LX. A reply to Mr. Donovan's observations, &c. on Mr. De Luc's paper published in our number for February

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Fig. 9. Root common to all rich earth: the root being larger still.

Fig. X. The first sort of pump; the second sort of pump, fig. XI.

LX. *A Reply to Mr. DONOVAN'S Observations, &c. on Mr. DE LUC'S Paper published in our Number for February.*
By J. A. DE LUC, Esq. F.R.S. &c.

To Mr. Tilloch.

SIR,—IN your Journal for March, art. xxxvii. I find "Observations on a Paper by J. A. De Luc, Esq. containing some Remarks on Mr. Donovan's Reflections concerning the Inadequacy of electrical Hypotheses; by M. Donovan, Esq."

If philosophical controversies were always carried on with such a tranquillity and fairness as reigns in that between Mr. Donovan and myself, such discussions would better forward true natural philosophy than they commonly do. I hope it will be the case when I shall have explained myself respecting what appears a disagreement between us.

A first point relates to the object of *excitation*. I had not present to my memory all the parts of Franklin's theory with regard to that part of electric science, when finding it in Mr. Donovan's paper, and considering it as his own opinion, I made some objections against it; therefore he has reason to say *that it does not prove against him, but against Franklin*; in proof of which he quotes the Doctor's work, "Experiments and Observations on Electricity," published in London in 1749.

The most important point in our controversy concerns Volta's theory, of which I thought to give a proof by an experiment against which Mr. Donovan objects. The theory of that justly celebrated electrician is, that the *standard of plus and minus* in our observations is no *fixed point*, but *changeable*, being the *actual electric state of the ambient air*. My experiments to prove it, which I still consider as decisive, Mr. Donovan accurately describes, and I shall repeat them in his own words; in which, however, he uses an expression which has misled him, and which I shall point out afterwards.

"By the continual dispersion of electricity in a room (where an electric machine was worked, having a *point* fixed to the prime conductor), the *air* of the room was rendered *positive*; a pair of insulated pith-balls in the *natural state* was brought in from an adjoining room; they were diverged *negatively*; but when they returned to the room whence they came, the *natural state* was

was resumed. Mr. De Luc explains this by supposing, that although the balls were at first in the *natural state* when compared with the electric state of that room; yet, when brought into an *air* containing a greater absolute quantity of electricity, they became relatively *negative*. Now if this admits a different explanation, the necessity of the above inference is destroyed; it is therefore of importance to try if this can be effected. Now it appears to me, that the principle of *electric influence* may be applied in explanation of Mr. De Luc's experiments on electrified air. The balls in the *natural state* are brought into a *positive atmosphere*; the electricity of the latter *repels* the natural quantity of the balls into their *internal substance*; the *external parts* therefore are left *minus*. With this view the sequel exactly corresponds; for, when the balls were brought back into the *unelectrified room*, they collapsed into their *natural state*. But on the opposite view, the balls by immersion into a *positive atmosphere* would assume that state and retain it when removed, and therefore continue to *diverge*. Hence it appears to me, that the experiment is an anomaly on the hypothesis of Volta, and perfectly reconcileable to the principle of Franklin."

I think that some further explanations will remove Mr. Donovan's difficulties; and the most direct way will be by stating in what consists the difference between the two theories.—In Dr. Franklin's, the *standard of plus and minus* was a certain (supposed) *natural quantity of electricity* belonging to all the substances of our globe, which fixation as merely arbitrary, and opposed by facts, prevented that theory, though true in itself, from being admitted by a great number of natural philosophers both in England and on the continent; it was even by such strong arguments explained in my works, that they were never answered by retaining that theory.

But Volta's theory has produced two great and permanent changes in the science of electricity. He first proved that there is no fixed nor permanent *standard of plus and minus*; that the *standard* was *changeable*, being the *actual electric state of ambient air*. He next explained the cause of the *electric motions of pairs of balls*, by proving that *air* possesses the *electric fluid* as well as all the bodies which it embraces; but with this circumstance, that it abandons some to the bodies that have *less*, and takes some from those which have *more* than itself: lastly, that as the *electric fluid* has some *adhesion* to the bodies which possess it, if these bodies oppose less resistance to *move* than to part with the *excess*, or to receive their *defect*, they are transported by the *electric fluid* where it has a tendency to move.

I have demonstrated that effect by an analogous experiment made with a pair of small *soap disks* suspended by thin threads,

as

as a pair of *pith-balls*. When these *disks* were laid together on *water* they *diverged*, and the cause of that *divergence* was *visible*: both *disks* gave *soap* to the *water* between them, but only *one* to the external *water* on both sides; and the *disks* moving towards this, they *separated* from each other, or *diverged*.

Returning to *electric motions*, Volta's theory was submitted to direct verification by the experiment which Mr. Donovan examines. But I shall first remark, that by changing the expression which I had used in explaining these phenomena, he has not understood me, nor could he understand Volta's theory. He uses the expression *natural state*, to define the *electric state* of the *air* in the room next to that in which an electric machine was set in motion; whereas I had used the expression, the *actual electric state of the air in that room*, in which the *pair of balls* which were in the *same electric state* did not, nor could not *diverge*. If therefore Mr. Donovan in his reasoning changes the expression *natural state*, into that of *actual and local electric state of the ambient air*, he will find that his objection is not applicable to the explanation of the phenomena which I have observed.

