XXXVII. Observations on a paper by G. A. De Luc, Esq. containing some remarks on Mr. Donovan's Reflections concerning the inadequacy of electrical hypotheses

M. Donovan Esq.

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be smaller. It consists only of six tiers or ranges of stone; the pyramid itself being 150 feet in height. The ranges of steps are twenty-five feet high and eleven feet wide. The rest of these structures are so fully and accurately described by Pococke, that little will be added here to his description of them. There is one built also with steps, which he believed to be as large as the principal pyramid at Djiza. The works at Saccára, independently of the different forms which characterize them, do all appear to be older than those of Djiza; the buildings being more decayed, and the stones crumbling, as if decomposed by longer exposure to the action of the atmosphere. Four miles to the south of Saccára stands a pyramid built of unburned bricks. This is in a very mouldering state. The bricks contain shells, gravel, and *chopped straw*: they are of the same nature as the unburned bricks in modern use in Egypt. Pococke concluded, from its present appearance, that it was built with five gradations only. It is of the same height as the other graduated pyramid of six degrees*.

[To be continued.]

XXXVII. *Observations on a Paper by G. A. DE LUC, Esq. containing some Remarks on Mr. DONOVAN's Reflections concerning the Inadequacy of electrical Hypotheses. By M. DONOVAN, Esq.*

To Mr. Tilloch.

SIR,—IN your last are contained some Observations on my “Reflections on the Inadequacy of the principal Hypotheses to explain the Phenomena of Electricity,” by Mr. De Luc. Any thing from the pen of that venerable and distinguished philosopher is deserving of the highest consideration: you will therefore allow a place to a few observations in reply.

It may in the first place be necessary to apprise Mr. De Luc of a misconception into which he has been led, no doubt, by some defect in the statement of my objections to the Franklinian hypothesis.

The hypothesis of electric excitation, in which it is supposed that, during the attrition of glass, the pores by opening receive electricity, and expel it when they close, is attributed by Mr. De Luc to me. To this, however, I have no title; it is a state-

* The author afterwards examined the catacombs here and at Alexandria; and he has furnished abundant evidence, from the position of the *Soroi* found in the latter, and from the height of the space in which the mummies were deposited, that in these cemeteries they were invariably placed in a horizontal position—not upright as maintained by some authors.

ment taken from Franklin, and will be found in his "Experiments and Observations on Electricity, London 1769," p. 78. The experiment detailed by Mr. De Luc, which, as he observes, "is a peremptory demonstration that the effect of friction is not to open the pores for receiving more electric matter, which is discharged when the friction ceases, as Mr. Donovan conceives it," therefore, does not prove against me, but against Franklin.

Mr. De Luc declares that "an error of Franklin has created all the just objections of Mr. Donovan." They are then admitted to be well founded, and my object is accomplished. It was Franklin, and not Volta, whose opinions I combated; and therefore Mr. De Luc's observations all relate to the possibility of applying to Volta what was intended for Franklin; so that, between the opinions of Mr. De Luc and those which I have ventured to bring forward, there is as yet no disagreement.

That I had no opportunity of an acquaintance with the *Idées sur la Météorologie* and the *Traité élémentaire* of Mr. De Luc is true; and I regret that works such as I suppose them to be never came within my reach. Living in a city where literary advantages of this kind are far less numerous than in other places, I have had often to feel and lament the consequent limitation of my knowledge on subjects with which I was most anxious to be acquainted.

Of the hypothesis of Volta concerning the standard of electrical comparison I was not altogether ignorant; indeed the only knowledge I possessed on the subject was derived from some papers of Mr. De Luc's in Nicholson's Journal. I formerly searched into various books with the design of gaining a better acquaintance with Volta's view of this standard, but I was unable to obtain it. I found, it is true, a paper by Volta in some of the old Philosophical Transactions, written in Italian: it related to the action of electric atmospheres upon his condenser, but they did not afford what I desired.

