causes of movements of the earth's crust; for if the fluid or viscous layer is chiefly due to internal heat and the relaxation of pressure near the surface, it may exist much nearer to our feet than could otherwise be admitted.

One of the gravest difficulties that the theory that added weight produces subsidence by acting on a fluid layer has had to contend with has been the great depth at which this fluid layer has had to be placed. It has always seemed to me next to impossible that liquid lava could well up from any such depths as those assigned to the viscous layer, or that a solid crust of so great a thickness should be sensitive to, as it is now shown to be, and rise and fall under, barometric changes. In acknowledging Mr. Fisher's letter and thanking him, I feel I am ungrateful in questioning that part of his work which interposes barriers which would break up the continuity of the viscous layer; I allude to his theory of "the roots of mountains." There does seem to me to be little fact in support of so startling a proposition, and I think the existence of volcanic vents, scattered through and in the midst of some of the highest chains, renders its acceptance difficult.

Mr. Murray restates his theory of the formation of coral atolls and reefs in the clearest manner, but I do not see that he explains any fact left unexplained by Darwin, or exposes any flaw in Darwin's reasoning. These masses of coral may have been continuously forming throughout even successive geological

Sheet 21. Geological Survey of Ireland, Antrim Coast, facing north-east.

periods, and their thickness is perhaps not exceptionally remarkable relative to that of slowly deposited oceanic sediments. There is no evidence that atolls are mere incrustations of volcanic craters, and it seems to me difficult to imagine so great a number of craters at the same level so completely masked. There are volcanic isles in abundance outside coral areas, but none I think, or few, of the form of a coral atoll. After all, Mr. Murray only shows that a second explanation is possible, though I still prefer the first.

I regret, being from home, that I am unable to answer Mr. Stanley. I may have alluded to the sinking of Greenland myself, and if I did not it was because the illustration was too familiar and self-evident. The sinking on the Greenland coast is not, I have understood, universal.

I still think it would render a service to science if readers of *NATURE* residing on sea-coasts would furnish authentic examples of elevation or subsidence or of waste. The magnificent Antrim coast, which I have recently visited, furnishes examples of subsidence among most unyielding rocks. The cliffs on the mainland are capped with basalt and dip inland, yet the basalt reappears in the Skerries out to sea with the same dip and at a much lower level. The same correspondence in stratification is seen between the mainland and Rathlin, but also with a great difference in elevation. The dip inland in all cases on this coast

should bring up much older rocks out to sea, unless we are prepared to admit a fault running parallel to the coast, and following its sinuosities, and at right angles to the general lines of faulting.

The way in which all the strata forming the cliffs along the Antrim coast dip inland is very remarkable. The accompanying tracing from the Geological Survey Map is of a particularly indented coast-line, and the arrows show that the dip is everywhere away from the sea, irrespective of any general strike. In fact the general strike must often be the reverse of that shown on the coast for the same strata crop out at much higher levels on the hills farther inland. I recollect that most cliffs that I have examined, particularly in Hampshire, dip away from the sea. It would appear that the removal of weight along a cliff line causes a local elevation, which gives a cant inward, whilst subsidence takes place under sediment farther out to sea. This seems to explain the observed facts connected with marine denudation; but I must take a future opportunity of entering more thoroughly into this part of the question.

Glasgow, September 12

J. STARKIE GARDNER

"Zoology at the Fisheries Exhibition"

LETTERS have been published in *NATURE* of August 9 and 16 (pp. 334 and 366) by Mr. Bryce-Wright of Regent Street and Prof. Honeyman of Canada, calling in question the accuracy of statements made in an article in *NATURE* (vol. xxviii. p. 289) which were condemnatory of exhibits for which these two gentlemen are respectively responsible. It is natural that they should seek to remove the unfavourable impression which the statements in question were intended to convey: they seem, however, to have been unacquainted with the complete character of the information upon which the statements were based. Mr. Bryce-Wright states that it is not the fact that some of the corals exhibited in Lady Brassey's case belong to him. Nevertheless it is the fact that when the jury of Class V. asked Mr. Bryce-Wright to point out the corals entered in the official catalogue under his name, No. "8136," he informed them that the corals so entered were in the same case with Lady Brassey's corals, and formed part of that collection. It is also the fact that in the opinion of experts the names attached by Mr. Bryce-Wright to many of these corals are incorrect; and as to his assertion that these specimens have been compared with those in the British Museum and with those obtained during the *Challenger* Expedition, it is a fact that neither the one series nor the other has been accessible for such purposes for some considerable time, and I have reason to believe that no qualified zoologist has made a comparison of the corals exhibited by Lady Brassey and Mr. Bryce-Wright with any collection at all.

The letter of Prof. Honeyman in reference to the naming and state of preservation of the Collection in the Canadian Department, for which he is responsible, is misleading. The discreditable state of that collection, to which a passing allusion only was made in *NATURE*, has been remedied in one or two instances since the visit of the jury of Class V. Should there be any doubt as to the justice of the opinion expressed in the article in *NATURE*, I would simply ask Prof. Honeyman whether he would have any objection to allowing the matter to be decided by reference to the report of the jury of Class V., of which he was a member. I should be surprised (and so I think would he) were the report of that jury, when published, found to be at variance with the opinion expressed in the article in *NATURE*. Prof. Honeyman's statement that the specimen of *Cryptochiton Stelleri* is properly exhibited in a convenient glass jar and labelled inside and out, is calculated to mislead. When first exhibited it was not labelled with any name; subsequently it was labelled with the name of a genus of Holothurians, "Psolus." After the visit of the jury of Class V., probably as the result of information imparted by some of the eminent zoologists who served on that jury, it was labelled with its proper name. Without citing details, I shall simply state that there are (or were when the article in *NATURE* was written) far more serious blunders in the identification of specimens and worse instances of bad preservation in the Canadian collection of Invertebrata than those to which special allusion has been made.

THE WRITER OF THE ARTICLE

A Complete Solar Rainbow

MR. D. MORRIS, in his account of this rainbow (p. 436) appears to have fallen into a mistake in stating that its inner dia-