

tinental Arctic ice can do the same thing required “the invention of Croll, who, sitting in his arm-chair, and endowed with a brilliant imagination, imposed upon sober science the extraordinary postulate”; and the sober, simple, scientific explanation of every difficulty, imagined or real, is that the upheaval of “the highest masses of land on the earth, including the massive mountains of Asia and the American Cordillera,” was “very rapid, if not sudden,” and that “the breaking up of the earth’s crust at the time, of which the evidence seems to be overwhelming, necessarily caused great waves of translation to traverse wide continental areas.” His own words, “no science but long-suffering geology would tolerate the absurdity,” seem more to the point here.

There need be no loss of temper or heated argument on the subject. Neither dogmatic assertion nor the weight of authority will ultimately prevail. In the eyes of many it may be impertinent of me to have opinions on this or any other subject; but all, it seems to me, have a right to add their mite in the hope that it will assist in the elucidation of the truth. Of one thing I am sure, and that is, that although we have been preceded by Forbes, Agassiz, Bishop Rendu, Ramsay, and Tyndall, and a host of others who have ceased to work in this world, there still remains very much to be done.

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R. M. DEELEY.

#### THE “SOUTHERN DRIFT.”

SIR,—I am sorry that in my paper there should have been any statement which Prof. Prestwich or anyone else could consider misleading. In mentioning the name of Prof. Phillips, as well as that of Prof. Prestwich, in connection with the Southern Drift, my only object was to afford information to those unacquainted with the literature of the subject. I suppose I put Phillips first because the date of the work referred to (1871) was earlier than the quoted paper of Prof. Prestwich.<sup>1</sup> But no one who is at all acquainted with Geology, or even with contemporary history, can suppose that the slight reference in Phillips’s work bears any sort of comparison to the full and exhaustive work of Prof. Prestwich, who has done more than any other geologist to create an interest in the once despised “superficial deposits.” Nor is it any disparagement of Prof. Phillips’s reputation as a geologist that his treatment of these deposits in the Thames district should be necessarily imperfect, and to some extent based on information supplied by others. It is evident that he knew of a hill-gravel formed by “currents from the south transporting flints and sarsen-stones”; but the passage quoted by Prof. Prestwich contains all that he has said on the subject in the work referred to. He has also figured neolithic forms as “Implementations from the drift.”

<sup>1</sup> Prof. Prestwich writes:—“This is a mistake. Prof. Prestwich’s first paper on this subject was published in 1847. Besides, flint, gravel, and sarsen-stones *alone* do not prove a drift from the south. It might as well have been from the west or north-west.”—J. P.

I hope, therefore, that Prof. Prestwich will accept my assurance that I had no intention of suggesting that he was in any way indebted to Phillips. I regret also that my reference to the Weald is not sufficiently clear; I said: "It is open to question whether the Weald ever had the character of a mountain-region." The question which I was considering was whether stones are ever worn by the action of torrents so as to produce a groove in one side, and I placed the remarks quoted above in a footnote. Of course the Weald must at some time have been a region of comparative elevation; but considering the nature of the materials of which it was composed, and that denudation must have gone on during the period of gradual elevation, it appeared to me permissible to doubt whether it was ever a mountain-region in the sense in which, for instance, the district of Snowdon is a mountain-region; that is to say, a region giving birth to numerous torrents.

But in any case the argument in my paper would not be affected; and I have certainly no wish to raise a controversy on a matter in regard to which Prof. Prestwich knows far more than I do.

THORNDALE, CRAVEN ROAD, READING.

O. A. SHRUBSOLE.

#### MOUNTAIN-MAKING BY TENSION.

SIR,—Mr. Vaughan "having stated a new theory to account for the inequalities of the Earth's Surface," and this theory being dependant upon the tensile strength of the Earth's crust, I suggested that he should favour us with some proof that the outer shell of the Earth is sufficiently strong to do the work demanded of it. He observed in his first paper,<sup>1</sup> "It obviously follows that the outer shell exerts a squeezing force upon the interior, and by compressing the mass into a smaller volume increases its density." In my communication to this MAGAZINE<sup>2</sup> I pointed out that no tensile stress that the Earth's crust could stand would be sufficient to compress the materials of the interior of the Earth, stating in effect that if the outer shell is assumed to be 30 miles thick, and of the tensile strength of steel, it could not exert a pressure of half a ton per square inch upon the interior without fracturing.

Mr. Vaughan now says that he does not rely upon decrease of volume due to pressure, but "upon the *transference* of material from beneath a surface of great pressure to below a surface upon which the pressure is much less." This is not very different to what I understood of his theory from his first paper, and my calculation was given merely to show what an exaggerated view Mr. Vaughan held of the compressive powers of a contracting crust. Mr. Vaughan's theory, so far as I can understand it, appears to be this:—Mountain Ranges are produced by the differential tensile stresses of a shrinking crust causing a local flattening of the Earth's curvature, and thus compelling a flow of material from where the crust is strong enough to prevent, to where it is weak enough to permit of, bulging. Now, on the assumption that a shell of steel 30 miles thick represents the tensile strength of the contracting crust—an exceedingly liberal

<sup>1</sup> GEOL. MAG. 1894, p. 264.

<sup>2</sup> *Ibid.* p. 414.