## ARCHDEACON PRATT ON M. DELAUNAY'S EXPERIMENTS ON THE INTERNAL FLUIDITY OF THE EARTH.

SIR,—In the GEOLOGICAL MAGAZINE for September, you gave us a paper by Archdeacon Pratt, combating M. Delaunay's objection to Mr. Hopkins's method of reasoning from the precession of the equinoxes to the internal fluidity of the earth. There are some errors of inadvertence in that paper, which I do not point out, as no one is better able than the writer himself to discover them. We may agree, however, thus far with Archdeacon Pratt,—M. Delaunay's objection is not as conclusive as he himself seems to think. The *principle* of that object is indubitably true. M. Champagneur has proved it so by direct experiment; and it is (as appears to me) self-evident, *à priori*. If the tendency of the hard crust of the earth to shift on the internal fluid mass be sufficiently small, *relatively* to the degree of viscidity of the fluid, the crust must carry that viscous interior along with it in the changes of direction of its rotation. But *is* the relation such, in the case in question, as to make M. Delaunay's principle applicable? This question, I suppose, never can be answered. If the crust were even 1,000 miles thick, and if the fluidity of the interior were perfect, the pole of the crust would be slipping over the fluid interior at the rate of one inch in about twenty-five minutes (if the crust be as thin as some geologists have supposed, the rate of slipping would have to be nearly a third greater); and this shifting movement would occur all round the great circle parallel, at each moment, to that containing the celestial poles of the equator and the equinoctial points (taking the retrogressive movement of the earth's axis in its mean direction). Now, what amount of viscosity would be necessary to overcome the enormous moment of inertia, round its axis, of a globular mass 6,000 miles in diameter (or much more, as some would think), and start it afresh at every instant, in a new direction, at the above rate (or, greater), from a state of relative rest? And is the actual viscosity sufficient? Certain considerations would weigh for, and others against, M. Delaunay's opinion; but on which side of the scales the preponderance lies we cannot tell, from our ignorance of some of the conditions of the problem.

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NEWTOWN PARK, BLACKROCK, DUBLIN,
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## THE LECTURE ON VOLCANOS.

SIR,—Upon my return to London I received the GEOLOGICAL MAGAZINE (for September), containing, page 440, a letter from Mr. Poulett Scrope, commenting upon my lecture on volcanos, which appeared in the July number; the following remarks in reply were, however, too late for last month's Magazine, as you informed me that the October number was then already in the press.

I have always looked upon Mr. Scrope's works on volcanos as being by far the best on the subject which we possess, whether they be considered from a philosophical or a descriptive point of view (and in the latter sense the author's experiences in the field render

them doubly valuable), and, consequently, I had great pleasure, when perusing his letter, to find that he felt inclined to differ from me only in a very few, and I might add, not very important, points.

Having devoted some time to experimental researches upon volcanic action, and having had more than ordinarily good opportunities of studying in the field the active as well as passive volcanos of various parts of Europe, Africa, America, and Australasia, I attempted, in the lecture alluded to, a résumé of the conclusions I had arrived at independently, but which in many points fully confirm some long previously announced by Mr. Scrope himself; and had it not been for the necessity I was under, of condensing so large a subject as volcanos into the short space of an hour's lecture to a non-scientific audience, and, consequently, compelled to omit all explanatory details, I believe that Mr. Scrope himself would not have felt inclined to differ, even as to the few points to which he refers in his letter, and to which I will now briefly allude.

1. With regard to the formation of volcanic sands, ashes, and dust, I fully admit, with Mr. Scrope, that the greater part is probably due to the attrition of the particles *inter se* during their repeated ascents from, and descents into, the crater; nevertheless, microscopical examination shows that, in a very large quantity, the particles are more or less fused or rounded externally, as well as full of air, gas, or steam pores or bubbles, which would indicate that they have been comminuted and, as it were, blown to pieces whilst in the viscid, if not molten, state.

2. The opinion that molten lava, when suddenly brought into contact with water, is broken up instantaneously into coarser or finer particles, even to mud, is founded, not only upon observation, but on experiment also; for when it is found that molten lava, furnace slags, glass, and other silicates, do become so broken up when poured out suddenly into cold water, I think it is but reasonable and fair to infer that they would also so behave in nature, and, therefore, demur to the assertion, "It is very difficult, not to say impossible, to do more than guess at the effect produced on a body of lava expelled from a volcanic vent beneath water." The triturating action of the sea, which would naturally be enormously increased in such convulsions, is not only admitted but assumed by me as part of the process; the whole drift of the paragraph in question being to explain how great sedimentary deposits, composed of volcanic matter, may be formed, often similar in appearance, but infinitely more rapidly than those beds slowly built up from the débris arising from the wearing away of terrestrial rocks, brought down by the action of rain and rivers.

3. As before alluded to, this is but an instance in which my conclusions are identical with those previously arrived at by Mr. Scrope, and I feel sure that all unbiassed geologists must ultimately admit that the internal agencies have played the most prominent part in determining the external features of the earth.

4. Lastly, Mr. Scrope objects to the use of the words Cataclysm and Cataclysmic in contradistinction to Uniformity and Uniformitarian.

I am quite willing to extend the signification of Uniformitarian, in geology, to any action, however varied in intensity, from the most slow and steady to, at times, the most rapid or violent; as long as such catastrophes or paroxysms, as Mr. Scrope calls them, occur (like the striking of a clock) at regular or equidistant intervals of time, even if they do occur "but once in a century, or even in a thousand years." Before, however, internal or volcanic action can be brought under such a heading, it must first be proved that their catastrophes or paroxysms do occur at regular intervals; and until this is shown—more especially as the bulk of evidence at hand at present tends rather to the opposite conclusion—it does not appear unreasonable to continue to call these agencies Cataclysmic, as long as by the word Cataclysm in geology is understood some more than ordinarily violent event in the earth's physical history occurring at intervals altogether irregular and undeterminable.

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## A FACT RELATING TO THE CRAG PIT AT THORPE, NEAR NORWICH.

SIR,—In my paper on the Norwich Crag and associated beds, read before the Geological Society last autumn, I mentioned a local feature which I had noticed some fifteen years since, and which I had not seen reproduced since that period, viz., the occurrence over the shelly sand (Norwich Crag), overlying the Chalk, of a thin bed of clay (which I suggested might be the Chillesford clay), succeeded by an ironstone conglomerate containing impressions of shells. On my visit there again last week, I found in a freshly cut part of the section at the west end of the pit the same features exhibited, but on a larger and better scale. A bed about 2 feet thick, at the lower part of the gravel capping the section, is cemented into a ferruginous conglomerate, which has been the means of preserving the casts and impressions of numerous shells. Under this is a bed of light grey clay, 1½ foot thick, and then 5 or 6 feet of white sand and fine gravel, with an abundance of the usual Norwich Crag shells in the lower part of it.

The impressions in the ferruginous conglomerate are very abundant and very well preserved. My visit was too short to make a proper collection of them, and as the bed may, as the former one was, be worked out before long, I would direct the attention of any geologists visiting the pit to the interest of making a good collection of these fossils, amongst which I noticed the following:—*Pectunculus glycimeris* (?), *Cardium edule* (?), *Mytilus*, probably two species, *Paludina lenta* (?), *Mactra*, *Mya*, etc. These are the beds which I placed in the horizon of what I have termed the Westleton shingle, and which in the neighbourhood of Southwold contain casts of Mytili and other shells in abundance, and reposes in all that area on the Chillesford clay.

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