LV. *An Account of Dr.* Bohadſch's *Treatiſe, communicated to the* Royal Society, *intituled,* Diſſertatio philoſophico-medica de utilitate electriſationis in curandis morbis, *printed at* Prague 1751: *extracted and tranſlated from the* Latin *by Mr.* Wm. Watſon, *F. R. S.*

Read Jan. 23, 1752.

THE treatiſe, of which I now offer an extract to the Royal Society, was ſent hither from my friend and correſpondent Profeſſor Boſe at Wittemberg, who is always deſirous of teſtifying his zeal and attachment to the Royal Society, by communicating to us whatever he imagines worthy our notice. The author of this treatiſe, Dr. Bohadſch, is a Bohemian, a very learned and ingenious gentleman, who, while he was in England about two years ſince, was frequently at our meetings, and was very converſant with, and much eſteemed by, many of our body, from whom he received very great civilities. He was more particularly taken notice of by his Grace the late Duke of Richmond, whoſe loſs we yet lament: His Grace did me the honour to recommend him to me, as a gentleman not leſs remarkable for his great knowledge in various kinds of literature, than for his exemplary modeſty: and it is with great pleaſure that I lay before you what comes from the hands of one, for whom I have ſo great an eſteem.

This treatiſe, from its title, promiſes only an account of the advantages of electriſation in medicine: but this is not the whole of which it treats; it exhibits alſo a ſeries of obſervations of the effects of electricity upon both ſolid and fluid bodies, upon animals in a ſtate of health, as well as upon thoſe diſtemper'd. Of each of theſe I propoſe to lay before you ſome account in the courſe of this extract.

Our author firſt takes notice, that electricity, being continued for ſome hours, leſſens the weight of the body electrified. He exemplifies this firſt on fluid bodies; two equal portions of which, before electriſing, he accurately weighs; and then the difference between theſe two portions, one of which has been electriſed between four and five hours, and the other, though in the ſame room, not electriſed at all, is attributed to the operation of the electric effluvia. His globes, I obſerve, are rubbed by the hands of an aſſiſtant.

Four ounces of river water expoſed in a glaſs veſſel of four inches diameter were electriſed five hours, and loſt in their weight eight grains.

Four ounces of river water, in the ſame kind of glaſs, but not electriſed, loſt in the ſame time only three grains. The difference then to be attributed to the electricity was five grains. The like quantities of the fluids hereafter mentioned were expoſed, as the water was, and the effects were as follow.

	Grains
Oil of olives, by electriſing, loſt . .	o
Vinegar	ij.
Water impregnated with nitre . .	iij.
New milk	iv.

Urine

	Grains
Urine	vij.
Spirit of turpentine	vij.
Spirit of wine	viij.
Volatile ſpirit of ſal ammoniac	xi.

Four ounces of rain-water were expoſed in a tin veſſel of four inches in diameter, and electriſed as before, and the loſs was ten grains.

A like quantity of the ſame water under the ſame circumſtances, electriſing excepted, loſt only three grains. In this inſtance, the effect to be attributed to the electriſing was ſeven grains.

He then put to the trial, in a tin veſſel inſtead of a glaſs one, the ſeveral liquors before-mentioned; and except the oil of olives, the water impregnated with nitre, and the milk, the reſt loſt by electriſing a few grains more of their weight.

He afterwards expoſed three ounces and a half of river water in a glaſs veſſel, whoſe diameter was but an inch, and this loſt by a like electriſation only two grains. The ſame quantity of water, under the ſame circumſtances, electriſing excepted, loſt in the ſame time nothing of its weight: ſo that, in this inſtance, the effect to be attributed to the electricity was two grains. The various liquors before-mention'd were likewiſe electriſed in a veſſel of the like capacity as that containing the laſt water, and they loſt much leſs by the operation, than when they were expoſed under a larger ſurface. All theſe liquors, electriſed for the ſpace of ten hours, as well in veſſels of tin, as of glaſs well ſtopped, loſt nothing of their weight. From hence our author concludes, 1. That electricity

augments the natural evaporation of liquors, unleſs thoſe of a viſcous kind, as oil of olives, which from their tenacity loſe nothing of their weight. 2. That electricity increaſes the evaporation of liquors in proportion as they are more or leſs volatile: for volatile ſpirit of ſal ammoniac ſuffered a greater evaporation, than either ſpirit of wine or ſpirit of turpentine. Theſe laſt loſt more than water, and even this loſt more than the ſolution of nitre and the vinegar, as we ſee by the experiments. 3. That electricity operates moſt in thoſe veſſels, which are moſt permeable to its effluvia, *viz.* in veſſels of metal more than thoſe of glaſs. 5. That the effects of electriſing are not obſerved in veſſels cloſely ſtopped.

He afterwards put to the trial ſeveral ſubſtances of a more ſolid form. A pear weighing four ounces and a half, electriſed five hours, loſt of its weight 6 grains. A pear of the ſame kind, not electriſed, loſt nothing: ſo that the difference ariſing from electrification was 6 grains. He then ſubjected other ſubſtances to this trial, and the effects were as follow.

	Grains.
A piece of dry oak loſt	o
A bunch of keys	o
Two new-laid eggs	ij
A piece of new crum of bread	iij
——— raw beef	iij
——— ſalt beef	iv
——— ſponge lightly moiſten'd	vi
A bunch of grapes	vij

From

From theſe experiments our author obſerves, that the electricity diminiſhes the weight of ſolid bodies, if theſe are impregnated with humours liable to evaporate: for the dry wood, metals, and other bodies, which ſeem to have no fluids, loſe nothing of their weight; and therefore it is only upon the fluids in them that the electricity operates.

Our author then exhibits ſome experiments made by perſons of credit, in order to diſcover, whether or no electricity would accelerate the growth of plants; and from ſeveral trials found that it did. There then follows a ſeries of experiments, which prove, that electricity augments the tranſpiration of animals. Theſe experiments were made upon puppies, pigeons, yellowhammers, and chaffinches; and the effects of thoſe electriſed, compared with thoſe of the ſame kind, which were not, evince, that electricity does increaſe the tranſpiration of animals. Our author here has annexed ſeveral curious tables, comparing the loſs of weight of the animals, while electriſing, to what they loſe in the ſame time without electriſing. Whoever therefore is deſirous of peruſing them, muſt conſult the work itſelf.

Dr. Bohadſch proceeds to give us a theory of thoſe diſtempers, in which electricity ſeems to have the greateſt effects. He confines himſelf however more particularly to the *hemiplegia*; of which diſtemper he gives us the hiſtory, correſponding with what we find in the beſt medical writers. He likewiſe gives us the uſual method of cure, and ſhews, that the attempts of relieving this malady by electricity, nearly ſquare intentionally with the remedies moſt celebrated in practice. That the electrical ſparks

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and commotion produce the ſame effect, though in a more powerful manner, as warm ſulphureous baths, frictions, ſinapiſms, ſtinging with nettles, *&c.* generally made uſe of in the cure of this diſtemper. This reaſoning does very well in theory; but I ſhould have been glad to have ſeen it juſtified by practice, and his own obſervations. But inſtead of theſe, our author contents himſelf with giving us over again the lying ſtories of Pivati: to which he has added the four caſes publiſhed ſome time ſince, and tranſmitted to the Royal Society, as well as to myſelf, by Profeſſor Sauvages, of Montpellier. Theſe caſes indeed do credit to electricity, but we want more of them.

Our author finiſhes this diſſertation, by deducing ſeveral concluſions from what he has premiſed, and theſe are as follow.

I. That electricity may be advantageouſly applied to medicinal purpoſes.
II. That it augments the natural tranſpiration of animals.
III. That this acceleration of tranſpiration in men is through the exhaling capillary veſſels, and not through the ſubcutaneous glands.
IV. That the nervous fluid may be called the electrical fluid.
V. That the nerves ſubſervient to ſenſation are not different from thoſe ſubſervient to motion.
VI. That the immediate cauſe of the *hemiplegia* is the immeability of the nervous fluid through the nerves.
VII. That of all other diſtempers the *hemiplegia* ſeems moſt properly the object of electricity.

VIII.

VIII. That it may be of uſe alſo in intermitting fevers.

IX. That a palſy in the left ſide of the body is owing to the right ſide of the brain, and *vice verſa*.

X. That anger, the parent of numerous evils, is ſometimes uſeful to paralytics.

XI. That as long as the paralytic limbs are rigid, it it is an argument, that the burſal ligaments of the joints, and the ſheaths of the tendons, are deficient in the fluid, adapted by nature for their lubrication.

XII. That every ſpecies of palſy does not ariſe from the nerves being either obſtructed, or compreſſed.

In concluding this account, I cannot help obſerving, that, contrary to his uſual modeſty, our author has been guilty of a ſlight plagiariſm in this work; as, without quoting his author, he has tranſlated from the French into Latin the tables above-mention'd, as well as his experiments, proving that electricity forwards vegetation, from our worthy brother the Abbé Nollet's treatiſe, intitled, *Recherches ſur les cauſes particulieres dés phenomenons electriques.* See Nollet pag. 358 to 380. Dr. Bohadſch has only alter'd the date 1747 to 1750. But it is to be remember'd, that theſe accounts were calculated for the meridian of Prague, and not for thoſe of London and Paris.

loins. The *os ſacrum*, as well as the *os pubis*, imperfectly oſſified. All its joints are very rigid and ſtiff. It has no *anus*, but paſſes off its water in the natural way. Its *ſternum* is very imperfect; and it has no *clavicula*. It ſeems inſenſible of pain, not removing its arms or legs, if laid in an uneaſy poſture.

LXI. *An Account of the Phænomena of* Electricity in vacuo, *with ſome Obſervations thereupon, by Mr.* Wm. Watſon, *F. R. S.*

To the Royal Society.

Gentlemen,

Read Feb. 20, 1752. IN a paper I had the honour to lay before you in January 1747, which was the laſt I communicated to you of my own upon the ſubject of electricity, and which has been ſince publiſh'd in the *Philoſ. Tranſ.**, I acquainted you, that I intended upon ſome future occaſion to lay before you a ſeries of experiments in electricity made *in vacuo*; from a compariſon of which with thoſe already made in open air it did appear, that our atmoſphere, when dry, was the agent, by which, with the aſſiſtance of other electrics *per ſe*, we were enabled to accumulate electricity in and upon non-electrics; that is, to communicate to them a greater quantity of electricity than theſe bodies naturally have. That, upon the removal

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* Numb. 485, p. 320.

removal of the air, the electricity did pervade the *vacuum* to a conſiderable diſtance, and did manifeſt its effects upon any non-electric ſubſtances, which did terminate that *vacuum*; and that by theſe means, originally-electric bodies, even in their moſt perfect ſtate, put on the appearance of non-electrics, by becoming themſelves the conductors of electricity.