Mr. Donovan makes this objection, "that the *pair of balls* brought from the next room, when coming in a *positive atmosphere* should be rendered *positive*, and continue to *diverge so*, when brought back to the first room." This was not the case, however. Therefore Mr. Donovan's objection is against a fact. But this would certainly have happened, had that pair of balls remained long enough in the room of the electric machine; for the *air* communicating at least its own *electric state*, they would then cease to *diverge*; but in that case, when brought back to the first room, instead of collapsing, they would have diverged as *positive*; having thus changed their *electric state*. Now Volta's theory explains all these cases: it is founded on this property of *air*, that it is a *non-conductor*, and cannot communicate the *electric fluid* to the bodies which it surrounds, or take some from them but in *absolute contact*; and from this circumstance proceeds the insulation of electrified bodies in *air*. However, by the continuance of the *contact* of the particles of *air*, these bodies are by degrees reduced gradually to its *electric state*.

With respect to *electric influences*, no true conclusion can be derived from experiments on that object when the *air* is not *dry*. For *moisture* is produced in the *air* by *aqueous vapour*, a *conducting fluid*; wherefore, in all my electric experiments, I had in the same room the *hygrometer* of my construction, and I made them at times when that *hygrometer* stood at about the same degree, as I have stated it in my paper in Nicholson's Philosophical Journal. Mr. Donovan had not this test in his experi-

ments

ments with an *excited glass tube*, which he brought under a pith-ball, and when removing the *tube*, the ball remained *positive*. This effect was probably owing to the *aqueous vapour* being then abundant in the room, and thus producing a *conducting medium*.

Coming now to the great object of electricity, namely, the *divergence* of the *electrified ball*, which is our *electrometer*; Mr. D. declares with ingenuousness that he has not had opportunity to see Volta's system completely stated; but he says, if I understand, it relates to two bodies, one of which is in a *natural state*, and the other in a *super-natural state*. But this is a misconception: Volta's theory relates to two bodies, *both* either *positive* or *negative*; and this *divergence* I have thus explained from Volta's theory, as in a pair of balls thus situated, *both* act to bring the *air between them* to their *electric state*; while *one* only acts on the *external air on both sides*, to which therefore they tend decidedly.

Mr. Donovan objects against that explanation, "that it virtually destroys the principle of *repulsion*, and refers all to *attraction*." This is true in the common acceptation of the words *attraction* and *repulsion*; but in strict natural philosophy, they ought to be called *tendencies*, as a *visible effect*, not implying the idea of *cause*, of which we might remain ignorant. But Mr. Donovan asks, "How can it be supposed at the same time, that *electricity* is an *elastic fluid*?" In Volta's theory *electricity* is only considered as an *elastic fluid*, in the upper region of the atmosphere; for, when it is produced in a manner which I have explained, it darts in a straight line and soon vanishes, as seen in *lightning*. But within the atmosphere it is a parasite fluid, always fixed to the particles of *air*; and I have proved, by an experiment related in my work *Idées sur la Météorologie*, that a *perfect vacuum* free from *aqueous vapour* is not a *conductor*. This experiment I made in presence of Dr. Franklin and some other experimental philosophers, who, knowing my opinion, desired to submit it to a test which Dr. Franklin himself prescribed. I performed the experiment, by which it was completely proved that a *perfect vacuum* did not transmit the *electric fluid*. Thus, after having doubted, they were fully convinced of this fact, that the *electric fluid* is really a *parasite fluid*, always attached to the particles of some *atmospheric fluid* and moving with them.

There remains another object of disagreement between Mr. Donovan and myself, concerning the *impermeability* of *glass* to the *electric matter*, which in my works on *Electricity* I had maintained as the real cause of the phenomena of the *Leyden vial*. On this object Mr. Donovan refers me to an interesting experiment which he has made, and thus describes: "A thin
flask

flask of glass with a neck many inches long was half-filled with mercury, and coated on the outside to the same height with foil. By means of a movable wire, an electric charge was thrown in; the wire was drawn out, and the neck was *hermetically sealed*. After a length of time the sealing of the neck was cut off; the wire was plunged into the mercury, but not the smallest *commotion* was perceived by the hand; although the original charge was capable of giving a *violent shock*. Thus I had encompassed a quantity of electricity on all sides by *glass*; after a *certain time* I found that the *glass* contained none. What should I conclude, but that it *escaped*? Were Mr. De Luc to repeat this experiment, he would certainly consider my inference as natural."

I am too old and too infirm to undertake any new experiments; I shall therefore only suggest an idea which Mr. Donovan may easily try. *Glass* becomes a *conductor* when *heated*; thus it may be that the *flask* was *discharged*, by the operation of *sealing it*; for the operation of *hermetically* sealing it, requires a great *heat*. I therefore suspect that the moment when the *flask* was *sealed*, it was *discharged*. This appears to me more natural than to suppose, against all the known phenomena of the *Leyden vial*, that *glass* is *permeable* to the *electric matter*. But Mr. Donovan might easily put it to the test of experiment in the manner above pointed out.

I am, sir,

Your most obedient servant,

J. A. DE LUC.

LXI. *Observations on the Priority of Mr. SMITH's Investigations of the Strata of England; on the very unhandsome Conduct of certain Persons in detracting from his Merit therein; and the Endeavours of others to supplant him in the Sale of his Maps;—with a Reply to Mr. W. H. GILBY's Letter in the last Number. By Mr. JOHN FAREY, Sen.*

To Mr. Tilloch.

SIR,—THE rash and unfounded reflections, which a Mr. W. H. Gilby has been induced to address to you from Edinburgh, pages 300 and 301 of your last Number, would not at this time have occasioned me to trouble you thereon, but for the very superior calls of *justice* as well as friendship, in favour of Mr. William Smith's irresistible claim to the discovery of the order, and to priority in the actual tracing and mapping of the surfaces of the principal part of the *British series of Strata*, and those scarcely less urgent calls, to support the cause of practical Eng-
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