

The principal discrepancy between our results is that the remarkable variety of enstatite, very hard and sometimes of a red-brown colour, which he describes as common in the deposits, is absent from all the collections which I have seen. Staurolite, on the other hand, which he does not mention, is very abundant. It occurs in worn crystals, black or dark brown, and with crystalline faces not good enough for measurement on the reflecting goniometer. The angles, however, agree well with those of staurolite. A curious fact is that no cross twins, so characteristic of this mineral, were observed among the crystals. Seeing that kyanite is so abundant, the presence of staurolite is exactly what might be expected. Mr. Menell is not disposed to agree with Professor Gregory that the diamonds may have been derived from pegmatites, but the conclusion to which I was led by an inspection of the minerals of the alluvial deposit was that they could have come only from the margin of a granite and a contact aureole. The whole paragenesis indicates this. Not one of the minerals I have seen resembles those of the Kimberley diamond pipes.

JOHN S. FLETT.

THE TRIMINGHAM CHALK.

SIR,—It seems to me also desirable to make a few comments on Mr. Brydone's letter in your November number. I did not "affect to regard" his note about his use of the magnetic for the true north "as addressed to myself personally." The communication, though it also expressed Mr. Hill's views, was written by myself to save time and trouble. Our remark was not intended as a criticism of anybody, but to explain why we had not altered the terms which we began to employ fourteen years ago, when our attention was concentrated more on the hypothesis advanced in the Geological Survey Memoir than on verbal details. We took those terms from the general direction of the coast, as shown on the Ordnance Survey Map, and I maintain that our practice, the statements in my note (except that the misprint, Weymouth for Weybourne, escaped correction), and my use of the word 'trend' are correct. The "trend of a coast" is not "ever varying from point to point and as you take it at the base or top of the cliff," but it expresses, according to Nares, Johnson, Webster, and others, the general direction, especially where there is a bending, of a coast, mountain chain, etc.

I never asserted the arch in my sketch to be identical with that to which Mr. Brydone referred in his papers. I said "the isolation of the more notable bluff is now complete," and gave a description of what then remained. My sketch and the photograph published by Mr. Hudleston in your November number exhibit the later stages of the work begun in October, 1905. My purpose in stating that a certain mass of chalk was a separate boulder was to imply, not that Mr. Brydone had denied this, but that the fact, under all the circumstances, diminished rather than increased the probability of a neighbouring mass being a seastack.

In regard to the line (*g*) in my sketch (the basement bed, according to Mr. Brydone, of the grey chalk) and my missing it on the opposite side, I can only plead the difficulty of seeing what one believes to be non-existent, but must confess that I did not understand him to mean that the *O. lunata* chalk had such a curiously irregular surface as he assigns to it in his last letter; that, however, in my opinion, only increases the difficulties in his hypothesis of an intra-Cretaceous unconformity. As this hypothesis appeared to me (as it still does) a fundamental one, and the other evidence insufficient to overcome its inherent improbability, I considered myself justified in limiting my criticism to the questions which lay within my more special field of work, and am now content to await further developments as the sea continues its inroads.

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THICKNESS OF LAND-ICE.

SIR,—I have just sufficient acquaintance with your reviewer of Chamberlin & Salisbury's Text-book, vol. i, to be able to discuss what was in his mind in penning the sentence to which Professor Schwarz takes exception in your November number, though I shall not venture to defend his gratuitous interjection of a reference to Professor Schwarz's views on the occasion in question.

Professor Schwarz claims that certain physicists have proved by calculation that ice cannot attain a greater thickness on the earth's surface than 1,400 to 1,600 feet, and with implicit faith in this calculation he seeks to reconcile the result with the geological evidence. The reviewer, however, probably lacking sufficient knowledge of physics to criticize the calculation, and being also doubtful whether the result is one on which all physicists are agreed, has fallen back upon the available geographical and geological evidence, and on this evidence alone has felt no hesitation in rejecting the postulated limits. He has, no doubt, considered that the Greenland ice-sheet, as described by Peary, must at its maximum far exceed the thickness allowed by these physicists; and he probably also still believes that the Antarctic ice in the valleys of the interior surpasses this limit, in spite of the ingenious argument of Professor Schwarz as to the progressive deepening of such valleys.

Then, as regards bygone glaciation, the reviewer perhaps remembered the glacial phenomena in British Columbia, where there is every indication that ice-sheets have filled pre-existing valleys to a much greater depth than 2,000 feet; and he may have recalled the conditions in the north-eastern portion of the United States, where the uplift of boulders in the Adirondacks, if due, as usually believed, to land-ice, must imply a thickness of ice on the Canadian lowland far exceeding the supposed limit.

Or without going so far afield, he may have had in mind the