Mr. Wilſon.

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

 the

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 ſupect, 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.

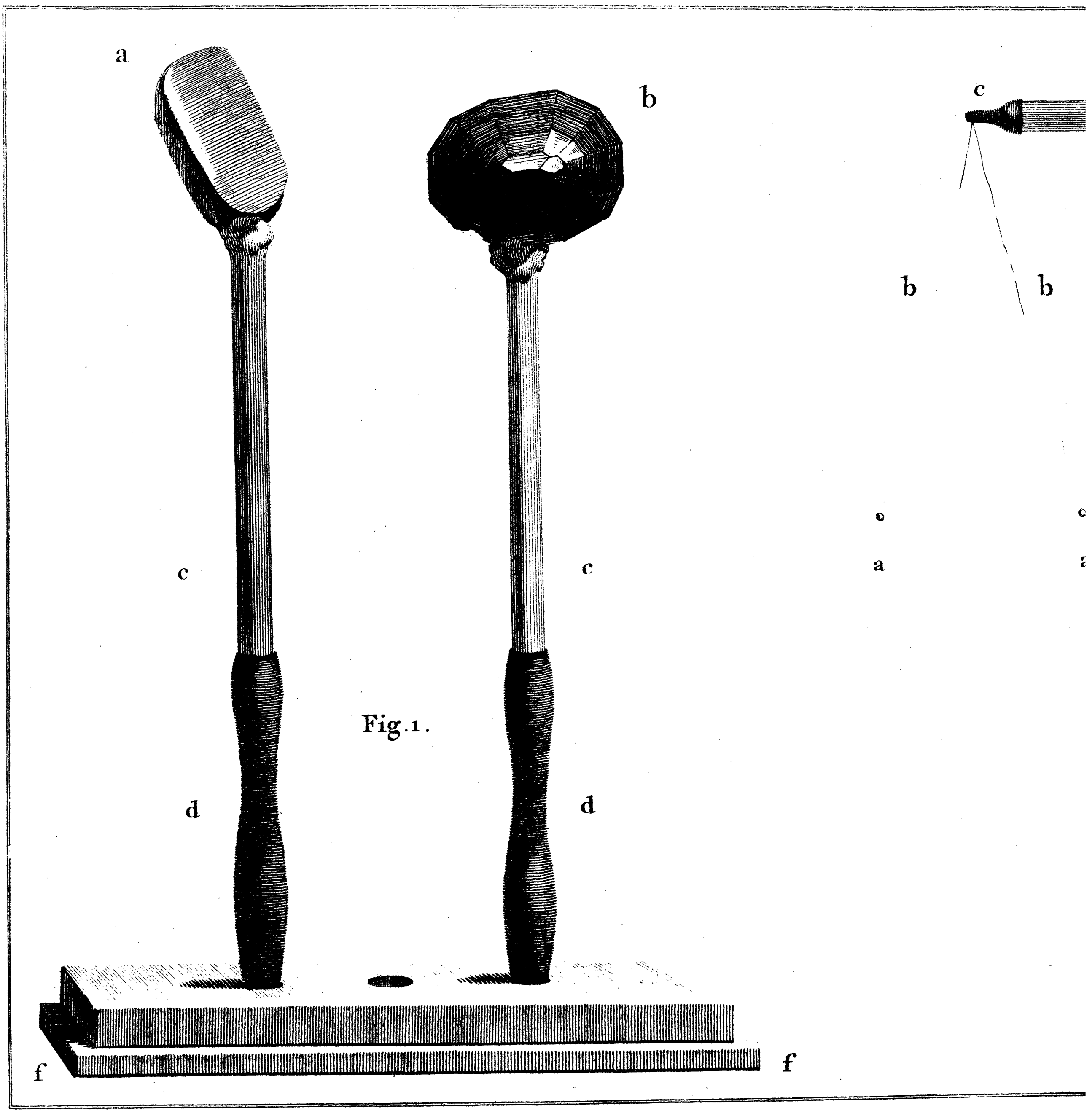
After the *convex ſide* of the *Tourmalin* (*b*, fig. 1. Tab. XXII.) has been held for a ſhort time, about one tenth of an inch, from the *flame* of a candle, *both ſides* thereof *are electrified plus:* and continue ſo for half a minute or more. And in a ſhort time after, the ſame *Tourmalin*, without 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.