To enter into a discussion with so experienced a philosopher as Mr. De Luc, were to presume myself what I am conscious of not being. I therefore decline that task, and content myself with barely submitting to him a few suggestions concerning his defence of Volta, and his observations on my objections.

Mr. De Luc by a striking experiment has illustrated the Voltaic hypothesis. By the continued dispersion of electricity in a room, the air was rendered positive. A pair of insulated pith balls in the natural state were brought in from an adjoining room: they now diverged negatively; but when returned to the room from whence they came, the natural state 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,

room, yet, when brought into an air containing a greater *absolute* quantity of electricity, they became relatively negative. Now, if this admit of a different explanation, the necessity of the above inference is destroyed: it is therefore of importance to try if this can be effected.

With regard to my experiment in which bodies possessed of similar states attract each other; Mr. De Luc considers that the excited electric repelled the similar electricity of the pith ball through the thread into some other part: the effect is owing to what Volta calls electric influence, and it appears to constitute an essential part of his system. But with this I have been long familiar; not as belonging to Volta, but to Franklin. Now it appears to me, that this principle of electric influences may be applied in explanation of Mr. De Luc's experiment 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 are therefore left minus. With this view, the sequel exactly corresponds; for, when the balls were brought back into the un-electrified room, they collapsed into their natural state. But, on the opposite view, the balls by immersion into a positive atmosphere should assume that state, and retain it when removed, and therefore still continue to diverge. Hence it would appear to me that the experiment is an anomaly on the hypothesis of Volta, and perfectly reconcileable to the principles of Franklin.

But my experiment above referred to, in which bodies of a similar state attracted each other, does not in my mind appear to be explained by the principle of electric influences. Mr. De Luc supposes that the excited tube repels the electricity of the ball through its thread, leaving it ultimately negative, and therefore attracting it. But at the moment when this attraction is about to take place, if the tube be dexterously removed, it will be found that the ball is still *positive*.

Furthermore, it is for Mr. De Luc's consideration, whether the principle of electric influences be even applicable to this experiment. Volta's principle, if I understand it, relates to two bodies, one of which is in the natural state, and the other in a supernatural state. In my experiment they are both in a supernatural state.

With regard to the explanation of the motion of electrified bodies given by Volta, and cited by Mr. De Luc, p. 99, I need only observe that it virtually destroys the principle of repulsion, and refers all to attraction. How then can it be at the same time supposed that electricity is an elastic fluid?

The experiment which I consider subversive of Franklin's supposed impermeability of glass, appears to Mr. De Luc explicable
by

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by the hypothesis of Volta. This philosopher analyses the electric fluid into two elements: the one, called electric matter, cannot pass through the glass; the other, called deferent fluid, passes with ease. Mr. De Luc subjoins: "so far this system might appear only an hypothesis to Mr. Donovan, as he does not know the work to which I refer." I must confess that to me the whole has a very hypothetical appearance, but I am willing to admit my ignorance of the above-mentioned work as the cause. There is not, however, any hypothesis in the simple experiment which was detailed in my former paper. A thin flask 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 moveable 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 slightest 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 natural.

Many more observations might be made, but these are all that I consider necessary to offer for Mr. De Luc's consideration. The object of my former paper was not to defend any hypothesis, but, on the contrary, to show that we are deceived when we suppose ourselves possessed of any knowledge of the electric fluid beyond the perception of its sensible effects. The only mode of inviting truth is to remove the barriers which oppose its progress. I am persuaded that there would be no one more willing than Mr. De Luc to lend his assistance to the acquirement of so desirable an object.

I am, sir, &c. &c.

Dublin, March 10, 1815.

M. DONOVAN.

XXXVIII. *On certain Products obtained in the Distillation of Wood, with some Account of bituminous Substances, and Remarks on Coal.* By J. MACCULLOCH, M.D. F.L.S. *Chemist to the Ordnance, and Lecturer on Chemistry at the Royal Military Academy at Woolwich**.

IT is well known that when wood and other vegetable substances are submitted to destructive distillation, there is produced, among other matters, a black dense fluid resembling molasses or com-

* From the Geological Transactions, vol. ii.