I had not ſo long delayed the illuſtration of theſe opinions by the experiments, which put me in poſſeſſion of them, but that I was not only diverted therefrom by very various avocations, but deſirous of giving them a ſtill greater degree of perfection, in order to place the above deductions beyond all controverſy. The executing the apparatus neceſſary hereto was not eaſily ſurmounted: I unſucceſsfully tried ſeveral artificers, who were not able to arrive at the nicety, which I thought neceſſary in the conſtruction of my inſtruments. Animated however by a late very honourable occaſion, and aſſiſted by Mr. Smeaton in the completing my apparatus, the event fully anſwered what I propoſed; although from the experiments I had made before the communication of the above accounts, I was fully convinced of their truth. I had other opinions indeed, which did ſtill require a further degree of demonſtration.

To make theſe experiments ſucceed, two things were more particularly required; firſt, that the inſide of the glaſſes made uſe of ſhould be perfectly dry; and therefore it was neceſſary, that their internal ſurface ſhould be expoſed to the wet leathers, uſually employed in pneumatic experiments, as little as might poſſibly be; otherwiſe, the vapours, ariſing therefrom

in exhauſting, defeated the intent by conducting the electricity, and thereby preventing its accumulation. Secondly, the more complete the *vacuum* was, *cæteris paribus*, the more conſiderable were the effects: and here I ſhould not do juſtice to real merit, were I ſilent in regard to Mr. Smeaton. This gentleman with a genius truly mechanical, which enables him to give to ſuch philoſophical inſtruments, as he executes, a degree of perfection, ſcarce to be found elſewhere; this gentleman, I ſay, has conſtructed an air-pump, by which we are impower'd to make Boyle's *vacuum*, much more perfect than heretofore. By a well-conducted experiment, which admits of no doubt as to its truth, I have ſeen by this pump the air rarefied to a thouſand times its natural ſtate; whereas commonly we ſeldom arrive at above one hundred and fifty. As the promotion of the mechanic arts is a conſiderable object of our excellent inſtitution, if this gentleman could be prevailed upon to communicate to the Royal Society that particular conſtruction of his air-pump, which enables it to execute ſo much more than thoſe commonly in uſe, it would not fail to be an acceptable preſent: but to return:

The experiments treated of in this paper muſt be conſidered to have been made in this *vacuum*. The electrical machine, with its prime conductor, need here no particular deſcription; but that of the glaſs, in which the *vacuum* was made, ſhould be more minutely conſidered. It conſiſted of a glaſs tube nearly three feet in length, and of almoſt three inches in diameter. A ring of braſs, exactly fitting this tube, was cemented to both its extremities, into each

of

of which was ſcrewed a hollow braſs cap, nearly of an hemiſpherical figure. Into the top of one of theſe caps was adapted a braſs box of oiled leathers, through which was admitted a ſlender braſs rod of a length ſufficient to reach within eight inches of the other extremity of the tube. Into the top of the other braſs cap was faſtened a braſs rod, like the former, only of eight inches in length. Thus the extremity of one of theſe braſs rods might at pleaſure, without letting in the air, be made to touch the other; and for the better obſerving what difference in effect would ariſe from an increaſe of ſurface, a ſmall braſs circular plate was made to ſcrew into each of theſe extremities. As the ſight of this inſtrument will convey to you at once a more clear idea than the moſt accurate deſcription, I take the liberty of laying it before you.

The intent of being able to bring the extremities of theſe rods near together, and to ſeparate them again to what diſtance you pleaſed, was, that it might without difficulty be determined, whether, and to what diſtance, the electrical fluid would manifeſt itſelf *in vacuo*, further than in air of the ſame denſity with the external.

The tube then thus fitted, and made dry both within and without, was placed in a cylinder of braſs, of about two inches long, and of a diameter juſt ſufficient to admit the braſs cap before-mentioned; and round the rim of this braſs cylinder, to prevent the ingreſs of air, was adapted a narrow piece of wet leather. Theſe being placed upon the plate of the air-pump, which ſtood upon cakes of wax, a piece of wire paſſed from the prime conductor to the long braſs

braſs rod, at the other extremity of the tube, and by theſe means, upon ſetting the electrical machine in motion, the long braſs rod in the tube was electrified. When the braſs plate at the bottom of this rod was placed near, or even at the diſtance of two inches from the plate of the other rod, the bruſhes of electrical fire were ſeen paſſing from the periphery of the upper plate to that of the lower, and every part of the air-pump ſnapped upon the touch of any one ſtanding upon the floor, and gave the other uſual ſigns of the accumulation of electricity. But, as theſe plates were made to recede from each other, *this effect grew leſs and leſs; ſo that, when they* were removed five or ſix inches from each other, no ſnaps could be drawn from the air-pump; as the diſſipation of the electric fluid was now as eaſy from every part of the prime conductor, as from the upper braſs plate in the tube: but it is to be noted, that this diſtance is different, as from the weather or other circumſtances the electricity is more or leſs ſtrong.

Upon exhauſting this tube, and electriſing as before the air-pump ſtill ſtanding upon cakes of wax, the electrical fire was not only ſeen to paſs from one plate to the other at the diſtance of 5 inches, but the ſame effect enſued at the greateſt diſtance, to which in the tube the braſs plates could be drawn. Being therefore deſirous to ſee a farther effect, and to avail myſelf of the whole length of this tube, I took from the inſide of it the ſhort braſs rod, to which the lower braſs plate was fixed, and faſten'd this plate at the very bottom of the tube into the cap. The conſequence was, that the electricity, meeting with ſcarce any reſiſtance, paſſed from the

top to the bottom of the tube, and electriſed the air-pump as before: and it was a moſt delightful ſpectacle, when the room was darkened, to ſee the electricity in its paſſage; to be able to obſerve, not, as in the open air, its bruſhes or pencils of rays an inch or two in length, but here the coruſcations were of the whole length of the tube between the plates; that is to ſay, thirty-two inches, and of a bright ſilver hue. Theſe did not immediately diverge as in the open air, but frequently, from a baſe apparently flat, divided themſelves into leſs and leſs ramifications, and reſembled very much the moſt lively coruſcations of the *aurora borealis.*

At other times, when the tube has been exhauſted in the moſt perfect manner, the electricity has been ſeen to paſs between the braſs plates in one continued ſtream of the ſame dimenſions throughout its whole length; and this, with a ſubſequent obſervation, ſeems to demonſtrate, that the cauſe of that very powerful repulſion of the particles of electrical fire one to the other, which we ſee in open air, is more owing to the reſiſtance of the air than to any natural tendency of the electricity itſelf; as we obſerve, that the bruſhes thereof from blunt bodies, when the electricity is ſtrong, diverge ſo much, as to form, when ſeen in the dark, an almoſt ſpherical figure. This figure ſeems therefore to ariſe from the electricity's endeavouring to inſinuate itſelf between the particles of air. The figure, that an elaſtic fluid of leſs denſity muſt form, when let looſe, and equably compreſſed by one more denſe and more elaſtic, muſt neceſſarily approach to that of a ſphere.

Upon

Upon admitting a very ſmall quantity of air into the tube, theſe phænomena diſappeared; not ſo much from the ſmall quantity of air admitted, as from the vapours, which inſinuated themſelves therewith. Theſe lined the ſides of the glaſs, and conducted the electricity imperceptibly from one end of the tube to the other. And to illuſtrate farther, that the vapours, and not the air, in the ſmall quantity admitted, occaſion'd this total diſappearing of theſe phænomena; upon experiment they have been viſible, though in a leſs perfect degree, when a much larger quantity of air was omitted to be exhauſted from the tube.

Theſe experiments ſeem, to evince, that however great the *vacuum* could be made, the electrical coruſcations would pervade it through its whole length.

From hence it appears, that our atmoſphere, when dry, is the agent, by which we are enabled to accumulate electricity upon non-electrics; as in the experiment before us, upon the removal of it, the electricity paſſed off into the floor through a *vacuum*, of the greateſt length we have hitherto been able to make, became viſible in this *vacuum*, and manifeſted itſelf by its effects upon the air-pump, being the non-electric ſubſtance, which terminated that *vacuum*: whereas, when the air is not taken away, the diſſipation of the electricity is from every part of the prime conductor. We ſee here alſo, contrary to what we have found hitherto, that an originally-electric body, *viz.* a dry glaſs tube, puts on the appearance of a non-electric, by becoming itſelf the conductor of electricity, that is, by its keeping out the air, and ſuffering the electricity to pervade the *vacuum*.

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How much ſoever the *vacuum*, here made uſe of, may exceed that, which is uſually arrived at, it is far from being a perfect one; and to make one more ſo, except that of Torricelli, which cannot without difficulty be applied to the preſent purpoſe, is not very eaſy. But it appears from the already mentioned experiments, as well as from a ſubſequent one, that the *vacuum*, which we are maſters of, does not tranſmit the electricity ſo perfectly as metals and water; as we are able to draw ſnaps from the prime conductor, an argument of ſome degree of accumulation, while the electricity is paſſing through the *vacuum*. This never happens, when metals, ſtanding upon the ground, touch the prime conductor. As we obſerve therefore, that the coruſcations diverge more or leſs, in proportion as there is more or leſs air left in the tube, this effect may ariſe even from the ſmall quantity of air ſtill remaining undiſcharged.

I was deſirous of knowing, for the farther illuſtration of my propoſitions, whether the experiment of Leyden could be made through the *vacuum*. For this purpoſe I made the before-mention'd exhauſted tube part of the circuit, ſo neceſſary to this experiment. What this circuit is, I have in my former communications ſo often and ſo clearly exemplified, that it would be needleſs to repeat it here. You know in this experiment it is likewiſe abſolutely neceſſary, that the whole quantity, or nearly ſo, of the accumulated electricity ſhould be diſcharged in the ſame inſtant of time. Accordingly, upon making the experiment, at the inſtant of the exploſion, you ſaw a maſs of very bright embodied fire jump from one of the braſs plates in the tube to the other: but this did not take place, when one of the plates was

farther diſtant from the other than ten inches. If the diſtance was greater, the fire then began to diverge, and loſe part of its force; and this force diminiſhed in proportion to its divergency, which was nearly as the diſtance of the two plates.