In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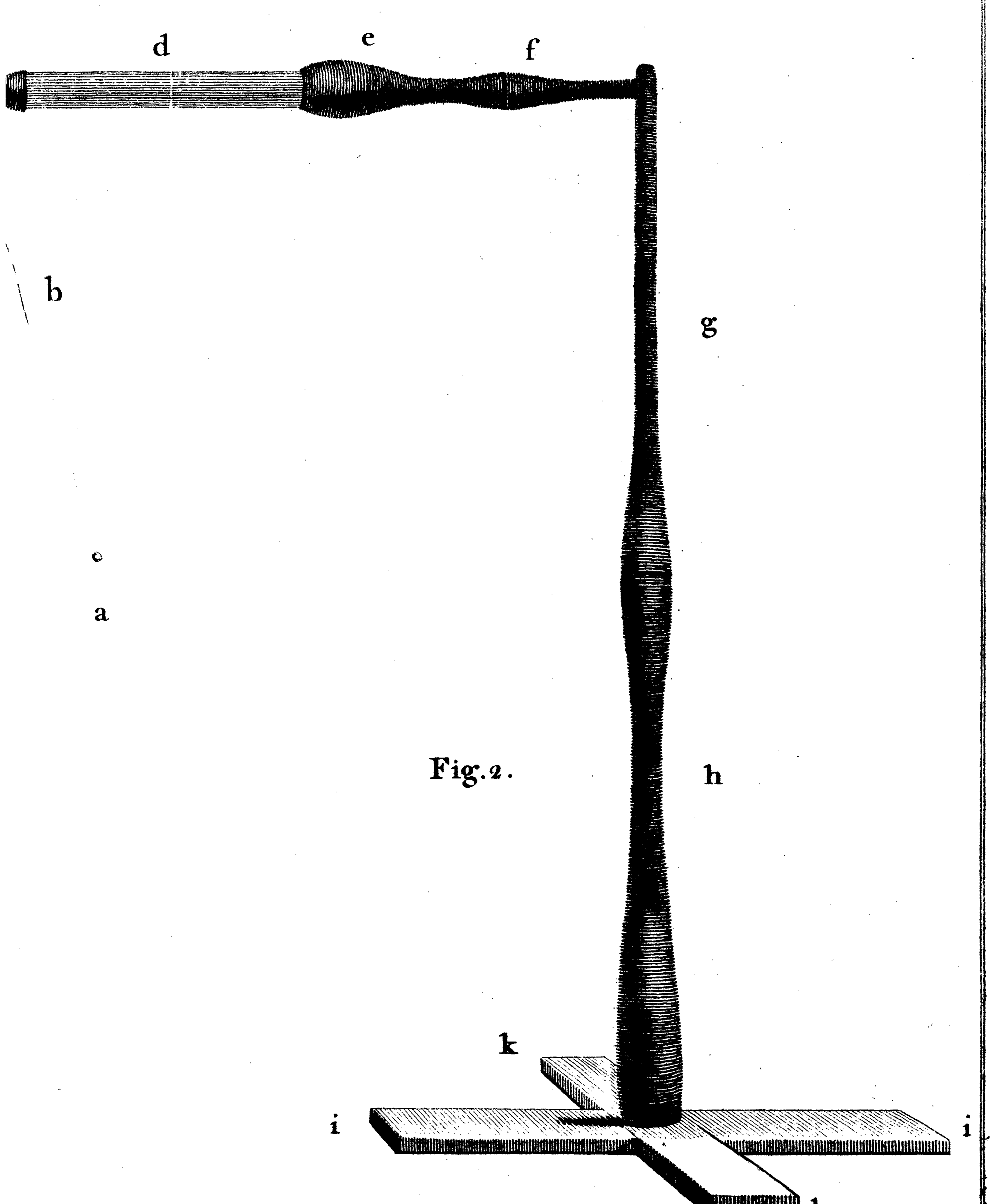
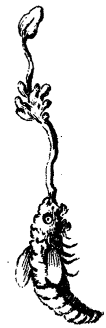


Fig. 2.



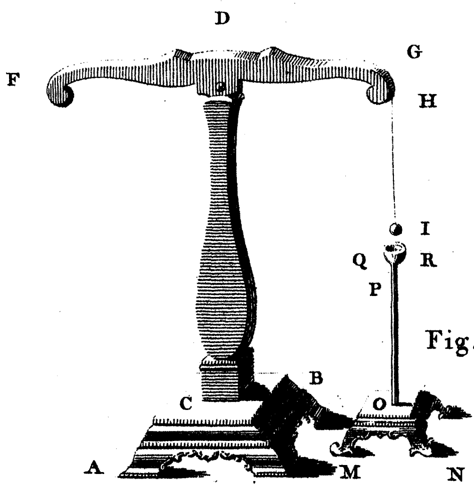


Fig. 4

Fig. 3.

