

1894, agrees almost exactly with the direction of the principal vibration at the same place; and, in a third, he estimates that at Gifu and other places in the Mino-Owari plain the movement of the ground during the earthquake of 1891 was not less than one foot.

The study of the pulsations from distant earthquakes is as fascinating to seismologists in Italy as it is to those in other countries. Professor Grablovitz makes a good suggestion for the organized investigation of earthquake pulsations: he proposes that a series of stations should be established near the great circles which approximate most closely to the main lines of volcanic action. Dr. Agamennone calculates the mean velocity of the earth-waves produced by the earthquake of Paramythia (Epirus) of May 13-14, 1895, and the earthquake of Amed (Asia Minor) of April 16, 1896; but in both cases he is hampered by uncertain initial data. Dr. Cancani estimates that the waves from the Indian earthquake of June 12, 1897, as they crossed Italy, were about 30 miles in length and 22 inches in height.

Among the miscellaneous papers may be mentioned two of considerable interest by the last-named author. Pheasants and other birds, it is known, feel the preliminary tremors of an earthquake before man, but Dr. Cancani believes that this is only the case at a distance from the epicentre, and he therefore infers (though the conclusion seems to me doubtful) that these tremors travel more rapidly than the main earthquake-vibrations. In the second paper he collects and discusses a number of observations on the so-called *marina* observed in the inland province of Umbria, and shows that they are identical with the *barisâl-guns* of India and the *mist-poeffers* of the North Sea coast.

The active volcanoes of Italy are the subject of incessant observation by a small, but careful, band of workers. Professor Mercalli, upon whom the mantle of Dr. Johnston-Lavis has fallen, devotes himself especially to the study of Vesuvius, and describes the phenomena observed from July, 1895, to December, 1896. Professor A. Riccò, the Director of the Observatory of Catania, communicates a few notes, chiefly relating to the central crater of Etna, while his assistant, Signor Arcidiacono, summarizes the principal eruptive phenomena of Sicily and the adjoining islands during the years 1896 and 1897.

C. DAVISON.

## CORRESPONDENCE.

### THE SUBMERGED PLATFORM OF WESTERN EUROPE.

SIR,—Prof. Hull is right in thinking that there is still much to be learned from a careful study of hydrographic charts. The existence of the submarine platform west of our Islands and of the great declivity which he calls an escarpment was of course well known, but the details of the submerged surface certainly merit more attention than they have received, and I do not think their interest is even yet exhausted.

But when Prof. Hull passes from observation to theory he makes several assumptions which are open to question. He calls the great

declivity "an escarpment," comparing it with true escarpments in England and France, and with the cliff-borders of the Nile valley (which are not technically escarpments). He says "all these escarpments have been formed over the surface of emergent lands," that they are absolutely terrestrial, and "that in ascribing a similar origin to those here under consideration we are only drawing a logical deduction from the premises laid down."

The logic of this does not seem very clear. Can Prof. Hull point to a true escarpment anywhere in Europe which has a length of 700 miles and a height above its base of 7,000 to 8,000 feet? Moreover, this so-called escarpment does not stop in the Bay of Biscay; it is continued round the coasts of Spain, it crosses the mouth of the Mediterranean, and runs down the whole length of Africa. It is part of the elevated shelf on which two continents stand, and Prof. Hull may call it an escarpment if he chooses, but it is not comparable with ordinary escarpments, and he is not justified in assuming that it has been formed by atmospheric agencies.

He also tells your readers that "a solid escarpment of this kind indicates a slow continuous elevation *after* the British platform had been planed down by wave action, and subsequent depression after a lapse of time." Here he assumes that the platform was formed first and the escarpment afterwards. I think most writers have supposed that the great declivity which marks the ancient border of the continent is a much older feature than the platform.

Finally, we are told that the formation of the platform "may be referred back with confidence to the Mio-Pliocene period, and that of the grand escarpment to the succeeding early Pleistocene or Glacial stage." There are probably others beside myself who would like to have the reasons for this confident assertion. Is there any reason why the formation of the escarpment and the union of Great Britain with Iceland should not have taken place in the Eocene period? That such a union may have been repeated at a later date is quite possible, but I think the history of the features described by Prof. Hull is much longer and more complicated than he supposes, and I would not like to say that either of them was formed wholly at any one period.

Prof. Hull may have good reasons for his statements, but he does not give them, and as his conclusions are not the only inferences that may be drawn from the facts, they must be discussed before they can be accepted.

A. J. JUKES-BROWNE.

#### VERTEBRATE PALEONTOLOGY.

SIR,—While thanking you for the gratifying review with which my "Outlines of Vertebrate Palæontology" are honoured in the August number of the *GEOLOGICAL MAGAZINE*, I should like to correct two misapprehensions of the reviewer.

Firstly, it is a mistake to suppose that any "*new terms* are introduced." All the terms employed are to be found in current literature, and most of them are in nearly universal use. Moreover, on its first mention each term which is not likely to be familiar to the elementary student, is not only printed in italics and briefly defined,