

must be left for further work. On one, however, I feel quite clear, namely, that the relations of the rocks have been completely misunderstood by Mr. Somervail.

(4). "On the Nature and Origin of the Banded Structure in the Schists and other Rocks of the Lizard District."¹ As this subject will form an important part of our paper, I content myself with observing that I can find no ground for Mr. Somervail's hypothesis of segregation, as he applies it. I question both the accuracy of his statements and the validity of his inductions. Doubtless before writing upon these difficult subjects, Mr. Somervail has trained himself by careful study both of rock-structures under the microscope, and of rock-relations in less complicated districts of other regions; but if so, I am utterly at a loss to understand his principles of interpretation and his methods of reasoning.

T. G. BONNEY.

BANDED ROCKS OF THE LIZARD.

SIR,—Mr. Somervail in his paper on the Lizard rocks, published in your last issue, advances the theory of segregation to explain all the phenomena displayed by the eruptive rocks of that interesting locality, but he does not favour us with any evidence in support of his theory, and he omits to explain facts that seem incompatible with it. That such rocks as peridotite, gabbro, diorite, basaltic, and felspathic traps, and granite—rocks of well-defined species differing from each other in mineralogical contents, structure, and chemical composition (points that imply genetic differences)—should be formed on the spot by segregation from a "common magma," is sufficiently startling to the petrologist; but when we find, as competent observers have found, that these rocks cut each other in well-marked dykes following each other in a regular sequence, and that each of the principal intruders carries along with it sharp fragments of the rocks through which it has intruded, the hypothesis involves the rejection of every canon of interpretation hitherto relied on by field geologists.

When one sees diverse igneous rocks cutting across each other in a way that implies differences in their order of eruption; and when one finds the lines of demarcation between these *successive* eruptions so sharp that even thin slices examined under the microscope show a sudden transition from a rock of one chemical and mineralogical composition to another of different chemical and mineralogical composition, it seems as unreasonable to a petrologist to attribute the formation of these definite and distinct species to segregation *in situ* as it would be to attribute the jaw-bone and teeth of a well-known quadruped, found in a bed of marl, to the fortuitous segregation of the carbonate of lime.

The above-mentioned rocks not only cut each other with a definite sequence, but they preserve their individual characteristics, whether

¹ GEOL. MAG. 1890, Dec. III. Vol. VII. p. 515.

they occur in veins a quarter of an inch thick, or in masses many miles wide.

Science is not advanced by the dreaming of dreams—to make progress we require evidence culminating in proof.

20, NEVERN SQUARE,
10th November, 1890.

C. A. McMAHON.

PROF. PRESTWICH, F.R.S., ON THE ELEVATION OF THE WEALD.

SIR,—I am much obliged to Prof. Prestwich for drawing attention to an expression in my “Note on the Elevation of the Weald” (*GEOL. MAG.* September, 1890), to which I feel bound to say *peccavi*. The fact is, when that paper was written, I was ignorant of the view which the Professor had put forward so long ago as 1858 in a paper, of which he has since been good enough to send me a copy. When my 1883 paper was written, the only published statement of Prof. Prestwich’s view on the geological data of the Wealden elevation, which I had before me, was that contained in the published abstract of a paper read (in my hearing) before Section C of the Brit. Assoc. at York in 1881. I am sorry I was misled by this; and the more so as it was criticized by me more than once in the 1883 paper, to which the Professor refers. A copy of that paper was sent to him at the time of its publication; but, strange to say, in the Professor’s letter (which is now before me) acknowledging the receipt of it (which seems to have been lost sight of since), and offering some remarks upon some points in it, no notice was taken of my criticisms on the York paper. Was it very extraordinary that under such circumstances I was lulled into the belief that I had correctly interpreted the statements contained therein?

Prof. Prestwich will kindly allow me to refer to some remarks I ventured to make in the discussions of Parts II. and III. of his recent great paper, “On the Westleton and Mundesley Beds, etc.,” the substance of which is published in the *Journal of the Geological Society*. These indicate, I think, sufficiently my position with regard to this question.

As to Mr. Clement Reid’s paper in “*Nature*” in 1886 (not 1888), I did not feel the necessity of pointing out (what must be obvious to any one who looks at it), that it was a “friendly corroboration” of Prof. Prestwich’s view expressed years before.

The argument for contemporaneity, “on the ground of approximate equality of altitude above the sea,” I had no idea of saddling upon Prof. Prestwich in particular. I mentioned it as the only argument I had heard put forward by geologists, with whom I had discussed the question, after I suggested in the pages of the *GEOL. MAG.* (1888) a different view to those generally held, from an examination of the principal sections “in the field.”

As regards the “larger and more theoretical questions” raised in my paper, I think I have sufficiently indicated the authorities which have furnished the data from which my inferences are drawn. I am, of course, allowed to draw my own conclusion from the Professor’s dignified refusal to consider them.