

valleys to make watertight barriers in the construction of reservoirs revealed the fact that the bottom of the valleys, wherever it was formed of shales and thin sandstones, was more or less folded and contorted. These folds and contortions caused the shales to let the water through with more or less freedom, and he had been called in repeatedly to advise as to how far it was necessary to carry the puddle trenches down below the valley bottom. He found, as a matter of experience, that these folds were superficial, and if the sinking was made to a sufficient depth below the bottom of the valley they disappeared altogether. It was therefore obvious that they were not due to deep-seated movements of compression resulting from the contraction of the earth. They are due to the relaxation of pressure caused by the removal of the rock by denudation from the area of the valley, and are analogous in every particular to the 'creep' in coal workings, caused by the excavation of coal, by which the surrounding strata crush down into the area of relaxed pressure and ultimately fill it up. This may be studied in any coal-pit where there is a superincumbent pressure, say, of more than 1,000 feet.

Two illustrations of folding and faulting by relaxation of pressure are presented by the puddle trench of the Langsett reservoir belonging to the Sheffield Corporation, and by the two reservoirs now under construction on the head waters of the Derwent by the Derwent Water Board. In the first of these the foldings in question at the bottom of the valley in the shale under the first grit are strongly marked at the surface. These folds gradually disappear, and are based upon a hard black unmoved shale offering a good foundation about 60 feet below the bottom of the valley. This is in the valley of the Little Don. The thickness of rock removed from the bottom of the valley amounted to no less than something like 8,000 feet of Coal-measures and Millstone Grit. In the case of the Derwent, in which the folding is much more marked and is accompanied by faulting, the thickness of rock removed amounted to at least 9,700 feet (7,200 feet of Coal-measures, 2,000 feet of Millstone Grit, and at least 500 feet of Yoredale). In this the movement had not extended beyond a depth of 90 feet. In the case of the Derwent reservoir lower down the river there are two systems of folding and faulting which do not penetrate beyond 60 feet from the surface. At that point a good foundation is found for the puddle trench of the bankment. These points are of considerable importance in considering the sites for reservoirs.—From *Water*, November 15th, 1904, No. 71, p. 491.

CORRESPONDENCE.

THE NEW QUESTION OF RIPPLE-MARK.

SIR,—In the August number of the *GEOLOGICAL MAGAZINE* I proposed to offer at some future time an explanatory paper on the formation of ripple-mark; writing then under the impression that experts were quite agreed upon a subject not generally known.

After my August paper was in type, Mrs. S. Ayrton expounded the subject of ripple-mark at the soirée of the Royal Society, and subsequently delivered a lecture at Cambridge under the auspices and special sanction of Section G.

In the course of the latter lecture Mrs. Ayrton ignored all previous enquirers, except Professor Darwin, whom she considered not to have fully appreciated the significance of his own experiments. This places me in a difficulty, as I am in doubt whether Mrs. Ayrton has considered the work of modern writers, or only the views expressed in the older text-books. Then there is another difficulty, and that is, that the subject was considered by Professor Osborne-Reynolds' Committee on Tides and Waves, which was a Section G committee; while the subject comes within the purview of the reappointed Committee on 'Terrestrial Waves, etc. As a matter of fact, Mrs. Ayrton's views, as endorsed by the Royal Society and Section G, are in conflict with the views accepted by two committees of the British Association and of all modern workers. Indeed, Mrs. Ayrton uses the words "Contrary to accepted opinion."

I am aware that Bacon declares that unanimity is a very dangerous thing, and that, theoretically, it might be safer for me to differ from Professors Darwin and Osborne-Reynolds, and Dr. Vaughan Cornish, who are the modern authorities who have dealt more particularly with the sand-ripples of tidal and other continuous currents; but, except in some microscopic points of details, there seems to me to be no room for doubt.

During the past months of September and October I had the little Moorland stream, the river Bovey, under close observation, and for eight weeks one particular sand-flat was more or less rippled.

Mrs. Ayrton maintains that a steady current cannot produce sand-ripples, and suggests that certain sand-waves on the Goodwin Sands are caused by stationary sea-waves. So far as my observation goes, there are no stationary waves in the open sea; and it is an uncontrovertible fact that a steady current, as understood by geologists, viz., a current flowing to all outward appearances continuously in the same direction, can, under certain conditions of speed, depth, and composition of bottom, form sand-ripples, as pointed out by Dr. Sorby, F.R.S., in 1859.

The point that geologists want decided is whether the sand-ripples proved by Monsier Siau in the Indian Ocean, in a depth of over 100 fathoms, were produced by continuous currents or by wave-action. A good deal turns upon the answer. To me the balance of evidence seems to be in favour of wave-action, because, as a rule, deep currents do not disturb the bottom, but slide over the strata of water which are in contact with the bottom; but I come to that conclusion in face of the fact that under certain circumstances continuous currents have been proved to ripple a sandy bottom.

I should like to see a committee appointed, to consist of all the leading workers who have written upon the subject of current ripple-mark, and especially of those who have delivered popular

lectures. I think, for instance, that if Professors Darwin, Osborne-Reynolds, and Fleming, with Dr. Vaughan Cornish and Mrs. S. Ayrton, were to confer and compare experiences a unanimous report might easily be arrived at. Until something of the sort is done the exposition of the subject, as endorsed and supported by the Royal Society and the British Association in their corporate capacities, will either be accepted by the public or cause a great deal of perplexity. The question does not touch my own special work, as all seem agreed as to the ripple-making powers of reciprocal wave-currents.

A. R. HUNT.

November 7th, 1904.

ELEPHAS MERIDIONALIS AT DEWLISH.

SIR,—I regret that I was unable to be present at the meeting of the Geological Society on the 9th inst., when my paper on the Dewlish elephant trench was read, suggesting human agency. I crave your permission to reply to one or two criticisms as reported in the Abstracts of the Proceedings. It is there said that some 'eoliths' found there were exhibited by me. If what I *did* exhibit is referred to, they were merely shown as geological specimens from the drift of the gravel with which the trench had eventually become filled—not as 'eoliths.' I have seen some 'eoliths' which were collected at Dewlish, but in my opinion (whatever that may be worth) they do not strengthen my hypothesis that the trench is artificial.

Mr. Hudleston remarked that he understood that the remains of only one elephant had been found. There are in existence *nine* well-preserved molars in museums, four at Dorchester, two at Salisbury, two at Cambridge, and one at Manchester. I exhibited at Cambridge all these except the Salisbury specimens. Mr. Pleydell in his paper in the "Dorset Field Club," 1889, mentions seven molars, so that two of the above enumerated must have been omitted in his list. In this paper he gives a list of remains. He says that isolated plates of other molars were scattered in various parts of the deposit, and that in some places fragments of ivory were so numerous as to predominate over other materials. This I think disposes of Mr. Hudleston's objection that the remains of only one elephant had been found.

It is obvious that the trench was not wide enough to contain the carcase of an elephant. But if such a beast once got his fore legs into a narrow trench twelve feet deep, he must have been in the "helpless condition" that Sir Samuel Baker refers to, in which he might have been dispatched at leisure. It is not likely that primitive men would have expended more labour upon their pitfall than was absolutely necessary.

O. FISHER.

HARLTON, CAMBRIDGE,
November 18th, 1904.