The difficulty however of applying the Torricellian *vacuum* to theſe experiments has been happily got over by the right honourable the Lord Charles Cavendiſh, our worthy Vice-preſident. This noble lord, who to a very complete knowlege of the ſciences joins that of the arts, and whoſe zeal for the promotion of true philoſophy is exceeded by none, has applied it in the following manner, and his lordſhip has had the goodneſs to put his apparatus into my hands. This apparatus conſiſted of a cylindrical glaſs tube of about three tenths of an inch in diameter, and of ſeven feet and half in length, bent ſomewhat like a parabola in ſuch a manner, that thirty inches of each of its extremities were nearly ſtraight, and parallel to each other, from which an arch ſprung, which was likewiſe of thirty inches *. This tube was careſully fill'd with mercury; and each of its extremities being put into its baſon of mercury, ſo much of the mercury ran out, until, as in common barometrical tubes, it was in equilibrio with the atmoſphere. Each of the baſons containing the mercury was of wood, and was ſupported by a cylindrical glaſs of about four inches in diameter, and ſix inches in length; and theſe glaſſes were faſten'd to the bottom of a ſquare wooden frame, ſo contrived, as that to its top was ſuſpended by ſilk lines the tube filled with

* See the repreſentation of this apparatus, Tab. XVI.

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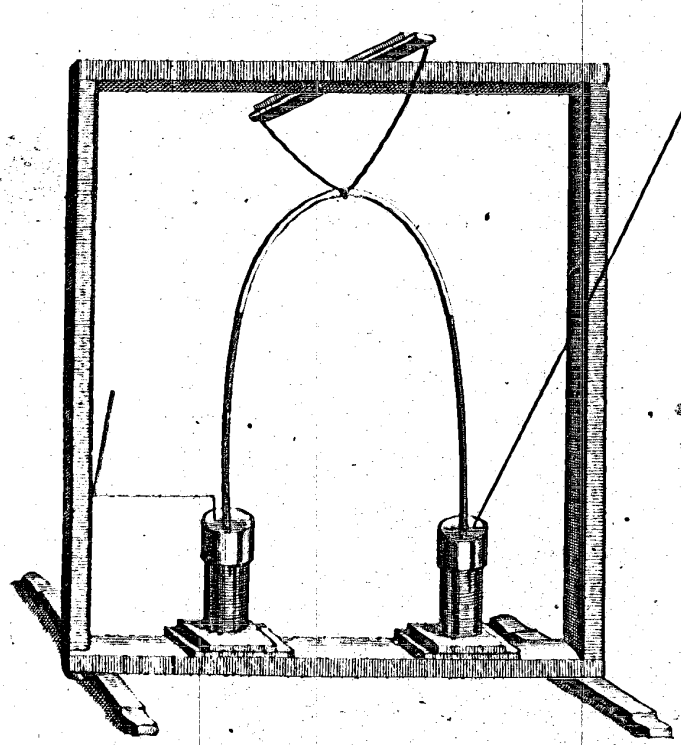
p. 370.



J. Mynde sc.

with mercury before-mention'd; ſo that the whole of this apparatus without inconvenience might be moved together. The Torricellian *vacuum* then occupied a ſpace of about thirty inches. In making the experiment, when the room was darkened, a wire from the prime conductor of the common electrical machine communicated with one of the baſons of mercury, and any non-electric touching the other baſon, while the machine was in motion, the electricity pervaded the *vacuum* in a continued arch of lambent flame, and as far as the eye could follow it, without the leaſt divergency.

That the electricity was not furniſhed from the glaſſes employed in theſe operations, nor from the circumambient air, I have heretofore, in my communications to you upon this ſubject, endeavoured to evince. I have ſhewn, that electricity is the effect of a very ſubtil and claſtic fluid, occupying all bodies in contact with the terraqueous globe; and that every-where, in its natural ſtate, it is of the ſame degree of denſity; and that glaſs and other bodies, which we denominate electrics *per ſe*, have the power, by certain known operations, of taking this fluid from one body, and conveying it to another, in a quantity ſufficient to be obvious to all our ſenſes: and that, under certain circumſtances, it was poſſible to render the electricity in ſome bodies more rare than it naturally is, and, by communicating this to other bodies, to give them an additional quantity, and make their electricity more denſe: and that theſe bodies will thus continue until their natural quantity is reſtored to each; that is, by thoſe, which have loſt part of theirs, acquiring what they have loſt;

and by thoſe, to which more has been communicated, parting with their additional quantity. Both one and the other of theſe is, from the elaſticity of the electric matter, attempted to be done from the neareſt non-electric; and when the air is moiſt, this is ſoon accompliſhed, by the circumambient vapours, which here may be conſidered as preventing in a very great degree our attempts to inſulate non-electric bodies. But theſe matters I have copiouſly treated of in my former communications upon this ſubject *; this ſhort recapitulation however I thought neceſſary, for the more eaſy illuſtrating what I propoſe to ſubjoin; and it is upon theſe principles that we are able to account for the circulation of electricity deſcribed in the *Philoſophical Tranſactions*, Vol. XLIV. p. 740.

If therefore the before-mention'd principles are true, and if the electricity is not furniſhed by the globe in its rotation, nor by the air, it ought to be viſible in the *vacuum* of the before-deſcribed glaſs tube, in its ingreſs to the frame of the electrifying machine, if this machine, and the man who turns the wheel thereof, are ſupported by electrics *per ſe*, and if, during this operation, the electricity, as faſt as furniſhed, is taken off by a byſtander, or otherwiſe, from the prime conductor; as under theſe circumſtances the *vacuum* is the only paſſage open to its progreſs, and from its elaſticity the electricity ſhould protrude itſelf through it. And from experiment this is the caſe; for, upon a piece of wire being connected with the end of the long braſs rod,

* See *Phil. Tranſ.* Vol. XLV. pag. 95 *& ſeq.*

rod, or with the braſs cap at the upper extremity of that tube, and the other end of the wire faſtened to any part of the frame of the electrifying machine, and this laſt put in motion, the electrical coruſcations are ſeen to paſs as before from one of the braſs plates contained in the tube to the other; and to continue, unleſs the air inſinuates itſelf, as long as the machine is in motion. If, under theſe circumſtances, the hand of a perſon ſtanding upon the floor is brought near the ſides of the glaſs, the coruſcations will direct themſelves that way in great variety of forms, extremely curious to behold. But here, as in the former experiment, our *vacuum* did not conduct ſo perfectly as metals or water; as a perſon, ſtanding upon the floor, and applying his finger to the upper braſs cap of the tube, receives a ſmart ſtroke: and this I conceive to ariſe, from the electricity of this braſs being ſo much more rarefied, or attenuated, than that of the body of the man, applying his finger.

This experiment ſhould be made in the middle of a large room, and the machine, and man turning it, ſhould be raiſed from the floor at leaſt a foot: otherwiſe the effects deſired will be diminiſhed by the electricity being in part furniſhed by the floor to the machine.

To what is here laid down it may be objected, that the electrical coruſcations in the laſt experiment proceed, not from the floor of the room, as I have conjectured, but from the electricity being, from the globe in motion, diffuſed at the ſame time upon the prime conductor, as well as all over the machine, and which in the tube becomes viſible in its paſſage

to the floor. But it is to be remember'd in this experiment, that no electricity is perceptible either *in vacuo*, or upon any part of the machine, as above-mentioned, unleſs at the ſame time the prime conductor is made uſe of; for, without that, there will be no diminution of the denſity of the electricity in the machine, as the quantity taken from the cuſhion by the globe in its rotation is returned upon it again the next revolution, the cuſhion being the firſt non-electric, which offers itſelf: but this I have have conſider'd at large, as may be ſeen in the *Philoſophical Tranſactions* *. This experiment therefore, in which the electricity is ſeen, without any preternatural force, puſhing itſelf on through the *vacuum* by its own elaſticity, in order to maintain the equilibrium in the machine, which had loſt part of its natural quantity of electricity by the preſent operation; this experiment, I ſay, I do not ſcruple to conſider as an *experimentum crucis* of the truth of the doctrines here laid down; to wit, not only that the electricity is furniſh'd by thoſe bodies, hitherto called non-electrics, and not by the electrics *per ſe* ‖; but

* Vol. XLV. p. 96.

‖ Since the communication of this paper to the Royal Society in February 1752, *viz.* in the ſucceeding ſummer, the truth of this doctrine is put out of all doubt by the diſcovery made in France, in conſequence of Mr. Franklin's hypotheſis, of being able, by a proper apparatus, to collect the electricity from the atmoſphere during a thunder-ſtorm, and to apply it to the uſual experiments, which demonſtrates, that the matter of thunder and lightning and that of electricity are one and the ſame. That the electricity did not proceed from the glaſs, or other electrics *per ſe*, as they had been uſually called, I firſt diſcover'd in the year 1746: ſee

but alſo, that we are able to add to, or take from, that quantity of electricity, naturally adherent to bodies.

By what denomination ſhall we call this extraordinary power? From its effects in theſe operations, ſhall we call it electricity? From its being a principle neither generated nor deſtroyed; from its being every-where and always preſent, and in readineſs to ſhew itſelf in its effects though latent and unobſerved, till by ſome proceſs it is produced into action, and rendered viſible; from its penetrating the denſeſt and hardeſt bodies, and its uniting itſelf to them; and from its immenſe velocity; ſhall we, with Theophraſtus, Boerhaave, Niewentyt, s'Graveſande, and other philoſophers, call it elementary fire? Or ſhall we, from its containing the ſubſtance of light and fire, and from the extreme ſmallneſs of its parts, as paſſing through moſt bodies we are acquainted with, denominate it, with Homberg and the chemiſts, the chemical ſulphureous principle, which, according to the doctrines of theſe gentlemen, is univerſally diſſeminated? We need not indeed be very ſollicitous in relation to its denomination: certain it is, that the power we are now treating about is, beſides others, poſſeſſed of the properties before mentioned, and cannot

See *Phil. Tranſ.* Vol. XLIV. p. 713.—749, and explained further Vol. XLV. p. 95, *et ſeq.*; and though the electric matter may be taken from the atmoſphere during a ſtorm of thunder, or even when it is only charged with what are uſually called thunder-clouds, that is, when the atmoſphere is replete with heterogeneous phlogiſtic matter, yet it muſt not be conſidered as coming from pure dry air, which, as I before mentioned, I conceive to contain in its natural ſtate ſcarce any of the electric matter, and is the agent, by which we are enabled to communicate electricity to other bodies.

A GENERAL BILL *of the* WEDDINGS, BIRTHS, *and* BURIALS; *and alſo of the Numbers, which* *of* London *(diſtinguiſhing which were Natives thereof) for the Year en*

Total Number of BURIALS.

