

(4) Grooved and striated surfaces are preserved under favourable circumstances. A full description is given of a number of instances, the direction of the striæ being recorded, as well as the fall in feet per mile from the summit of the Beacons. The author in summing up his observations comes to the conclusion that the erratics in the Eglwysilan and Caerau group were probably, as a rule, transported by floating ice, but that some may be the relics of old moraines; that the Boulder-clay of South Brecknockshire is chiefly the product of land-ice; and that the striated rock-surfaces are in some cases the result of glaciers which have descended existing valleys. In other cases they may have been produced by an ice-sheet, which it is possible may have come from the N.W.

CORRESPONDENCE.

TEXT-BOOK OF GEOLOGY BY ARCHIBALD GEIKIE, LL.D.

SIR,—It is very far from my desire to write a word in derogation of the high merits which this work possesses; but I may be allowed to draw the attention of its author to a few points personally affecting myself, and to some extent my colleague, Dr. Rowney.

Dr. Geikie has incorrectly represented the view we hold as to the origin of the "canal system" of "*Eozoon Canadense*." Coupling our names with that of Professor Möbius, the readers of the *Text-Book* are informed that we "have endeavoured to show that the supposed canals and passages are merely infiltration veinings of serpentine in the calcite" (p. 639). This remark may be correctly applied to Dr. Möbius; but in none of *our* publications is there the least indication of any endeavour of the kind to be found. We have, on the contrary, maintained that the "canal system" has originated, in many cases, from the wasting action of carbonated solutions on clotules of "floculite," or, it may be, saponite—a disintegrated variety of serpentine, and, in others, from a similar action on crystalloids of malacolite ("white pyroxene"). In both cases there are produced residual "figures of corrosion," or arborescent configurations, having often a "regular disposition," even to the extent of a sheaf-like symmetry.¹ This, in which we certainly agree with Dr. W. B. Carpenter, is "quite unlike any mineral infiltration;" for, as stated in our *Old Chapter of the Geological Record*, p. xviii, we had palpable evidence that it had been determined by mineral cleavage—a divisional structure, which is essentially *regular* and correspondingly *symmetrical* in malacolite and its monoclinic allies.²

While on the subject of Eozoonal structures, it behoves me to mention that the *Text-Book* contains no reference to our views,

¹ Formerly, I held with Dr. Rowney, that the "canal system" is in general *irregularly* arranged; but having been lately favoured by Dr. Carpenter with an inspection of his numerous beautiful preparations, besides having examined some specimens that have lately come into my possession, I feel bound, and in agreement with my colleague, to give expression to the terms stated in the text.

² The mineral origin of the "canal system" is demonstrable by the clearest evidences, hereafter to be published, abounding in specimens of hemithrene, intermixed with ordinary gneiss, which I have lately received from Ceylon.

published at intervals running over sixteen years, on the origin of, and on changes in, serpentine, chrysotile, and other related minerals.

Nor is there the least notice of any of the phenomena which we ascribe to chemical changes (methylosis) in rocks.

My Memoir on Jointing and Slaty Cleavage, published in the Transactions of the *Royal Irish Academy*, vol. xxv. pp. 605-662, 1875, is altogether unnoticed.

I have in the last place to mention that all reference to my *Monograph of the Permian Fossils of England*, also some subjects introduced, are omitted in the *Text-Book*; although the latter contains (p. 752) some figures of shells copied from it, but without acknowledgment.

Under the conviction that Dr. Geikie doubtless desires his *Text-Book* to contain fair and correct references to the labours of his colleagues on the subjects he has touched upon, I am disposed to believe that he will not overlook these notes when preparing, as pretty certainly will be the case, another edition of his valuable work.

WILLIAM KING.

GLENOIR, NEAR GALWAY, Nov. 5, 1882.

THE RIGIDITY OF THE EARTH.

SIR,—In his letter in the November No., 1882, "On the Depression of Ice-loaded Lands," the Rev. O. Fisher laments the disagreement between the mathematical physicists and the geologists respecting the rigidity of the body of the earth. But if the earth have a *viscous* rigidity, there need be no incompatibility between their respective contentions. The character of a viscous solid is that, though it may seem to be quite rigid when tested by a short-lasting stress, it may be capable of yielding very considerably, in some cases almost indefinitely, to a much smaller stress continued for a sufficiently long time. Sir William Thomson's conclusion as to the steel-rigidity of the earth is founded upon the magnitude of the short-period ocean tides, classing with these even the monthly tides due to the ellipticity of the moon's orbit (he has given up the argument from precession). But he himself declared in his Address to the Physical Section of the British Association, at Glasgow, in 1876, that the absence of any indication of a 18.6-year ocean tide, depending on the revolution of the moon's nodes, could not be easily explained without assuming or admitting a considerable degree of yielding in the body of the earth. That is to say, our earth, as a whole, is a viscous, or practically viscous, solid, which, notwithstanding its apparently very high rigidity when tried by reciprocating stresses of short period, may be able to yield to the full satisfaction of geologists to a sufficiently long-continued pressure, such as that of a great depth of ice in high latitudes during the Glacial period. I omit some other considerations which go to strengthen this conclusion.

M. H. CLOSE.

UNIVERSITY CLUB, DUBLIN, Nov. 7, 1882.