Fig. 5

In regard to the other experiments with the *heated 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 *ballance* 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 prémier de ces inſtrumens eſt réprésenté 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 lévier à bras égaux F G. étoit un peu mobile. Chaque bras de ce lévier 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 lévier 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 ſoïe cruë, j'y attâchai par le bas un petit morceau de liège arrondi de la groſſeur à peu près d'une lintille. La longueur du fil H I. étoit de 5 à 6. pouces environ, et j'attâchai le pendule avec de de la cire à un des bras du lévier F G. Sur un autre petit pied quarré M N, qui eſt réprésente 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 hémiſphére O R, de deux lignes environ. Il eſt très-facile de faire ce tube en ſouffllant une petite boule de verre, comme on fait en faiſant un Thermométre, et en briſant enſuite cette boule jusque'à la moitié. Je me ſers dans les expériences de cet inſtrument pour y poſer la *Tourmaline*, afin qu'elle puiſſe agir librement.

no

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 extreamly 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'emploïe encore de petites pincettes A B C, 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. afinque 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'Electricité produite par la *Tourmaline*.

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, *Farh. 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 extreamly 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*.

EXPERIMENT III.

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.

EXPERIMENT IV.

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.

EXPERIMENT 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 existance 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 fogg, 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.

EXPERIMENT 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. *Brawn* 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.*

EXPERIMENT 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-

EXPERIMENT 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 electirc 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 yeild 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 Telleſcopes preſs upon each other with their own weight only, philoſophers know, by the phænomena 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 phænomena, 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 yeilding 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 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 tryal;) * 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

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 enterance 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

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 *ballance* 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 *ballance* 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

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 thereſore 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 *h* 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 mortaſs, 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.