Whereof have died of the following Ages and S

		One Month old, and under		From One to Three Months		Three Months to One Year		One Year to Two Years		Two to Three		Three to Four		Four to Five		Five to Ten		Ten to Fifteen		Fifteen to Twenty		Twenty to Thirty		Thirty to Forty	
		Natives	Others	Natives	Others	Natives	Others	Natives	Others	Natives	Others	Natives	Others	Natives	Others	Natives	Others	Natives	Others	Natives	Others	Natives	Others	Natives	Others
Abortive.	Males																								
	Females																								
Ague.	Males																								
	Females																								
Apoplexy, Palſy, Cramp, and ſuddenly.	Males																								
	Females																								
Aſthma and Phthiſic.	Males																								
	Females																								
Chin-cough, Whooping cough, and Worms.	Males																								
	Females																								
Child-bed, Miſcarriages, and Femin ne Diſorders.	Females																								
Colick, Gripes, Twiſting of the Bowels, Flux, Vomiting, Looſeneſs, Bloody Flux, and Bleeding.	Males																								
	Females																								
Conſumption, Jaundice, Diabetes, and Fal ing Sickneſs.	Males																								
	Females																								
Convulſions.	Males																								
	Female																								
Dropſy, Tympany, Stoppage in the Stomach, Riſing of the Lights, and Swelling.	Males																								
	Females																								
King's-Evil, Itch, Leproſy, Scurvy, St. Anony's Fire, Scald Head, Impoſtume, Sores, Ulcers, Cancer, Fiſtula, Gangrenes, and Mortifications.	Males																								
	Female																								
French Pox.	Males																								
	Females																								
Small-Pox and Purples. Inoculated.	Males																								
	Females																								
Small-Pox and Purples. Not ſo.	Males																								
	Females																								
Fevers, Calentures, Inflammations, Sore Throat, and Quinſy.	Males																								
	Females																								
Gout, Rheumatiſm, and Sciatica.	Males																								
	Females																								
Gravel, Strangury, and Stone.	Males																								
	Females																								
Rickets, and Infantine Diſorders in the Head, Liver, Groin, and Ruptures.	Males																								
	Females																								
Lethargy, Grief, Lunatic, Vapours, Megrims, Spleen, Head-ach, Blaſted, Planet-ſtruck, and Bed-ridden.	Males																								
	Females																								
Meaſles, Chicken-pox, Swine-pox, and Raſh.	Males																								
	Females																								
Pleuriſy.	Males																								
	Females																								
Teeth.	Males																								
	Females																								
Thruſh.	Males																								
	Females																								
Caſualties, Bruiſed, Burnt, Choaked, Drowned, Executed, Self-murdered, Murdered, Overlaid, Poiſoned, Scalded, Shot, Strangled, Smothered, Starved, and Suffocated.	Males																								
	Female																								
Totals of each Age.	Males																								
	Females																								

Total Number of BIRTHS ——

Of which { Males — ——
Females — ——

Total Number of WEDDINGS ——

Sickned of the Small-Pox { Inoculated
Not Inocu

FOUNDLINGS received ——

…RIALS; *and also of the Numbers, which have died of each Age, Sex, and Disease, within the City and Suburbs …hich were Natives thereof) for the Year ending* December *the 31st* 1750.

Total Number of BURIALS.

Whereof have died of the following Ages and Sex:

…Year to …Years	Two to Three.		Three to Four.		Four to Five.		Five to Ten		Ten to Fifteen		Fifteen to Twenty		Twenty to Thirty		Thirty to Forty		Forty to Forty five.		Forty five to Fifty.		Fifty to Sixty.		Sixty to Seventy		Seventy to Eighty		Eighty to Ninety		Ninety to One Hund.		Beyond One Hund.		Total of all Ages.	
Others.	Natives.	Others.	Natives.	Others.	Natives.	Others.	Natives.	Others.	Natives.	Others.	Natives.	Others.	Natives.	Others.	Natives.	Others.	Natives.	Others.	Natives.	Others.	Natives.	Others.	Natives.	Others.	Natives.	Others.	Natives.	Others.	Natives.	Others.	Natives.	Others.	Natives.	Others.	Natives.	Others.

Of the Natives of *London* have died of the following respective Annual Births; *viz.*

BIRTHS.

Years inclusive.

Beyond 1651
1651 to 1660
1661 to 1670
1671 to 1680
1681 to 1690
1691 to 1700
1701 to 1705
1706 to 1710
1711 to 1720
1721 to 1730
1731 to 1735
1736 to 1740
1741 to 1745
1746
1747
1748
1749
1750

Total —

Total Number of WEDDINGS —

Sickned of the Small-Pox { Inoculated
{ Not Inoculated

FOUNDLINGS received —

Total Number of BURIALS —

Of which { Males —
{ Females —

cannot but be of very great moment in the ſyſtem of the univerſe.

I am, Gentlemen, with all poſſible reſpect,

London, Feb. 12, 1752. Your moſt obedient humble ſervant,

W. Watſon.

LXII. *A Letter from Dr.* Bevis *to Dr.* De Caſtro, *F. R. S. containing Extracts of Father* Auguſtin Hallerſtein's *aſtronomical Obſervations made at* Pekin *in* 1744 *and* 1747.

Read March 5, 1752. I AM much obliged to you, Sir, for furthering F. Aug. Hallerſtein's letter to me. It informs me, that the inſtrument I wrote the deſcription and uſe of, was arrived ſafe at Pekin. According to that miſſionary's requeſt, I have carefully looked over the obſervations he ſent to Dr. Sanchez at Paris, to be communicated to the Royal Society through your hands. They are compariſons of all the planets with known fix'd ſtars taken in the Jeſuit's College at Pekin, in 1746 and 1747, with a well-adjuſted pendulum-clock, and a micrometer; and appear to me to have been done with judgment and accuracy

tric matter with that of lightning completely demonſtrated.

I was pleaſed to hear of the ſucceſs of my experiments in France, and that they there begin to erect points upon their buildings. We had before placed them upon our academy and ſtate-houſe ſpires.

XCVI. *A Letter of Mr.* W. Watſon, *F. R. S. to the* Royal Society, *concerning the electrical Experiments in* England *upon* Thunder-Clouds.

To the Royal Society.

Gentlemen,

Read Dec. 21, 1752. AFTER the communications, which we have received from ſeveral of our correſpondents in different parts of the continent, acquainting us with the ſucceſs of their experiments laſt ſummer, in endeavouring to extract the electricity from the atmoſphere during a thunder-ſtorm, in conſequence of Mr. Franklin's hypotheſis, it may be thought extraordinary, that no accounts have been yet laid before you, of our ſucceſs here from the ſame experiments. That no want of attention, therefore, may be attributed to thoſe here, who have been hitherto converſant in theſe inquiries, I thought proper to appriſe you, that, though ſeveral members of the Royal Society, as well as myſelf, did, upon the firſt advices from France, prepare and ſet up the neceſſary apparatus for this purpoſe, we were defeated in our expectations, from the uncommon coolneſs and dampneſs

dampneſs of the air here, during the whole ſummer. We had only at London one thunder-ſtorm; *viz.* on July 20; and then the thunder was accompanied with rain; ſo that, by wetting the apparatus, the electricity was diſſipated too ſoon to be perceived upon touching thoſe parts of the apparatus, which ſerved to conduct it. This, I ſay, in general prevented our verifying Mr. Franklin's hypotheſis: but our worthy brother Mr. Canton was more fortunate. I take the liberty, therefore, of laying before you an extract of a letter, which I received from that gentleman, dated from Spital-ſquare, July 21, 1752.

" I had yeſterday, about five in the afternoon, an " opportunity of trying Mr. Franklin's experiment " of extracting the electrical fire from the clouds; " and ſucceeded, by means of a tin tube, between " three and four feet in length, fixed to the top of " a glaſs one, of about eighteen inches. To the up- " per end of the tin tube, which was not ſo high " as a ſtack of chimnies on the ſame houſe, I faſtened " three needles with ſome wire; and to the lower " end was ſolder'd a tin cover to keep the rain from " the glaſs tube, which was ſet upright in a block " of wood. I attended this apparatus as ſoon after " the thunder began as poſſible, but did not find it " in the leaſt electrified, till between the third and " fourth clap; when applying my knuckle to the " edge of the cover, I felt and heard an electrical " ſpark; and approaching it a ſecond time, I re- " ceived the ſpark at the diſtance of about half an " inch, and ſaw it diſtinctly. This I repeated four " or five times in the ſpace of a minute; but the

" ſparks

" ſparks grew weaker and weaker; and in leſs than
" two minutes the tin tube did not appear to be
" electriſed at all. The rain continued during the
" thunder, but was conſiderably abated at the time
" of making the experiment." Thus far Mr. Canton.

Mr. Wilſon likewiſe of the Society, to whom we are much obliged for the trouble he has taken in theſe purſuits, had an opportunity of verifying Mr. Franklin's hypotheſis. He informed me, by a letter from near Chelmsford in Eſſex, dated Aug. 12, 1752. that, on that day about noon, he perceived ſeveral electrical ſnaps, during, or rather at the end of, a thunder-ſtorm, from no other apparatus than an iron curtain-rod, one end of which he put into the neck of a glaſs phial, and held this phial in his hand. To the other end of the iron he faſten'd three needles with ſome ſilk. This phial, ſupporting the rod, he held in one hand, and drew ſnaps from the rod with a finger of his other. This experiment was not made upon any eminence, but in the garden of a gentleman, at whoſe houſe he then was.

Dr. Bevis obſerved, at Mr. Cave's at St. John's gate, nearly the ſame phænomena as Mr. Canton, of which an account has been already laid before the public.

Trifling as the effects here mention'd are, when compared with thoſe, which we have received from Paris and Berlin, they are the only ones, that the laſt ſummer here has produced; and as they were made by perſons worthy of credit, they tend to eſtabliſh the authenticity of thoſe tranſmitted from our correſpondents.

I flatter myſelf, that this ſhort account of theſe matters will not be diſagreeable to you; and am, with the moſt profound reſpect,

Gentlemen,

Your moſt obedient humble ſervant,

Lincoln's-Inn-Fields, Dec. 20, 1732. W. Watſon.

