

of the Benedictine monks of Fulda, the trustworthiness of the lunar and solar tables, and the accuracy of the computer who brought out so marvellously correct a result without knowing that it agreed exactly with the true meaning of the record.

No doubt equal credit may be given to the computer's statement that this eclipse was total in London, totality continuing at St. Paul's from 1h. 16m. 20s. to 1h. 18m. 10s. local mean time.

C. S. TAYLOR.

Height of T'ai Shan.

A FORMER student of mine, Mr. S. Couling, has recently ascended T'ai Shan, the loftiest of the sacred mountains of China, and one of the most ancient and popular places of pilgrimage. He believes that the height of it above the surrounding plain has never before been measured, and has sent me his observations to reduce. The elevation from the plain to the summit comes out at 4780 feet; whilst a temple vaguely stated to be about 400 feet below the summit is, as ascertained by barometer, 4485 feet above the plain.

SILVANUS P. THOMPSON.

The Shadow of a Mist.

LIVING on the Blue Mountains at an elevation of 5000 feet, I am frequently astonished at the ever varying beauty of the mists and clouds. But a short time ago it was my good fortune to see the shadow of a mist, itself not visible.

On the evening of November 16, shortly after 7 o'clock in the evening, I was watching the electric light with which the military authorities were experimenting at Port Royal, 15 miles distant in a straight line. The light at times was so brilliant that the shadow of a person standing 20 feet from the house was distinct on the white-painted front, even when he held a lamp partially turned down close to his body on the side next the house. Rain was falling, but so slightly that there was no need for an umbrella. No mist or cloud was visible in the direct line to Port Royal, and yet a net-work of shadow was thrown on the house, the meshes of which were 3 or 4 inches in width. The shadows were all in motion, moving from east to west, in the direction of the scarcely noticeable breeze; individual portions of the meshes disappearing and re-forming as they moved, so that it was quite dazzling to look at the shadow, reminding me of the ripple on water as seen against a strong light. A puff of tobacco smoke had a shadow only when an inch or two from the house, so that the mist must have been much denser, and yet it cannot have been of any great breadth, or the shadow would have been uniform instead of reticulated. No doubt many of your readers can explain this appearance, which to me seemed so singular.

W. FAWCETT,

Director of Public Gardens and Plantations.

Cinchona, Gordon Town P.O., Jamaica, December 1, 1887.

The Ffynnon Beuno and Cae Gwyn Caves.

IT would seem that so long as the controversy with regard to the contents of these caves is confined to Dr. Hicks, Prof. Hughes, and Mr. W. G. Smith, the points at issue will never be decided. Dr. Hicks argues most needlessly for the *pre*-Glacial age of the cave deposits; Prof. Hughes calmly assumes that the outside deposits are *post*-Glacial; and many geologists must be heartily tired of hearing these two gentlemen contradict one another without defining what they mean by the terms Glacial and *post*-Glacial.

The fact is that the St. Asaph drift (to which Prof. Hughes now admits the outside deposits belong) is part of the later Glacial series of Northern England; and Prof. Hughes has no right to call it *post*-Glacial without defining what he means by that term. Most people call them Glacial deposits. If therefore the cave-deposits are older than this drift, they are not necessarily *pre*-Glacial, as Dr. Hicks maintains, but only anterior to what Mr. Meillard Reade terms the marine low-level boulder-clays. Now many think that these clays and their associated sands are coeval with, or newer than, the so-called *post*-Glacial river-gravels of Southern England. It is not surprising therefore that the cave fauna should be the same as that of the river-gravels, and it is perfectly needless to compare it with the fauna of the Cromer Forest bed.

In Lincolnshire the same marine shells occur in sands and gravels beneath the latest sheet of boulder-clay, and a gravel

beneath the same clay at Burgh has yielded teeth and bones of *Elephas antiquus*, *Rhinoceros leptorhinus*, and *Bos primigenius*. These beds are on the same line of latitude as St. Asaph, and are probably of the same age as that drift; but it may be that neither of them are older than the oldest river-gravels of the Cam or Thames valleys.

It has been repeatedly pointed out that the terms Glacial and *post*-Glacial cannot be used as conveying any idea of relative age except along one and the same parallel of latitude, and it is rather surprising that the Woodwardian Professor of Geology should seem to be unaware of this. If by *post*-Glacial Prof. Hughes means later Glacial or newer Pleistocene, everyone will probably agree with him, but he confuses the issue by his bad choice of terms.

The palæontological evidence is really of no value—the argument leads nowhere; what we want is an expression of opinion by some geologist who has seen the locality and the recent excavations, regarding the explanation proposed by Prof. Hughes, viz. that the present position of the bones beneath the marine drift is due to the falling in of the roof of the cave near one entrance, while the animals may have got into the cave by another opening. Many geologists have visited the locality—will some of them communicate their views on this point?

A. J. JUKES BROWNE.

Southampton, December 28, 1887.

THE OLD MOUTH AND THE NEW: A STUDY IN VERTEBRATE MORPHOLOGY.

"THE question of the nature of the mouth," says Prof. Dohrn in one of the first of his celebrated "Studien zur Urgeschichte," "is the point about which the whole morphological problem of the Vertebrate body revolves." According to Dohrn, the present mouth of Vertebrates arose from the coalescence of a pair of gill-clefts. In this we have an example of Dohrn's principle of change of function, and also, as I hope soon to demonstrate, of Kleinenberg's law of the substitution of organs. I do not now wish or intend to give an account of the researches by which Dohrn showed that the mouth in some cases first arises as a pair of lateral invaginations of epiblast, still less of my own small contribution to this question, which consisted in recording the facts that the mouth also resembles a gill-cleft in some other particulars.

It suffices here to say that these researches have not yet been refuted, and that the view that the present mouth of Vertebrates is, so to speak, a new structure, rests on a very sound foundation.

With the blastopore as the foundation of mouth and anus I have here no concern, nor have I any sort of sympathy with the upholders of a theory which has been condemned and rejected by embryologists such as Lankester, Kleinenberg, and Salensky.

The problem I have to discuss is, granted that the present Vertebrate mouth is a new¹ structure, what traces, if any, are to be found of the old mouth? It is conceivable, and I strongly emphasize the point, that the old mouth might have disappeared, even from the development, without leaving a trace behind.

We seem to be gradually getting out of the idea that ontogeny is even a fair repetition, much less a perfect one, of phylogeny, for absolutely rudimentary organs (organs performing no function at all) are only retained as larval or embryonic organs, as the basis or *Anlage* of other organs, or, finally, because they are inseparably connected with the development of other organs. Of the latter a fair case, it seems to me, is to be seen in the rudiment of the parietal eye in the higher Vertebrates. This organ, functionless except in a few fishes and reptiles, possibly only reappears in the development because it is intimately connected in some way or other with the paired eyes.

A still better example is, I think, to be met with in the

¹ It is rather paradoxical to speak of a thing as new which has existed in its present form for untold millions of years.