LETTERS TO THE EDITOR

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Thickness of the Earth's Crust

UPON my return to London yesterday, I received the two last numbers of NATURE (May 11 and 18), in both of which I find communications on this subject. In the first of these, by Archdeacon Pratt, that gentleman inserts a quotation from a lecture delivered by me, on January 29, this year, "On the Nature of the Earth's Interior" (*vide* NATURE, February 9, 1871), to the effect that the recent experimental researches of the eminent astronomer and mathematician, M. Delaunay, had destroyed the basis upon which the late Mr. Hopkins's reasonings, as to the solidity of the earth's interior, were founded, and asks the lecturer, *i.e.*, me, "I wonder why he has taken no notice of my letter in reply to M. Delaunay, which was printed in your journal for July 1870, six months before the lecture was delivered, and which also appeared about the same time in the *Philosophical Magazine* and the *Geological Magazine.* In this I showed that M. Delaunay had evidently misconceived the problem, and that Mr. Hopkins's method is altogether unaffected by his remarks."

As Archdeacon Pratt has the candour to admit that "any one with an ordinary degree of knowledge of popular astronomy and of mechanical action is quite competent to form a good opinion on the point in dispute," I would, in answer to the question he puts to me, simply state that, after a careful study of the letter he refers to, upon its first appearance in the *Philosophical Magazine,* I *purposely* avoided referring to it in my lecture, since I failed to discover that the author had in it "showed that M. Delaunay had evidently misconceived the problem," or any reasons whatsoever which could shake my faith in the conclusions of M. Delaunay, subsequently confirmed experimentally by M. Champagneur. I would also mention that, previous to this lecture, I attended the meeting of the Royal Society on the 22nd December, 1870, expressly to hear a subsequent paper by Archdeacon Pratt "On the Constitution of the Solid Crust of the Earth," on which occasion the opinions of Professor Stokes and the experimental demonstration of Mr. Siemens, as to the untenable nature of the author's conclusions, still further confirmed me in the views I put forth subsequently in my lecture.

It is now superfluous to specify in detail the precise reasons for my rejecting the arguments of Archdeacon Pratt, as I have, in a great measure at least, been anticipated in so doing by the substance of two letters, signed respectively "A. J. M." and "A. H. Green," which appeared in my absence in the last number of NATURE; to these I may refer in support of my view, in which I may also add one of our first English mathematicians has concurred; that M. Delaunay has not changed his will be seen from the Proceedings of the Academy of Sciences at Paris, March 6, 1871.

Having always entertained the highest opinion of the scientific labours of the late Mr. Hopkins, I have taken pains to make myself acquainted with his writings as far as possible for me; but when Archdeacon Pratt states "what Mr. Hopkins did may be divided into two parts—he first *conceived an idea,* which was to be the basis of his calculation; and then he made the calculation," I regard the whole pith of the question as embodied in these words, which admit that Mr. Hopkins based his elaborate calculations upon *an idea,* now shown by M. Delaunay to be incorrect, whilst the latter gentleman, on the contrary, founds his deductions upon premises which he first proves to have stood the test of experiment. Where eminent scientific men are arrayed on each side of a question of this nature, the remarks made in the last paragraph of the archdeacon's communication seem rather out of place, and might be applied with equal force in an entirely opposite sense to that intended by their author.

May 20 DAVID FORBES

The Geographical Distribution of Insects

IN NATURE (No. 74, p. 435) was a very interesting article on geographical distribution by Mr. Wallace, combating some recently-urged views of Mr. Murray's. Mr. Wallace took, as an example, the Madeira Islands, and sustained his position upon the numerical statistics furnished by Mr. Wollaston in his books. That these conclusions are very different from those arrived at by Mr. Wollaston is evident and as a six months' residence in the more remote group of the Canary Islands confirmed to my mind Mr. Wollaston's position, while bringing into relief facts utterly incompatible with Mr. Wallace's, I have ventured to publish a few remarks on the question.