XCVI. *Extract of a Letter from Mr.* Brown, *Apothecary, at* Salisbury, *to Mr.* Wm. Watſon, *F. R. S. concerning the Succeſs of* Inoculation *there.*

Read Dec. 21, 1752, and here printed with Additions:

I AM much obliged to you for the obſervations, which you were ſo kind as to ſend me; concerning the method of inoculating for the ſmall-pox, and the ſubſequent treatment of that diſtemper. This I ſhould not have deferred till now, but that I was deſirous of ſending you ſome account of our ſucceſs therein.

Since the receipt of your letter, inoculating has been very much practiſed here, and with great ſucceſs; of which the account I now ſend may be looked upon as pretty authentic. From the 13 of Auguſt, to the beginning of February, have been inoculated, in this city and neighbourhood, four hundred and twenty-two perſons. On five or ſix of theſe, to my knowlege, it had no effect; though on one the experiment was tried a ſecond time.

Of

Of this whole number four have died; one of which was a patient of mine, who, I am inclined to think, did not do juſtice to this method: but that is ſubmitted to better judgment; for the day, on which the operation was performed, the patient's blood had been heated violently by exerciſe, and ſuddenly chill'd again, by putting on clean linen, juſt before the operation was performed; which, I apprehend, is receiving the infection in an inflamed ſtate of blood: but with this I was not the leaſt acquainted, till about ſix hours before the patient's death.

AN

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TO THE

Forty-ſeventh VOLUME

OF THE

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TO THE

Forty-seventh VOLUME

OF THE

Philosophical Transactions,

For the YEARS 1751 and 1752.

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XXXII. *An Account of a Treatiſe, preſented to the* Royal Society, *intituled,* "Letters "concerning Electricity; in which the "lateſt Diſcoveries upon this Subject, and "the Conſequences which may be de-"duced from them, are examined; by "the Abbé *Nollet*, Member of the Royal "Academy of Sciences of *Paris*, Fellow "of the Royal Society, of the Inſtitute of "*Bologna*, &c." *extracted and tranſlated from the* French, *by Mr.* William Watſon, *F. R. S.*

Read May 17, 1753. THE work before us contains 262 pages in 12mo, excluſive of the preface, and four copper plates, repreſenting ſeventeen figures.

This treatiſe is the production of a great maſter upon the ſubject of electricity: he has already publiſhed two volumes expreſsly thereupon, beſides ſeveral memoirs among the works of the Royal Academy of Sciences at Paris. For ſeveral years he has done me the honour of being my correſpondent, and I have communicated ſeveral valuable papers from him to the Royal Society.

The diſcoveries made in the ſummer of the year 1752 will make it memorable in the hiſtory of electricity. Theſe have opened a new field to philoſo-

phers, and have given them room to hope, that what they have learned before in their muſeums, they may apply, with more propriety than they hitherto could have done, in illuſtrating the nature and effects of thunder; a phænomenon hitherto almoſt inacceſſible to their inquiries.

But to make the moſt certain advantage of theſe new diſcoveries, we ſhould confine ourſelves to facts; and if we do draw conſequences from them, they ſhould be immediate and neceſſary ones; for, whenever our diſcoveries ſeem to promiſe to be uſeful and important, we are apt to hope and expect great ſucceſs from them: we muſt therefore be careful to reſtrain our imagination, or we ſhall fall into error.

Theſe conſiderations have induced our author to examine with care, what may truly be concluded from the experiments propoſed by Mr. Franklin of Philadelphia, and ſince carried into execution in France, and elſewhere, in relation to the electricity of the clouds during a ſtorm; by weighing every circumſtance, and comparing the greatneſs of the effects, which have been had in view, with the more than apparent inſufficiency of the means, which have been employed to produce them. He thinks, he ſees clearly, that the conſidering the electriſation of pointed bodies as a proof of leſſening the matter of thunder, is abuſing a real diſcovery to flatter ourſelves with a vain hope; and it is chiefly to diſſipate this error, if it yet ſubſiſts, that determined our author to print, in the work before us, ſome reflexions, which he had made at firſt only for himſelf, and a few perſons, to whom he was deſirous of communicating his opinion.

Mr.

Mr. Franklin's treatiſe upon electricity contains a great many very curious experiments; but the deductions from them being different from thoſe, which the Abbé Nollet has given upon the ſame ſubjects, it might be imagined, if he were ſilent upon this head, that he had given up his former opinions. The honour, which the Royal Academy of Sciences has done them, in publiſhing them in their Memoirs, and the kind reception, which the public has given them, has obliged him to re-examine theſe opinions, and to undertake their defence; more eſpecially as he ſees, that he has powerful reaſons to ſupport them. This has alſo been a motive for the preſent publication, which our author is deſirous ſhould be conſidered, leſs as a criticiſm upon Mr. Franklin's doctrine, than as a defence of his own.

In ſome parts of theſe letters, our author mentions an electricity, which is very often, and perhaps always, in our atmoſphere, when there is no appearance of thunder. He ſpeaks of this, as if he only ſuſpected it, and, in a manner, as if it wanted confirmation. He was then unacquainted with ſome deciſive experiments made upon this ſubject by Dr. le Monnier *, at St. Germain-en-laye, and which have been juſt publiſhed. He now conſiders, as a thing certain, that electricity is a very common meteor, which may manifeſt itſelf, when the weather is moſt ſerene;

* In a memoir read to the Royal Academy of Sciences at Paris, Nov. 15, 1752.

ſerene; and that thunder is, ſtrictly ſpeaking, only one of its modifications, which renders it more ſenſible to us.

The Abbé Nollet's treatiſe contains nine letters; ſix of which are addreſſed to Mr. Franklin, one to Mademoiſelle Ardinghelli, who, when only ſixteen years old, tranſlated Dr. Hales's treatiſe of Hæmaſtatics into Italian, and added thereto ſome very ingenious remarks; one to Mr. Jallabert of Geneva, and one to Mr. Boze of Wittemberg: To theſe are added ſome experiments in electricity, made in ſupport of opinions, laid down in this work, in the preſence of Meſſieurs Bouguer, de Montigny, de Courtivron, d'Alembert, and le Roi, who were appointed by the Royal Academy of Sciences for that purpoſe.

In the firſt letter our author gives his correſpondent Mademoiſelle Ardinghelli an account of the diſcoveries in electricity in the year 1752; among which he takes particular notice of the experiment made on May 10, at Marly-la-ville, in conſequence of Mr. Franklin's hypotheſis; wherein pointed non-electrics, ſupported by electrics *per ſe*, gave manifeſt ſigns of electricity during a thunder-ſtorm. This experiment, in the letters to Mr. Collinſon, Mr. Franklin had propoſed, but, as far as may be judged, had not then carried into execution. The experiment of Marly-la-ville was ſoon after verified by Dr. le Monnier at St. Germain-en-laye, who found further, firſt, that the like effects were produced, whether the iron rods were pointed, or not; and that it was indifferent, whether their poſition was horizontal, or not. Secondly, that thunder electriſed not only iron, but alſo

wood,

wood, living bodies, and other electriſable ſubſtances. Thirdly, that it was not abſolutely neceſſary to place theſe bodies at the tops of buildings; and that it was ſufficient for them to be placed about four feet from the ground in an open ſituation, and at ſome diſtance from large buildings. Fourthly, that bodies electriſed in this manner produced the like phænomena with thoſe electriſed by glaſs after the uſual manner. It was afterwards diſcovered, that electriſable bodies, thus diſpoſed in open air, were ſometimes electriſed under thick clouds, but without thunder, lightning, or even without rain or hail.

The Abbé Nollet recommends, that theſe experiments ſhould be made with circumſpection, as he has been informed by letters from Florence and Bologna, that thoſe, who have made them there, have had their curioſity more than ſatisfied by the violent ſhocks, which they have ſuſtained, in drawing off the ſparks from an iron bar electriſed by thunder. One of theſe in particular ſays, that once, as he was endeavouring to faſten a ſmall chain, with a copper ball at one of its extremities, to a great chain, which communicated with the bar at the top of the building, in order to draw off the electrical ſparks by means of the oſcillations of this ball, there came a flaſh of lightning, which he did not ſee, but which affected the chain with a noiſe like wild-fire. At that inſtant, the electricity communicated itſelf to the chain of the copper-ball, and gave the obſerver ſo violent a commotion, that the ball fell out of his hands, and he was ſtruck backwards four or five paces. He never had been ſo much ſhocked by the experiment of Leyden.

From

From the experiment at Marly-la-ville, and thoſe which have been made ſince, have been drawn two conſequences; one, that the matter of thunder, and that of electricity, are one and the ſame: the other, that, by the means of pointed iron rods, one might, without its doing any harm, draw off all the fulminating matter from a ſtormy cloud. But our author has ſhewn, that bodies being pointed are not abſolutely neceſſary; and is deſirous, we ſhould not too haſtily believe, that miſchiefs ariſing from thunder may be averted by the apparatus propoſed. He thinks the means vaſtly too ſmall for the greatneſs of the cauſe.

Our author's firſt letter to Mr. Franklin is an introduction to the five ſubſequent ones.

The ſecond letter treats of the nature of the electric matter. In this its analogy with fire is conſidered and proved; and our author takes notice, that Mr. Franklin, he imagines, who has certainly made ſome important diſcoveries into the properties of electricity, cannot but be diſſatisfied with the editors of his work, for publiſhing, "that he exhibited to "our conſideration an inviſible ſubtil matter, diſſe-"minated throughout all nature, *&c.* which had hi-"therto eſcaped our obſervations." The latter part of which aſſertion is not ſtrictly true; as the conſidering the matter of fire, and that of electricity, to be one and the ſame, is a fundamental principle of what both the Abbé Nollet and myſelf formerly publiſhed upon this ſubject.

The third letter to Mr. Franklin contains ſeveral proofs, that glaſs is not impermeable to the electric matter.

matter. Some of the experiments of our author upon this ſubject I heretofore did myſelf the honour to lay before you; and they are in my opinion fully concluſive.

The fourth letter to Mr. Franklin relates to ſeveveral phænomena of the experiment of Leyden. In this letter it is examined, whether the effects of this experiment proceed from the glaſs phial, or from the non-electrics contained therein; and experiments are produced to prove, that the power of giving a ſhock in an electriſed phial of water, proceeds from the water in the phial, and not from the phial itſelf, as Mr. Franklin imagines. In this letter likewiſe is an examination of Mr. Franklin's opinion, that, in the charged phial, as much fire as is received by one of its ſurfaces is loſt by the other.

The fifth letter to Mr. Franklin is in relation to the power of pointed non-electric bodies drawing off and throwing off electrical fire, at a much greater diſtance than obtuſe bodies do of the ſame kind. Our author thinks, that Mr. Franklin has attributed more power to pointed bodies, than, upon experiment he finds to be true.

