

CORRESPONDENCE.

THE ROCKS OF SOUTH DEVON.

SIR,—In Professor Bonney's unfriendly criticism of my paper on the Devonian rocks of South Devon, in the October Number of the GEOLOGICAL MAGAZINE, I am taxed with the commission of three faults among other failings, viz:—

- (1) The avoidance of certain apparently possible alternatives which my critic deems of importance.
- (2) The not having studied a Devonshire problem in "other fields than South Devon."
- (3) The having attempted a research with insufficient materials.

In reply to the first I may state that had I been able to discuss Prof. Bonney's South Devon paper, the points referred to by him would have been satisfactorily disposed of; but I was unable to discuss that paper for the following reason. In October, 1891, Prof. Bonney volunteered to me the statement that he did not mean to enter into any controversy on the subject (of the Devon schists) until his shield was struck by a knight of equal experience. Under the circumstances I had no option but to leave the Professor and his paper alone.

With respect to the second objection, it is evident that the affinities between two sets of Devonshire rocks can only be studied in Devonshire, and not elsewhere. My subject was much more restricted than my critic seems to suppose.

Respecting the charge of insufficiency of materials for research, Prof. Bonney is scarcely in a position to find fault, seeing that he dismissed the whole of the complicated Start headland with the cursory observation—"Two specimens from different parts of the Start headland call for no special remark" (Q.J.G.S. vol. xl. p. 15). Your readers will scarcely be able to realize the significance of this naive remark.

SOUTHWOOD, TORQUAY,
16th November, 1892.

A. R. HUNT.

GLACIAL GEOLOGY.

SIR,—I have read with much interest the papers by Mr. Mellard-Read and Mr. Percy Kendall in your July and November issues. On the one hand we have the submergence theory proved up to the hilt, and on the other the glacier theory sustained with equal show of reason. Does it not strike the combatants that they may both be right and both be wrong? For at one time during the Pleistocene Period the land was certainly deeply submerged in the sea, whilst at another it was with equal certainty enveloped in ice.

There are one or two points in Mr. Kendall's paper to which I should like to refer. Soon after the late Dr. Carvill Lewis came to England, I had the pleasure of showing him the principal sections of Boulder-clay and sand in the Trent Basin, and I think I convinced him that even if there is "a commingling of the Drift" in some deposits in that area, there is also an equally marked absence of com-

mingling in other Drift deposits of the same area. If Mr. Kendall were to try to explain the distribution of the rocks in the Drifts of the Trent Valley on the glacier theory alone he would be in even greater difficulties than he is at present.

Mr. Kendall is not quite accurate in implying that Dr. Carvill-Lewis was the originator of the idea that the valley of the Trent formed a large lake at one time. This was clearly stated in a paper read by me before the Geological Society in 1886. In that paper I make the Middle Pleistocene Epoch open with a "land locked and probably ice-locked" "Melton-sand sea." Indeed the idea is used to explain the absence of mollusca in the deposits of this epoch. Dr. Carvill Lewis, I think, held that the water level in this sea or lake was above that of the Atlantic; but the facts rather supported the view that it was connected with the outside sea, the watershed of Central England being submerged several hundred feet.

It appears to be quite time that the advocates of glacier theories and submergence theories joined hands for the purpose of ascertaining if a more careful study of the "Glacial Succession" will not reconcile their present conflicting views.

R. M. DEELEY.

10, CHARNWOOD ST., DERBY, Nov. 15th, 1892.

THE MAMMOTH AND THE GLACIAL DRIFT.

SIR,—In the September Number of the *GEOLOGICAL MAGAZINE* (p. 405) Sir Henry Howorth writes: "I claim to have shown that, as tested by these islands, the Mammoth beds are in every instance overlain by the Drift, and are never underlain by it;" this claim being limited to cases where it is possible to apply the test of superposition. In my letter of October, I took two of his cases and showed that in both the beds enclosing Mammalian remains were underlain by Glacial Drift, *i.e.*, that the main mass of the local Boulder-clay passed beneath them; thereby disproving the verbal accuracy of his statement.

Again, on p. 400, he discusses the gravels in the valley of the Ouse, near Bedford, a case by the way in which the test of superposition does not apply. In this connection he quotes the discovery of flint-implements "at Thetford on the Ouse," and a few lines lower down he "turns to another site *in the same valley*," being one not far from Bedford (*italics are mine*). Replying to my obvious comments on this he says he has nothing to correct and nothing to alter in what he wrote, except the spelling of a word, and that the point is "only a test of my knowledge of the English language!" I feel sure your readers will by this time have seen that it was really a test of Sir H. Howorth's knowledge of English geography, and, as I said, of his practical acquaintance with the subject. I did not expect that I should be called upon to point out that the valley of the Little Ouse, between Norfolk and Suffolk, is entirely different and distinct from the valley of the Great Ouse, near Bedford! Not even Sir H. Howorth's approved ingenuity in the use of the English language can make them parts of one and the same valley. There