

erosion, and as showing how similar (allowing for the difference in size) are the phenomena of the Himalayan and the Alpine glaciers. But I think that moulins, as a rule, are not likely to be very important agents in the formation of the rock-basins in which lakelets and tarns are often lodged. So far as my experience goes, the range over which the moulin-torrents can act is very restricted; for the crevasse, which gives the opportunity to the water, is generally formed very nearly at the same part of the glacier. Thus after the moulin has travelled for a very short distance down the glacier, a new crevasse opens out behind it and cuts off the torrent. I have frequently seen four or five dry shafts in advance of the working moulin. The lateral range also of the moulin must be small. Hence I think that the giant's-kettle (as is usually supposed) more accurately represents the ordinary product of a moulin. An excellent illustration is afforded by the well-known "glacier-garden" at Lucerne. I think, also, that the rock-basins, of which we speak, are more commonly found in situations where moulins would not be numerous or large, viz. in cwms and corries. It is, however, true that in certain undulating rock districts, as parts of Scandinavia and the Scotch Highlands, lakelets are common. The form of these, however, does not appear to bear much relation to the hollow produced by a moulin. So that I doubt whether we can regard a moulin as an agent of primary importance in the production of an ordinary rock-basin, though it may sometimes be a minor contributory. As I have more than once discussed the question of the probable cause of the formation of tarns as well as of large lake-basins, it is needless to repeat what has appeared in print.

T. G. BONNEY.

23 Denning Road, N.W., November 13.

#### "The Zoological Record."

In your Notes for October 26, on p. 621, you follow the Editor of the *Zoological Record* in suggesting that, under the present financial conditions, palæontology should be removed from the volume issued by the Zoological Society, and provided for by the palæontologists themselves. Against such retrogression we desire to protest. "Everyone knows," as you say, "that an incomplete record is of very little use"; and how absurdly incomplete a record would be that took no account of palæontology! The objectors probably spring mostly from the ranks of systematic zoologists. We will deal with them on their own ground. The systematic position of *Limulus* has long been a vexed question, which no one can attempt to solve without consulting the work of Malcolm Laurie on the fossil Eurypterids. The classification of the Crinoids has troubled zoologists since the days of Johannes Müller; but neither he nor anyone ever dreamed of settling it without reference to palæontology. Students of recent Bryozoa will not be grateful to those who keep them in ignorance of J. W. Gregory's lately published work on the Bryozoa of the early Tertiary rocks. And so we might go on *ad infinitum*. Another argument that may affect the systematists is that if they reject all names of fossil genera and species from the record, they will have no means of knowing whether the new names they may wish to propose have been used before or not. It is even possible that some of them may unwittingly describe as new forms already described by some unknown palæontologist. It is hardly necessary to remind the morphologists, embryologists, and zoogeographers of the help that they constantly receive from the palæontologists; they, at least, will not wish to have the record made incomplete.

It is suggested that every branch of science should have a record, and that palæontologists should undertake the compilation of a separate one. This would as good as double the work, both for recorders and students. What we have said above shows that palæontology is not a separate science. Zoologists and palæontologists ought to be the same people, and when they have strength enough they are so, as the names of Cuvier, Owen, and Huxley sufficiently testify. The palæontological recorder would still have to work through the writings of the zoologists, while even the pure neontologists would have perpetually to refer to the palæontological record.

What is really wanted is to complete the *Zoological Record*, not to make it incomplete—to go forward, not backward. It is admitted that some of the recorders do tackle the palæontological literature. Why should not all? If a group is too large for one man, then give it to two, and if a second man cannot be got to work on half-pay, then double the pay.

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To prevent the record becoming too big, make it merely an index, and cut out the abstracts, which are rarely correct. If more money is wanted, appeal to other societies which might naturally be supposed interested in the work. It is unfair that a single society should bear the burden of a work that is of value to all, and one can hardly suppose that it would refuse kindly offers of help. We believe, indeed, that the only reason why some of the recorders abstain for the present from the palæontological work is because they feel that part, at least, of the expense ought to be borne by the society more directly interested.

R. I. POCKOCK,  
F. A. BATHER,  
B. B. WOODWARD.

British Museum (Nat. Hist.), October 30.

#### Recognition Marks.

A QUESTION in natural history has occurred to me, which, I think, might with advantage be discussed in your columns.

It is usual to account for the white tail of the rabbit (*Lepus cuniculus*) by saying that it is useful as a danger signal to others of the species. Wallace, in his "Darwinism," speaking of rabbits, says that "the white upturned tails of those in front serve as guides and signals to those more remote from home."

Now, there appear to me to be two objections to this theory. The first is that the tail of the hare (*Lepus timidus*) is also white, and is turned up in precisely the same manner when running; but it is obvious, from the habits of this animal, that in its case it would be quite unnecessary for such a purpose.

And in the second place, if this were so, how could it have been produced by evolution? The object of the white tail is said to be to assist other rabbits to escape, not the possessor of the white tail itself. But the principle of evolution is the survival of the animal fittest to preserve its own life, not of the fittest to preserve the lives of others of the same species.

G. J. MACGILLIVRAY.

3 Belford Park, Edinburgh, November 6.

MR. MACGILLIVRAY has failed to grasp the principle of natural selection when he thinks that it cannot produce a character useful to other animals of the same species. The action of natural selection is to preserve the *species*, as well as each individual separately; and, consequently, every character useful to the species as a whole would be preserved. This is obvious when we consider such characters as nest-building in birds, and milk-secretion in mammals, which do not benefit the individual possessors, but their offspring; and the same principle applies to every character which is mutually useful to individuals of the same species, as are what I have termed "recognition characters." Neither can I admit that the habits of the hare render the white upturned tail "quite unnecessary." The hare is a nocturnal feeder, and a mark which readily distinguishes a friend from an enemy, and enables the young during their short period of infancy to keep within sight of the mother, must be of considerable importance.

ALFRED R. WALLACE.

#### Correlation of Solar and Magnetic Phenomena.

In writing on this subject (*NATURE*, vol. xlix, p. 30), to save space I omitted to refer to one other case of resumed connection. But as such omission might be misunderstood, may I here briefly allude to it? M. Trouvelot, on June 17, 1891, observed changes going on in connection with a luminous appearance near the western limb of the sun, such as he had not before seen. But the magnetic movement was in this case insignificant (see *The Observatory*, vol. xiv, pp. 326-328). The same reasoning as before may be applied. If the smaller magnetic motions do really directly depend on solar changes of so marked a character, how does it happen that many greater recorded magnetic movements remain without corresponding solar change having been seen? It is a very interesting, indeed critical point, but much more information is necessary to prove that such close connection really exists.

The appearance was seen by Trouvelot near the sun's limb. There is a significant sentence ending a letter from the Rev. Walter Sidgreaves, of Stonyhurst (*The Observatory*, vol. xiv, page 326), as follows:—"But there are no indications of magnetic disturbance accompanying the solar eruptions seen through the spectroscope. Even the brilliant display on the western limb, of the 10th [September 10, 1891], has left nothing that