The ſixth letter to Mr. Franklin is upon the analogy of thunder with electricity. This is a fact at preſent ſo well eſtabliſhed, as to admit of no doubt. But our author cannot agree with Mr. Franklin in his opinion, " that thunder is at preſent in the power of " men, and that we are able to diſſipate it at our " pleaſure: that an iron rod (ſuch a one as Mr. " Franklin has directed, and ſuch a one as has been " made uſe of) is ſufficient to diſcharge of all its fire

" a

" a ſtormy cloud againſt which it is directed." For his part, he confeſſes, that he cannot believe it; firſt, becauſe he ſees too great a diſproportion between the effect and the cauſe: ſecondly, becauſe the principle, which is given us to ſupport this opinion, is not ſufficiently eſtabliſhed. He can hardly think, that the fulminating matter, contained in a cloud, capable of covering a great city, can be drawn off in a few minutes by a pointed bar, as thick as your finger. If even a number of theſe placed upon the tops of eminencies were only neceſſary to prevent the effects of thunder, would not the vanes and croſſes at the tops of our ſteeples have been ſufficient to procure us this advantage? Theſe buildings however, in all times, have not been exempted from the miſchiefs of thunder. He deſpairs of our weak efforts ever being able to diſarm the heavens.

Our author here gives us the repreſentation and deſcription of his apparatus for electriſing during the thunder: it differs in nothing eſſential from thoſe, which we uſed laſt year.

In this letter are likewiſe conſider'd the validity of Mr. Franklin's hypotheſis of electric and non-electric clouds; the former ariſing from the ſea, the latter from the land; their operation, upon their approaching one near the other; the difference, according to Mr. Franklin, between electrical and common fire; and ſeveral other parts of Mr. Franklin's doctrine.

The eighth letter is addreſſed to our worthy brother Profeſſor Jallabert of Geneva; and, among other curious particulars, inſerts part of a letter, which our author had received from Mr. Jallabert, giving an

account of an experiment, which Mr. Jallabert had ſome time ſince made at the water-works at Geneva. An account of this experiment was communicated by myſelf to the Society; and it has near relation to the experiment, which we made here in electriſing the river Thames ſix years ago. Mr. Jallabert conſults the Abbé Nollet in relation to the ſolution of the phænomena of this experiment; and the Abbé does me the honour now to give the ſame ſolution thereto, which I firſt gave to a ſimilar experiment of Dr. le Monnier's, and laid before the Society in January * 1746, and ſince applied upon other occaſions in illuſtrating the electrical circuit.

The ninth letter is addreſſed to Mr. Boſe, profeſſor of mathematics and philoſophy at Wittemberg; and is in anſwer to one of Mr. Boſe, in which this gentleman expreſſes himſelf ſurpriſed, that ſo many ages have paſſed, without it having been diſcovered, that thunder electriſes bodies; ſince it depends upon an experiment ſo ſimple, and which it is hardly poſſible to fail in, when you deſire to repeat it under proper circumſtances.

Upon this our author obſerves, that people in general only ſee the facts, and are ignorant of, or do not conſider, the means, by which philoſophers arrive at them; nor perceive the circumſtances, without which theſe phænomena could never have been made known to us; and that Mr. Boſe will ceaſe to be ſurpriſed, as he is ſo well verſed in theſe phænomena, when he reflects upon what our author offers.

To

* See *Phil. Tranſ.* Vol. XLIV. p. 388.

To make the experiment in queſtion, it is neceſſary that bodies ſhould be ſupported by glaſs, ſilk, or reſin, without touching any thing elſe communicating with what we now call non-electrics; without which, the ſigns of electricity, which are ſought for, cannot manifeſt themſelves.

To this experiment, therefore, a previous knowlege is required, of inſulating bodies to be electriſed; but where is the man who was acquainted with this fact thirty years ago? Before that period, it was not even gueſſed at by any one.

Since Mr. Gray diſcovered, that bodies muſt be inſulated, to communicate to them a perceptible electric virtue, to what purpoſe could we ſet up iron bars under a ſtormy cloud? This thought could not have happened, but to thoſe who had taken notice of the analogy between lightning and electricity, and upon whom this idea had made a ſtrong impreſſion. And no one could think ſeriouſly upon this analogy, but ſince the diſcovery of the experiment of Leyden, that is, ſince the year 1746. Before that time the electriſation of bodies by thunder could not have been perceived, but by an accident very difficult to meet, on account of the conditions requiſite.

Nevertheleſs it may be urged, that bodies, being really electriſed, have ſhewn themſelves in all ages *, as

* I formerly took notice, that the electrical attraction had been obſerved ſo early, as to be mentioned by Theophraſtus (ſee *Phil. Tranſ.* Vol. XLIV. p. 732); ſo its luminous appearance, though only conſidered as a meteor, is mentioned by Plutarch, in the life of Lyſander. Pliny, in the ſecond book of his natural hiſtory, chap.

as hiſtorians both ancient and modern have made formal mention thereof. But to this it may be replied, that it was not enough to know the fact, unleſs people were enough acquainted with it to take it for what it really was; that is, the electric virtue: for without that, obſervations of this kind could have very little weight with

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37, calls theſe appearances ſa's; and tells us, not only that they ſettled upon the maſts, and other parts of ſhips, but alſo upon mens heads: 'Exſiſtunt (ſays that hiſtorian) ſtellæ et in mari terriſque. 'Vidi nocturnis militum vigiliis inhærere pilis pro vallo fulgorem 'effigie ea: et antennis navigantium, aliiſque navium partibus, 'ceu vocali quodam ſono inſiſtunt, ut volucres ſedem ex ſede mu-'tantes: . . . . geminæ autem ſalutares, et proſperi curſus 'prænunciæ; quarum adventu, fugari diram illam ac minacem, 'appellatamque Helenam, ferunt. Et ob id Polluci et Caſtori id 'numen aſſignant, eoſque in mari deos invocant. Hominum quo-'que capiti veſpertinis horis, magno præſagio circumfulgent." But (adds he) all theſe things are 'incerta ratione, et in naturæ 'majeſtate abdita.'

Seneca, in his Natural Queſtions, chap. 1. takes notice of the ſame phænomenon. 'Gylippo (ſays he) Syracuſas petenti viſa eſt 'ſtella ſuper ipſam lancem conſtitiſſe. In Romanorum caſtris viſa 'ſunt ardere pila, ignibus ſcilicet in illa delapſis.'

Cæſar *de Bello Africano*, cap. 6. *edit. Amſtel.* 1686. We here find them attending a very violent ſtorm. 'Per id tempus fere 'Cæſaris exercitui res accidit incredibilis auditu; nempe Virgili-'arum ſigno confecto, circiter vigilia ſecunda noctis, nimbus cum 'ſaxea grandine ſubito eſt exortus ingens. . . . Eadem nocte 'V legionis pilorum cacumina ſua ſponte arſerunt.'

Livy, chap. 22. mentions two ſimilar facts: 'In Sicilia militi-'bus aliquot ſpicula, in Sardinia in muro circumeunti vigilias 'equiti, ſcipionem, quem in manu tenuerat, arſiſſe, et litora cre-'bris ignibus fulſiſſe.'

Theſe appearances are called by both French and Spaniards inhabiting the coaſts of the mediterranean, St. Helme or St. Telme's

 fires;

with any perſon engaged in the inquiry. At preſent, indeed, when we know, from the experiment of Marly-la-ville, that a ſtormy cloud is a great electric maſs, the action of which extends itſelf ſenſibly even to bodies, which are upon the ſurface of the earth, we muſt agree, by reflecting on them, that the lights, which have been ſeen upon the croſſes placed on the tops of ſeveral ſteeples, thoſe, which the Roman ſoldiers ſaid they had obſerved at the end of their pikes, and thoſe lambent flames, which appear upon the maſts of ſhips, which mariners call St. Helmo's fire, are ſo many electrical phænomena. But until the moment that this experiment was made, which open'd our eyes with regard to the poſſibility and nature of theſe marvellous effects, theſe appearances were regarded either as popular illuſions, or falſe prodigies, or even as luminous vapours, which might be ranged in the claſs of *phoſphori*. Moreover, as theſe were ſeen but ſeldom, if ever we had been tempted to attribute them to the influence of ſtormy clouds, we might have been diſſuaded therefrom, by conſidering the little agreement there is, between the rarity of

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fires; by the Italians, the fires of St. Peter and St. Nicholas, and are frequently taken notice of by the writers of voyages.

If ſome late accounts from France are to be depended upon, we are informed, that at Plauzet it has been obſerved for time immemorial; and M. Binon, the curé of the place, bears his teſtimony of the truth, that, for twenty-ſeven years, which he has reſided there in that capacity, in great ſtorms, accompanied with black clouds, and frequent lightnings, the three pointed extremities of the croſs of the ſteeple of that place appear ſurrounded with a body of flame; and that, when this phænomenon has been ſeen, the ſtorm was no longer to be dreaded, and calm weather returned ſoon after.

of theſe effects, and the frequency of the cauſes, which might produce them.

We ſee, therefore, how important it is to deſcribe exactly the phænomena we obſerve: otherwiſe, how long may it be, before we can deduce any real inſtruction from thoſe, which we have been informed of in a negligent and ſuperficial manner? We have heard all our lives of St. Helmo's fire, of thoſe which the antients call Caſtor and Pollux, and of the comazants of our mariners. But, from what we have had related to us, and from what we have read, who could have been prevailed upon to range them with electrical phænomena? We have heard them repreſented, as thin lambent ſhining lights, a kind of phoſphoreal vapour: but there is a paſſage in the memoirs of the Count de Forbin, quoted by our author, wherein mention is made of St. Helmo's fire; which if any one, well verſed in the phænomena of electricity, had carefully attended to and conſidered a few years ago, he might have prognoſticated ſucceſs to Mr. Franklin, when he propoſed his experiment upon thunder. "In the night (ſays the author of "thoſe memoirs) on a ſudden it became exceed-"ingly dark, and thunder'd and lightened moſt dread-"fully. As we were threatened with the ſhip's "being torn to pieces, I ordered the ſails to be taken "in: We ſaw, upon different parts of the ſhip, above "thirty St. Helmo's fires: Among the reſt, there was "one upon the top of the vane of the main-maſt, "which was more than a foot and half in height. "I ordered one of the ſailors to take it down: When "this man was on the top, he heard this fire; its "noiſe reſembled that of fired wet gunpowder: I

"ordered

" ordered him to lower the vane, and come down;
" but ſcarce had he taken it from its place, but
" the fire left it, and fixed itſelf upon the top of
" the mainmaſt, from which it was impoſſible to
" remove it; and continued there a conſiderable time,
" until it went out by little and little, *&c.*"

If all the authors, who have taken notice of St. Helmo's fire, had ſpoken of it as this juſt quoted, philoſophers might have reproached themſelves for its having been ſo long before they had a juſt idea thereof, and for their not having ſhewn the principle upon which it depended. But how few hiſtorians are there, who could have related this fact with circumſtances ſo proper to put us in a right train, as thoſe juſt mentioned?