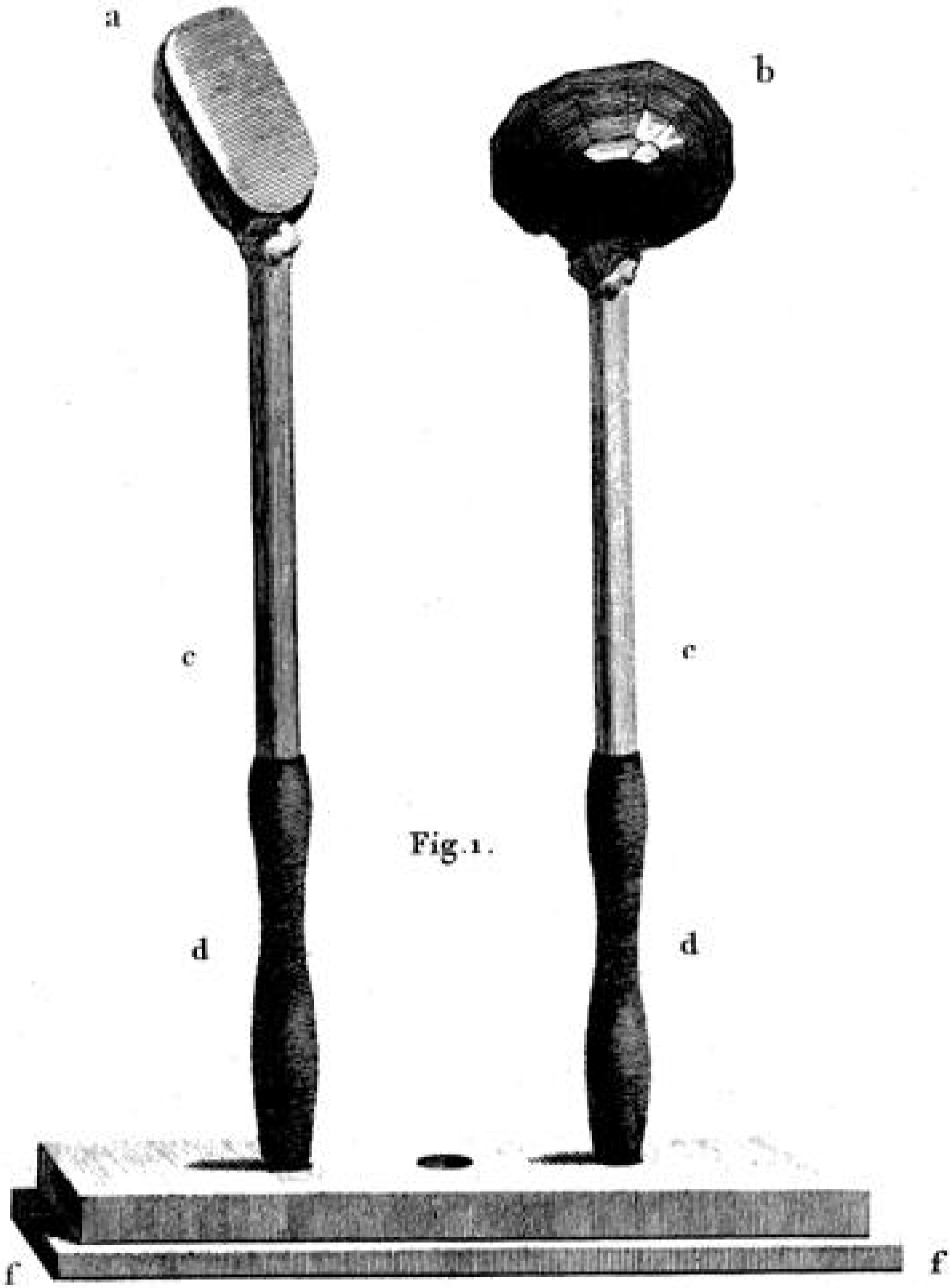


Fig. 1.

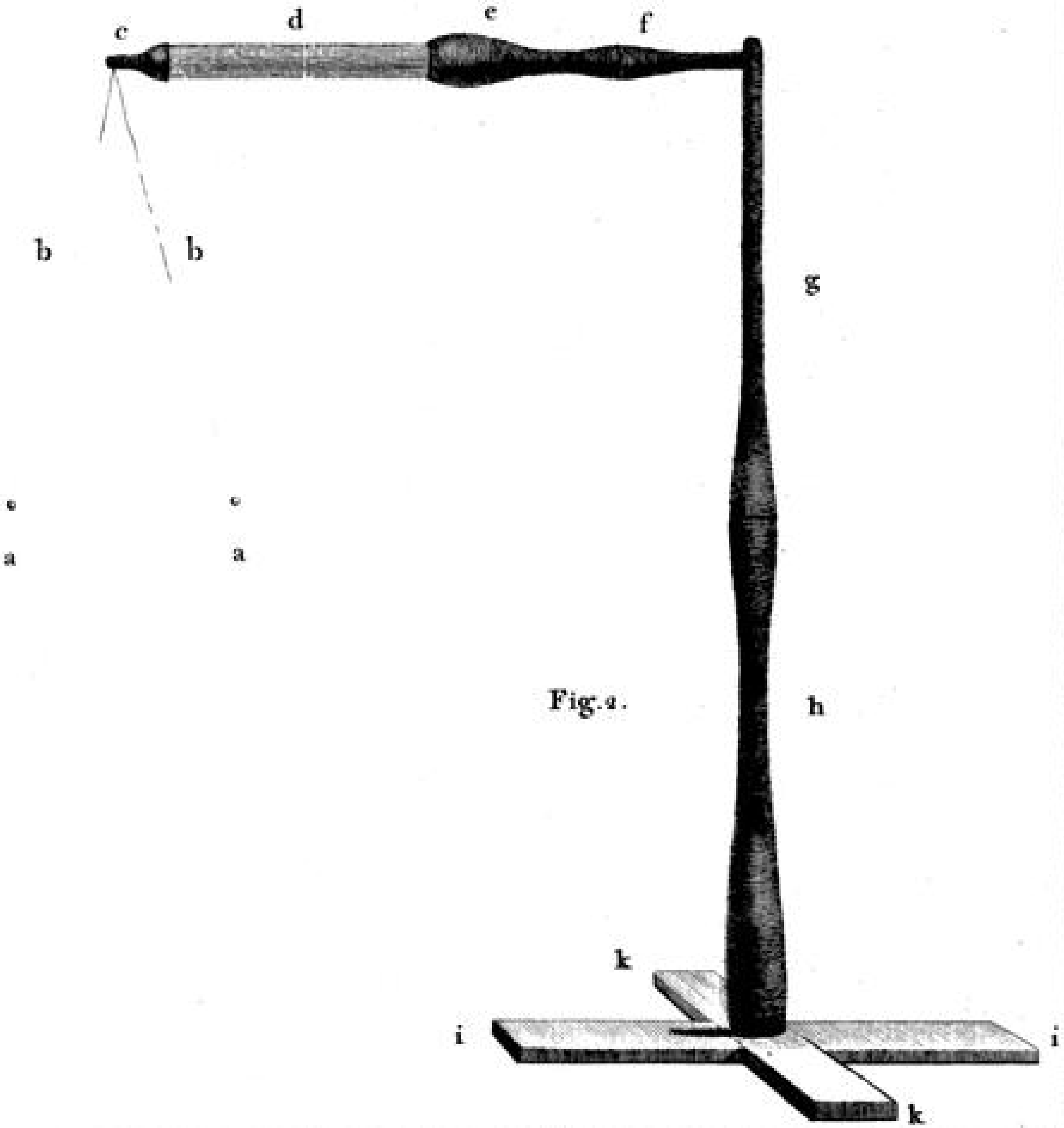


Fig. 2.