Mr. Murray's views of the distribution of beetles seem to me resolvable into saying that there are two faunas, a tropical (Brazilian and Africo-Indian) and an extra-tropical one. My own slight researches in exotic coleoptera (confined hitherto to the Coccinellidæ) strongly confirm this; and a curious instance of the connection between the northern and southern extra-tropical faunas occurred to me the other day. *Eriopis connexa,* a rather pretty little ladybird, occurs from Hudson's Bay and Vancouver's Island all the way to the Straits of Magellan; following, of course, the line of the Andes. But my object was principally to question some of Mr. Wallace's conclusions with regard to the Madeiran fauna.. First of all, I was struck by the absence of any hypothesis for the origin of the very curious endemic forms which form the most important part of the fauna, and which most closely unite it to that of the Canaries and Azores. These Mr. Wollaston, myself, and apparently Mr. Murray regard as affording proof that these islands, or rather groups of islands, were once parts of a considerable continent, and I certainly am at a loss to see how else they are to be explained; for though Mr. Wallace regards the Madeira islets as possibly formerly connected, he would, I suppose, be unwilling to extend this to the other groups. Mr. Wallace appears to regard Mr. Murray's hypothesis to be that the Atlantic continent, of which Madeira is a remnant, derived its fauna from Europe; but it seems rather to be that in the Miocene period (or earlier) there was a similar continent, connected indeed with Europe, not deriving its fauna from Europe any more than Europe from it. Perhaps the best way of answering Mr. Wallace's view will be to take the case of the Canary Islands, whose fauna, resembling the Madeira as it does so closely, must have had a similar origin. Here the argument from apterous genera fails to a very great extent. Thus *Carabus* is represented by three species, while in S. Spain there is one, and in N. Africa only six or seven. *Thorictus* has three representatives, and here it may be noticed that ants'-nest beetles are decidedly not numerous in the islands, so that the "unusual means of distribution" fail on the whole to get them across the water. *Rhizotrogus* is represented by the closely allied also N. African genus *Pachydema.* Of the very numerous European *Rhizotrogi* only two Sicilian ones are apterous, so that its absence in Madeira tells either way. *Otiorhynchus* is no doubt absent, but its place is more than supplied by *Atlantis* (20 sp.) and *Laparocerus* (30 sp.). *Pimelia* again is represented in the Canaries by twelve species, and the apterous genera of Heteromera by more than fifty species, which almost demonstrates the necessity of looking for Tenebrionidæ in localities where they are likely to occur.

Tarphius it certainly is difficult to conceive carried across by winds or waves, seeing that its habits are so retired that it has escaped notice till very recently in Europe. Now, however, it is beginning to turn up in suitable mountain localities of Andalusia, Portugal, the Apennines, Sicily, and Algeria; four species are described, and I have seen two others, all agreeing *inter se* and differing in structure from any Atlantic species. Moreover, it must have been carried apparently to the Azores as well. Then of the peculiar apterous genera quoted, *Thalassophilus, Torneuma, Scoliocerus, Xenomma,* and *Mecognathus* occur now also in Europe, requiring only a collecting-power equal to that of Mr. Wollaston for their discovery. There remain as puzzles upon the hurricane theory twenty-two blind species in the Madeira and the Canaries, and the whole series of Euphorbia-infesting species, fifty in number, all winged, and forming for the most part special genera. Finally, with regard to the fauna of the Azores, the condition of the islands must be taken into account; if the species found round Santa Cruz, Oratava, and Funchal were enumerated, about this proportion of European species would be found. The best island, Pico, has not been worked, and in the others almost all the original vegetation has disappeared. The fact that in the scraps (as they literally were) of Euphorbias, *Tarphius* and *Acalles* occurred, shows that if any of the pristine flora could be found a fair number of species might be expected. *Elastrus dolosus* may certainly have come from Madagascar by the very ingenious route sketched out by Mr. Wallace; but the occurrence of *Urania* in Madagascar, Brazil, and the West Indies suggests a possibly shorter route, even though no *Elastrus* be known as yet to occur in America.

In conclusion I may state that I am going to spend a year or