" And here I cannot but obſerve, as I am con-
" vinced, that the matter of thunder and that of
" of electricity are one and the ſame, how vaſt an
" idea muſt the attending to the before-mentioned
" paſſage excite in the mind of perſons, accuſtomed
" to the phænomena of electricity? How immenſe
" a quantity of it muſt they conceive to have been at
" that time in the atmoſphere ſurrounding the ſhip,
" and within the verge of its action, to furniſh more
" than thirty St. Helmo's fires; the ſame, in fact,
" which we ſee at the ends of our conductors in
" electriſing, one of which was more than a foot and
" half in height? At this time, and under theſe cir-
" cumſtances, the maſts, yards, and every part of
" the ſhip, I conſider as conductors of electricity,
" between the, at this time electriſed, atmoſphere,
" and the ſea: and tho', being of a vegetable nature,
" and, if dry, even of the worſt kind for this pur-
" poſe,

" poſe, they conducted electricity much leſs perfectly
" than metal under the like circumſtances would
" have done, I doubt not, but that they were greatly
" inſtrumental in averting the danger, with which
" the ſhip was threatened.

" Upon theſe conſiderations, I do not ſcruple to
" recommend, as Mr. Franklin has done, communi-
" cations of metal between the ſpindles and iron-
" work at the tops of the maſts of ſhips, and the ſea,
" or, which will anſwer the ſame purpoſe, the bilge
" water in the well. This can be liable to little
" objection, as the doing it is neither difficult, nor
" expenſive; an iron wire, of the thickneſs of a gooſe-
" quill, conducting electricity more readily than any
" piece of timber, however large; and theſe maſts
" do it ſo much the worſe, as they are of a reſinous
" nature.

" From attending to theſe phænomena, we every
" day ſee more and more the perfect analogy (to
" compare great things with ſmall) between the
" highly electriſed glaſs jar, in the experiment of
" Leyden, and a cloud replete with the matter of
" thunder. But more of this poſſibly upon ſome
" future occaſion.

" Though the number and continuance of the St.
" Helmo's fires, in the paſſage before-mentioned,
" probably tended greatly to preſerve the ſhip from
" the deſtruction, with which it was then threatened,
" yet the cauſe may be too great, and come on too
" faſt, to be leſſened enough by theſe means to avert
" the miſchief. Thus in the account, publiſhed in
" the * *Philoſophical Tranſactions*, from Captain John
" Waddel,

* Vol. XLVI. p. 111.

" Waddel, his ſhip was almoſt beaten to pieces by " the thunder and lightning: although, as he expreſſes " himſelf, there were ſundry large comazants over " head, ſome of which ſettled on the ſpindles on " the topmaſt-heads, and burnt like very large " torches. When this account was written, theſe " phænomena were only conſidered as the preſages " or attendants of a ſtorm, and no ſort of inference " propoſed from them."

But to return to our author: His work cloſes with a ſeries of experiments, intended to demonſtrate the validity of the concluſions exhibited therein. Theſe merit the particular attention of thoſe converſant in theſe matters; but I muſt refer you here to the work itſelf, and only obſerve, that ſome of the experiments are made *in vacuo*, and are of the ſame kind with thoſe which I communicated to the Royal Society in February 1752; and which have been ſince publiſhed in the *Philoſophical Tranſactions* *.

Upon tho whole, I think the treatiſe before us a very valuable one, as it gives us the ſtill riper thoughts of an able writer upon a difficult, and, till very lately, an almoſt unknown, ſubject; of one, who, beſides his inquiries into this part of philoſophy, has a great compaſs in the knowlege of nature, and is therefore well qualified to inveſtigate her phænomena.

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* Vol. XLVII. p. 363, et ſeq.

Philosophical transactions of the Royal society of London : giving some accompt of the present undertakings, studies, and labours of the ingenious in many considerable parts of the world

PHILOSOPHICAL

TRANSACTIONS.

PART I.

VOL. XLVIII.

great diſtance, through the upper part of the atmoſphere, where the reſiſtance is leaſt?

LIV. *Extract of a Letter from Profeſſor* Boſe, *of* Wittemberg, *to the Right Honourable* George *Earl of* Macclesfield, *Pr. R. S. with Obſervations thereupon, by Mr.* Wm. Watſon, *F. R. S.*

Wittemberg, Auguſt 1, 1753.

Read Dec. 6, 1753. IN the beginning of Auguſt 1752, after great and continued rains, many of our rivers overflowed their banks, and covered the neighbouring grounds, more or leſs according to their level, to a conſiderable diſtance: and the quantity of water was ſo great, that in ſome places it was not diſcharged for more than a week. More particularly the river Unſtrut in the territory of the landgrave of Thuringue required a very great time to empty itſelf, not only as that river runs over a large tract of country, but alſo as between Artern and great Jena, where this river joins the Sale, its bed in ſeveral places is very much confined.

When the inundation was abated, it was obſerved from the little city Laucha quite up above Artern, not only upon the fields and meadows, but alſo upon the buſhes and trees, that there was a green and very tough viſcous ſlime, which by the help of a ſtick could be drawn out to two or three ells in length. The ſubſequent heat of the ſun dried this matter, and it appeared like wool upon the buſhes; but the fields, when

when ſeen at a diſtance, ſeemed as though they were covered with ſand. This matter had a ſmooth appearance outwards, but within was like a ſheep's ſkin. Downwards next the ground it had a ſort of wool; and when the whole was waſhed with ſoap, it whitened, and appeared like a clean fleece of white wool. Of this ſubſtance the country-people ſoon made wicks for their lamps, as ſeveral lined their cloaths with it, as they would with furs.

It was further obſerved, that where this ſubſtance was mowed off from the meadows, the graſs under it was quickly dried up; but, where it was not removed, the graſs in the following December was as green and freſh as in the ſpring. —— Thus far Mr. Boſe.

Obſervations.

The vegetable ſubſtance, which, upon the ſpecimen ſent over by the profeſſor to our truly Noble and Learned Preſident, he has intituled " a ſort, " perhaps, of *Alcyonium molle*," is a ſpecies of that genus of plants, which the more modern botaniſts call *byſſus*. And it is of that ſpecies, or a very ſlight variety therefrom, which is called, by the juſtly celebrated Dillenius *, in his *Hiſtoria Muſcorum*, *Byſſus tenerrima viridis velutum referens*. It is alſo mentioned and figured by *Micheli* † in his *Nova Plantarum Genera*, under the title of *Byſſus terreſtris viridis herbacea et molliſſima, filamentis ramoſis et non ramoſis*. This genus of plants, in the order of nature, comes between the moſſes and *fungi*.

* *Hiſtor. Muſcor.* p. 7.
† *Micheli Nov. Plant. Genera*, p. 211. Tab. LXXXIX. Fig. 5

fungi. The ſpecimen now ſent, being white on one ſide, ariſes from its either being waſhed or bleached by the ſun; for when wet, according to Mr. Boſe, it was green: and this colour is mentioned both by Dillenius and Micheli in their ſeveral denominations.

This vegetable is found in England, as well as in many parts of Europe, in moiſt meadows, covering the ground like a carpet, and ſometimes to a great extent.

We muſt be careful, however, how we connect the ſubſtance in queſtion, and others of the ſame genus with the βύσσος of the antient Greek writers, or the *byſſus* of the Latin. What that ſubſtance was, has been matter of great controverſy. Thus much is certain, that garments made of it were the apparel of the rich. And in the New Teſtament, St. Luke, in the parable of the rich man and Lazarus, ſays of the former, as a mark of his opulence, ἐνεδιδύσκετο πορφύραν καὶ βύσσον; this is tranſlated in our Engliſh verſion, "he was cloathed in purple and fine linen". It is more probable, that the *byſſus* of the ancients was a very fine ſort of cotton: but whoever has the curioſity of examining what has been ſaid upon this ſubject, may conſult Pliny * and Wormius †; but, above all, Bodæus à Stapel §, in his commentary upon Theophraſtus; who has, upon this occaſion, as well as upon a great variety of others, given us an ample teſtimony of his vaſt erudition.

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* *Plinii* lib. xix. c. 1.

† *Muſ.* p. 139.

§ Pag. 425, et ſeq.

PHILOSOPHICAL TRANSACTIONS.

I. *An Account of the Uſe of Furze in fencing the Banks of Rivers: In a Letter to the Reverend* Stephen Hales, *D. D. F. R. S. from the Reverend Mr.* David Wark.

Reverend Sir,

Read Jan. 8, 1761. I HAD occaſion to inform you before, that on obſerving a little ſand placed in the midſt of a river, where the ſtream was pretty rapid, I inquired into the cauſe, and found a furze buſh lodged there, which had detained the ſand, in ſpite of the current. It was eaſily concluded from hence, that furze might be profitably uſed in fencing the banks of rivers, at a very cheap rate, and thereby preventing many acres of rich ſoil from being changed into barren gravel. Several years after, I prevailed on ſome gentlemen of my acquaintance to try the experiment; which was ſo

LIII. *An Account of a Treatiſe in* French, *preſented to the Royal Society, intituled,* "Lettres ſur l'Electricité, *by the Abbé* "Nollet, *Member of the Royal Academy* "*of Sciences,* &c. &c." *By* William Watſon, *M. D. R. S. S.*

Gentlemen,

Read Dec. 17, 1761. ABOUT eight years ſince, the learned and ingenious author of the work before us publiſhed a treatiſe, of which the preſent work may be conſidered as a continuation. That conſiſted of nine letters upon the ſubject of electricity, which were addreſſed to perſons, who had diſtinguiſhed themſelves by their endeavours to illuſtrate this part of natural philoſophy. In like manner, the preſent performance conſiſts of eight letters, and is addreſſed, as the former, to his friends and correſpondents.

As an account of the former treatiſe was communicated by myſelf to the Royal Society, and printed, by direction of the council, in the Philoſophical Tranſactions *, the author requeſts, at the end of the ſixteenth letter, which is addreſſed to me, that I would give myſelf the additional trouble to lay before you an account of the preſent work. This requeſt I moſt readily comply with, not only in obedience to the order of the Society, but likewiſe as a teſtimony of

* Vide Vol. XLVIII. p. 201.

