that evaporation equals precipitation on the plateau leads one to infer that it is likely that the ice-cap there is quite thin. If the upholders of the 5,000 feet ice-sheet will produce the record of a tabular iceberg more than 1,600 feet thick, or if they will show that there is a valley 5,000 feet from crest to trough running under the flat ice-cap of Greenland—the observations at present available tending to show, on the contrary, that the deep valleys on the coast go but a short way inland and end abruptly on the edge of a plateau —then I will believe that physical laws have been suspended in their operation for the special benefit of glacialists.

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THE TRIMMINGHAM CHALK.

SIR,—It seems desirable to make a few comments on Professor Bonney's paper in your September number. On the question of "western and eastern" or "northern and southern" bluffs, I cannot see what the trend of the coast, ever varying from point to point and as you take it at the base or top of the cliff, can have to do with the relative position of two fixed points. A line drawn from bluff to bluff runs by the compass 5°–10° S. of S.E., so that I and any earlier writers who used *magnetic* bearings are accurate in speaking of "northern and southern" bluffs. Can it be that Professor Bonney is treating our magnetic bearings as if they were geographical, and supplying an instance of the very confusion I sought to forestall by a note obviously addressed to the general public. (Professor Bonney affects to regard it as addressed to him personally, but I can assure him that the paper by him and Mr. Hill gave rise to no alteration in the form or substance of mine.) On the East Coast it is in any case natural (and not inaccurate) to speak of points along the coastline as north and south, while they are no nearer due east and west than 10°–15° E. of S.E indicates.

I am less fortunate than Professor Bonney in having only found one place where the foreshore chalk has a skin of boulder-clay, the plastic clay having, under pressure from the cliffs above, crept over the chalk for a few yards in a depression. It seems a very natural thing to happen.

Professor Bonney has not fully grasped my theory as to his blocks A, C, and E. I believe that the eroded surface, unconformable to the lines of flint, of the *Ostrea lunata* chalk in these three blocks was formed in Cretaceous times, and then still in Cretaceous times the grey chalk was deposited on it, most thickly in the hollows, e.g. between C and E, and in the pocket in the seaward face of A shown by my figs. 13 and 16. On this theory no twisting of the grey chalk is required, nor is there any difficulty in its occurring still at the top and bottom of the sloping face of C. (As I have stated, it formerly covered the whole of this sloping face and was continuous *above* the sand with the grey chalk in E.)

I do not know with whom Professor Bonney is arguing that it is "more probable" that the thin slab is a separate boulder. I clearly stated this as my view (and see my fig. 17).

As to the grey chalk, I hold it to be of Cretaceous age because the soft matrix contains a pure Cretaceous fauna very abundant both in species and individuals, many of the perfect or well-preserved specimens being so delicate that the presence of one in a remanié deposit would be very remarkable, and the presence of two almost miraculous. Apart from the basement bed and the intimate mixture of very fine clay which causes the greyness, the grey chalk is absolutely pure throughout the thickness (maximum eleven feet) which has been exposed at the North Bluff. This makes a strong *primâ facie* case as yet unanswered. I have also good ground to believe that Clement Reid's sandy bed (which I have already found lying unconformably on *Ostrea lunata* chalk) is the basement bed of the grey beds which on the foreshore crop out from *under Ostrea lunata* chalk, and are not only identical with the grey chalk of the bluff in appearance, fossil contents, and peculiar flints, but are also, as I can now say, the only other beds in which I have found *Terebratula obesa, Ostrea iuæquicostata,* or *Ostrea canaliculata.* The significance of this is obvious.

Professor Bonney ignores the direct evidence as to the North Bluff and presumptive evidence as to the South Bluff that they are in direct physical connection with large masses of the foreshore chalk, and abstains from discussing any palæontological evidence or the behaviour of the foreshore chalk. This of course simplifies the criticism of a theory based almost wholly on those three classes of evidence, but which when formed proved capable of application to the special case of the North Bluff.

It may be well to take this opportunity to point out (to the general public) that the arch sketched by Professor Bonney was formed in March last, and is not the arch of *Ostrea lunata* chalk and grey chalk over a pinnacle of clay referred to in my paper, and which was broken through on the 1st October, 1905. His line *g* is the coarse basement bed of the grey chalk, the continuation of which on the opposite side of the arch, where it is less coarse, he has missed.

Mr. Jukes-Browne's letters leave untouched my original proposition that *Terebratulina striata* is the best zone fossil. I am also still unready to admit that it is logical to combine several beds easily distinguishable palæontologically or lithologically, often in both ways, in one zone on the strength of a common peculiar fauna, and then to name the zone from a fossil which is most capriciously restricted to some only of these beds. The characteristic assemblage of fossils he quotes in the Survey Memoir, vol. iii, p. 12, can only be obtained in the *Ostrea lunata* bands, if in all of *them.*

R. M. Brydone.

16, South Audley Street, W.
9*th October*, 1906.