and predecessor, Professor Phillips, the Drift beds of the neighbourhood of Oxford and the Thames Valley. Beyond the fact of the occurrence of flints in the Thames Valley, he was not aware of the widespread distribution of *chert* from the Lower Greensand on the higher hills of the Thames district, on which I mainly based my hypothesis of the Southern Drift. In fact, at that time, no one had recognised this chert *débris*, or if it had been noticed the fragments were spoken of as *Sarsen-stones*.

So far back as 1847, in "The Ground Beneath Us," after speaking of the flint gravel of the Thames Valley, I say: "It must have been from some distant spot that the materials of this gravel had been derived. . . . . The nearest place . . . . is in the range of hills passing by Croydon and Epsom, a distance of six to ten miles southward from Clapham." Again, "Whatever may have been the cause of this exceptional phenomenon the great and preponderating mass of flint *débris* from the Chalk hills, and of *sandstone* and *chert* from the Greensand hills of Surrey, leaves no reasonable doubt that the main bulk of the gravel of Clapham and of London has been derived and transported from the *Surrey downs* and *Sussex hills*."

Mr. Shrubsole sees a difficulty in the existence of a Wealden dome, which he considers open to question. But how, without higher ground than any in the Thames Valley, could *débris* from the Lower Greensands have drifted over the ground to the northward of it?

These remarks are not intended to convey any disparagement of Professor Phillips' excellent work, which I have often had occasion to study with advantage. JOSEPH PRESTWICH.

BOULDERS OF ELÆOLITE-SYENITE IN EAST YORKSHIRE.

SIR,—The absence of the well-known elæolite-syenite (laurdalite of Brögger) from the Norwegian boulders hitherto identified in Holderness has more than once been mentioned, and is cited by Sir Henry Howorth in your August Number as in some way supporting his theory that the boulders were brought artificially as ballast. Why laurdalite should be less suitable for ballast than laurvikite does not appear. The non-recognition of the former is, of course, easily explained by its resemblance to the latter, which occupies a much larger area in the Christiania basin, and is correspondingly more plentiful among the boulders. Nevertheless it is satisfactory to be able to record the occurrence of the Norwegian elæolite-syenite on the Holderness coast. Visiting Dimlington a few months ago, I selected from the profusion of syenitic boulders on the beach eight which seemed worthy of closer study. These and the slices cut from them are now before me. Two contain abundant elæolite, and are identical in every respect with specimens of laurdalite from its original home; one or two others have accessory elæolite and sodalite.

Since these boulders were collected on the beach, the facts mentioned do not appeal to those who find comfort in the ballast theory. Indeed, it is not easy to see how Sir Henry Howorth can

https://doi.org/10.1017/S0016756800143845 Published online by Cambridge University Press

be met at all. Passing over his travesty of my argument, which I suppose is not to be taken seriously, I find nothing in his article that does not evade the point. He first suggested that the records of Norwegian boulders on our East coast were due to observers having been deceived by material artificially transported. Thereupon I pointed out the well-known fact that such boulders are found imbedded in the Holderness clays, as well as on the beach. Of this he takes no explicit notice, but proceeds to shift his ground and throw doubt on the identification of the rocks in question. If Sir Henry will submit some of the disputed boulders to his eminent but anonymous petrological friend, the testimony of the latter will no doubt receive due weight; meanwhile, though a hundred witnesses may depose that they have *not* seen Scandinavian boulders in Yorkshire, the jury will listen rather to the evidence of one or two who *have* seen and investigated the matter.

St. John's College, Cambridge.
August 18*th*, 1894. Alfred Harker.

GLACIAL GEOLOGY.

Sir,—In your issue for August last Sir Henry H. Howorth, replying to a short letter in which I criticized an article of his you had printed, remarks: "I hope Mr. Harker and Mr. Deeley will continue to face the issues between us, and not be content, as others have been, with fulminating more or less testy protests, and then retiring from the field." It is clear that your correspondent here refers to a discussion which took place in the pages of "Nature" between himself and Dr. Alfred Russell Wallace. In that discussion Sir Henry H. Howorth, to discredit his opponent's views, denied the correctness of a statement made by Dr. Wallace. In reply, Dr. Wallace showed that Sir Henry H. Howorth had, in his "Glacial Nightmare," taken the same view as Dr. Wallace; and as Sir Henry H. Howorth would not admit that he had played fast and loose with his facts, Dr. Wallace very properly refused to further discuss the matter with him. Is it proper to call this retiring from the field with more or less testy protests? In my letter I charged Sir Henry H. Howorth with having misrepresented the teaching of two letters, one written by Prof. Bonney and the other by Prof. Hughes. In his reply we have no word of explanation or apology for this, or even reference to it; but instead an attack, delivered quite beside the mark, intended to throw discredit upon me. He also sneers at "English official geologists" as a body, so I can, at any rate, congratulate myself upon being in excellent company.

In face of the discussions which have already taken place—discussions in which it has been pointed out that, as far as is known, ice in bulk is plastic (that it has no *yield-point* in the sense that steel or even clay has, and that, therefore, so long as there is an upper slope to the ice the ice must move)—it would be useless to try to make the matter clearer in a letter. However, I will quote again from Sir Henry H. Howorth by way of illustration. He requires

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