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the eſteem and regard, which I have long entertained, and ſhall continue to do, for the excellent author of it.

The principal deſign of the work before us, is to ſupport, and further confirm, the hypotheſis of the author, and of ſeveral other perſons, who have conſidered theſe matters, *that the effects of electricity depend upon the ſimultaneous affluence and effluence of the electric matter.* This treatiſe, like the former, is printed in 12mo. and contains 284 pages, excluſive of the preface, and four tables, exhibiting fourteen figures.

In defending his opinions, in relation to the effects of electricity, the Abbé Nollet has given a variety of new experiments, which cannot but be agreeable to thoſe, who are converſant in theſe matters. He has alſo occaſionally mentioned thoſe of other perſons, which are come to his knowledge, and which he apprehends not to be ſufficiently known. He has traced the origin of ſeveral happy inventions, and has exhibited to us the real authors of them. He has given, as he imagines, additional value to ſeveral experiments, which appear to him to have been too much neglected; and brought others, which have been over-rated, to their proper ſtandard.

As this work is of a controverſial kind, the author has had particular attention to ſuch points, as have been the occaſion of conteſt; to weigh the reaſons of his opponents, and to add new explanations to ſuch of his opinions, as ſeemed to want them; more particularly, to ſuch as have appeared to him to have been miſunderſtood.

The firſt of theſe letters is addreſſed to M. Necker, profeſſor of experimental philoſophy at Geneva. In

this letter, our author endeavours to eſtabliſh his opinion, publiſhed long ſince, in regard to the exiſtence of the ſimultaneous affluence and effluence, and conſequently the double current, of the electric matter, in oppoſite directions. And herein our author, by a ſeries of experiments, obviates ſome doubts, which had occurred to Mr. Necker, in relation to the validity of this hypotheſis.

The ſecond letter is addreſſed, as the former was, to M. Necker of Geneva. In this letter, the hypotheſis of M. Jallabert of Geneva, a very worthy member of this Society, in relation to the electrical phænomena, is examined; and ſuch part of it, as does not coincide with the ideas of our author, he endeavours to confute by an ingenious ſeries of deductions.

The third, fourth, and fifth Letters are addreſſed to M. Du Tour, of Riom in Auvergne, who has been a diligent enquirer into the nature and properties of electricity. In the firſt of theſe, is a careful examination of the validity of the doctrine of *plus* and *minus* in bodies electrified. So early as in February 1745, I communicated to the Royal Society an experiment, and ſome deductions therefrom, which laid the foundation of this doctrine. This experiment, and the deductions in conſequence of it, were afterwards printed in the Philoſophical Tranſactions *. Theſe I explained more at large, both by experiments and obſervations, in another paper, read to the Society in February 1745-6 †, and were the experiments, which ſo early cauſed me to conceive, that there was

* Vide Vol. XLIV. p. 739.

† See Phil. Tranſ. Vol. XLV. p. 93—101.

ſomething

ſomething in the phænomena of electricity, not to be reſolved, but upon ſtatical principles; and enabled me firſt to aſſert, that the phænomena in bodies electriſed, however ſimilar they might appear, did really ariſe *from their electricity being either greater or leſs than their natural quantity*. This doctrine has, ſince that time, been the cauſe of a vaſt variety of experiments, both here and abroad, by which great light has been thrown upon this part of natural philoſophy. How far our author has been able to overturn this doctrine, muſt be left to other judges to determine.

In the fourth letter, the doctrine of reſinous and vitreous electricity is examined. In this letter, as well as in the fifth, a great number both of experiments and deductions are produced, not only to weaken the doctrine of *plus* and *minus*, but to eſtabliſh the principle of *ſimultaneous affluence* and *effluence* of electric matter; as, if this principle is allowed, the doctrine of reſinous and vitreous electricity may be reduced to it: as our author is of opinion, that there is only one and the ſame kind of electricity, whether it is natural or artificial; and that, however appearances may make it ſeem to vary, the electricity is one and the ſame.

The ſixth letter is an anſwer to one of Father Beccaria, profeſſor of experimental philoſophy in the univerſity of Turin, publiſhed in Italian, in the year 1753, and addreſſed to the Abbé Nollet. This letter of Pere Beccaria was tranſlated into French, and publiſhed at Paris in 1754, by M. Delor, with many additions and annotations. It contains a very great number of curious experiments and obſervations,

both upon artificial and natural electricity; many of which are brought to prove the validity of the doctrine of our worthy member Dr. Franklin, in oppoſition to that of the Abbé Nollet. More particularly, he endeavours to confute the abbé's opinion, in relation to the affluence of the electric matter, which the abbé has, by experiments and obſervations, ingeniouſly endeavoured to confirm. Pere Beccaria's obſervations upon natural electricity, and upon meteors, on which he has made a prodigious number of experiments, many of them of a delicate nature, do him a great deal of honour.

The ſeventh letter, the ingenious author does me the honour to addreſs to me. In this letter, he, with juſtice, laments the calamities of war; more particularly, as it, in a great degree, prevents that correſpondence between men of letters, which contributes ſo much to their mutual ſatisfaction, and upon which the improvement of ſcience ſo much depends. The more particular purport of this letter, is to anſwer ſome objections, which Mr. David Colden, of North America, publiſhed againſt the former letters of our author. Theſe relate more particularly to the impermeability of glaſs to the electric fluid, and to the explanation of the phænomena of the experiment of Leyden. Beſides theſe, he gives us his idea of non-electriſed bodies electriſed *plus*, as he does not approve of the idea generally received of the *accumulation* of electricity. He mentions, that he has read Mr. Canton's memoir *relating to electricity, with his obſervations upon ſtormy clouds*. He finds many curious facts in that work; but thinks them not ſufficient to make the deductions Mr. Canton has done, in favour of the doctrine

doctrine of *plus* and *minus*. M. Du Tour of Riom, has ſent the Abbé Nollet a memoir, which he has likewiſe been ſo kind as to ſend me, containing a review of theſe experiments, from which he thinks it very eaſy to reſolve all theſe phænomena, upon the doctrine of ſimultaneous affluence and effluence of the electric matter.

The eighth letter is addreſſed to M. De Romas, aſſeſſor to the preſidial of Nerac, and contains remarks upon electrical kites; upon Father Ammerſin's manner of preparing and uſing wood to *inſulate* bodies, in making electrical experiments; and likewiſe ſome obſervations concerning the doctrine of ſimultaneous affluence and effluence of the electric matter. M. De Romas, in flying his electrical kite, was the firſt who uſed a cord compoſed of hemp and wire. This compounded cord conducted the electricity of the clouds far more perfectly than a hempen cord would do, even though it was wetted; and this cord being terminated by one of dry ſilk, enabled the obſerver, by a proper management of the apparatus, to make what experiments he thought proper, without danger to himſelf. The Abbé Nollet, however, deſires M. De Romas to be very cautious in making theſe experiments, and not too much to confide in his ſilk lines; as the vaſtneſs of the electrical matter in thunder-ſtorms may overcome the property of the ſilk, and even make it a conductor of electricity, and hazard the life of the obſerver. The quantity of electricity brought by M. De Romas's kite from the clouds has been ſo great, that, on the 26th of Auguſt 1756, " the ſtreams of " fire were an inch thick, and ten feet long, which " were conducted by the cord of the kite to the

" non-

" non-electric bodies near it, and the report of which " was equal to that of a piſtol." If a ſtroke of this kind had gone through the body of M. De Romas, probably the late unfortunate Profeſſor Richmann had no longer been the only martyr to electricity.

Father Ammerſin's method of preparing wood, ſo as to make it ſerve the purpoſe of glaſs, wax, &c. in electrical experiments, was publiſhed at Lucerne in the year 1754, and our author has given us an extract of it at the end of his work. This father found, that the frying of wood, after its being well dried in an oven, or otherwiſe, in either the oil of walnuts or that of linſeed, made it fit to *inſulate* thoſe bodies, which you choſe to electriſe, by preventing the diſſipation of the electricity: not only ſo, but what makes it ſtill more valuable to thoſe, who are engaged in theſe purſuits, you may excite electricity with it, as the Abbé Nollet ſays he has done, to his great convenience. He ſays further, that the end of a board mounted upon four pegs, a pair of wooden ſhoes, ſome truncheons of beech, walnut, or lime, &c. fried in oil, coſt him but little, and anſwered his purpoſe better than cakes of wax, pitch, roſin, and all the ſupports of glaſs or ſilk, which he had employed before: and, in caſe of neceſſity, a cylinder of this prepared wood, or a globe turned out of it, will excite an electricity ſo ſtrong, that you need not be at the trouble of exciting it with other bodies. Father Ammerſin himſelf employs common wooden meaſures, ſuch as are uſually found in granaries, firſt boiled in oil, and afterwards mounted ſo as to be turned by his wheel.

The

The Abbé Nollet, being deſirous of ſupporting the validity of ſome opinions of his, in relation to the nature and properties of electricity, deſired of the Royal Academy, that a committee ſhould be appointed, to examine the truth of ſome experiments, which the abbé conſidered as proofs of what he had eſtabliſhed. A committee was accordingly appointed, which conſiſted of Meſſieurs Deparcieux, Fougeroux, Bezout, Tillet, and Briſſon, who all atteſted to the academy, that the reſults of theſe experiments, at the making of which they were preſent, were ſuch as the abbé had foretold, in a memoir, which had been read to the academy; an atteſtation of which is given in this work, ſigned by M. De Fouchy, ſecretary to the academy, and is dated 10th April 1760.

Theſe experiments are ſixty in number, ſome of which are ſubdivided to more ſubordinate ones, and are moſt of them exceedingly well choſen. They tend to prove the ſimultaneous affluence and effluence of the electric matter, a doctrine long ſince eſpouſed, and very well ſupported by our author; but vehemently, and with much aſperity, controverted by ſome gentlemen at Paris. For a detail of theſe experiments, I muſt refer you to the work itſelf; and as they without doubt are very fairly ſtated, every perſon converſant in theſe enquiries will carefully conſider them, and, at the ſame time, reflect how far the hypotheſis is deducible from the phænomena.

I am, with the moſt profound reſpect,

Gentlemen,

Your moſt obedient

humble ſervant,

May 24, 1761. W. Watſon.

Philosophical transactions of the Royal society of London : giving some accompt of the present undertakings, studies, and labours of the ingenious in many considerable parts of the world

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