

few, each of which was more than sufficient to dam up the drainage in a Highland glen.

Mr. Miller suggests that "when the mechanics of glaciers is better known," it will appear that the appetite of glaciers for digging will grow with what it feeds on. Possibly this may be so; but, as the present state of our information on the subject leads us to infer that the laws governing the movement of rivers of ice are similar to those which determine the flow of streams of water, it seems to me that our knowledge will have to be very much bettered indeed, before such a proposition stands the smallest chance of general acceptance.

JOHN W. JUDD.

#### ON THE SILURIAN ROCKS OF SWEDEN.

SIR,—In the August Number of your *MAGAZINE* there is a letter from Mr. Hicks directed against me, and though it is of little use to discuss with Mr. Hicks, who, instead of arguments, usually sets forth only assertions, I shall say a few words, in order to point out some mistakes and mis-statements in that letter.

When Mr. Hicks in the beginning of his letter says, "that Dr. Linnarsson is unable to put forward stronger evidence in opposition to these [Mr. Hicks'] views is clearly a powerful argument in my favour," I must remark that my letter in the June Number (of which here is the question) was not intended to be a critique of Mr. Hicks' views in general, but only to refute his reasonings in the letter inserted in the May Number. I think that every attentive reader will find that I have sufficiently shown their weakness, and then nothing more can be required.

Mr. Hicks now seems to hold it at least possible that the Paradoxides beds of Sweden represent also the lower parts of the Menavian beds, but then he adds, to my astonishment, that "there is no evidence of a previous fauna." One might have thought that Mr. Hicks, from what I have written in the April Number of the *GEOLOGICAL MAGAZINE*, would have known that there are below the Paradoxides beds two faunas, that of the Fucoid Sandstone, and that of the Eophyton Sandstone.

I doubt whether Mr. Hicks knows much more of the stratigraphical and palæontological characters of the oldest Russian beds. He himself says, it is true, that, with regard to the Russian beds, he has "looked to the general order of the deposits, and the general character of the fauna, for a clue." But I dare say that it is my opinions of their age, and not his, that are supported by "the general order of the deposits," and that hardly any one who has the slightest acquaintance with the Swedish and Russian beds can come to such conclusions as Mr. Hicks in this matter. From the *Orthoceras* Limestone down to the *Dictyonema* Schists inclusively, there is quite the same series of beds in the Swedish and the Russian area, and therefore it cannot be denied that the *Dictyonema* Schists of both areas are equivalent, "if the general order of the deposits," to which Mr. Hicks has himself appealed, is to be relied upon. As to "the general character of the fauna," I (and, I think, geologists in general) should

very much like to know which are the fossils of the strata underlying the Dictyonema Schists to which Mr. Hicks refers as being "Silurian in type and not Cambrian." To me the scanty knowledge of the fauna of these beds does not seem sufficient to warrant, *per se*, a conclusion as to their age. Fortunately, the sequence of the strata gives a good clue. The Dictyonema Schists of Sweden are very nearly related to the Olenus Schists, which they always immediately overlie, in some places seemingly without any sharp line of demarcation. The Olenus Schists of Sweden are exactly equivalent to the Lingula Flags of England, and therefore the rocks underlying the Russian Dictyonema Schists cannot, at least, be younger than the Lingula Flags, if we are to judge from the palæontological evidences, and from the sequence of the strata.

Mr. Hicks says that if I could only be brought to recognize his views of the gradual encroachment of the sea from a western or south-western direction over the European area, I should certainly feel none of those difficulties which now occur to my mind. It may be so, but it is my habit, in scientific investigations, first to make myself acquainted with facts, and then to proceed to generalizations. Even if Mr. Hicks may think it more proper and easy first to form theories, and then to "dispose of" the facts, so that they may not interfere with the theories, I cannot help thinking that the opposite way is the true one, and that the knowledge of facts must forego the forming of theories. As I—and probably also your readers—now begin to feel tired of the paper war with Mr. Hicks, I shall not hereafter pursue it.

G. LINNARSSON.

#### SECTION OF BOULDER-CLAY, NORTH DOCKS, LIVERPOOL.

SIR,—As a natural consequence of the two recent letters in the GEOLOGICAL MAGAZINE, in reference to the Boulder-clay recently exposed in the new dock works to the north of Liverpool, I beg to hand you a copy of the Report I sent, as a member of the Committee, to the Rev. H. W. Crosskey, F.G.S., Secretary of the Boulder Committee of the British Association.

"Recently very extensive excavations have been made in the Boulder-clay at Bootle, a northern suburb of Liverpool. The site is intended for new docks, and extends along the River Mersey, being reclaimed from the shore within the tidal range. The following is a section of the Drift deposits that have been exposed continuously over many acres; but the thickness of each varies considerably according to position, and the Middle Sands and Gravels often thin out and leave the Upper Boulder-clay reposing on the Lower Boulder-clay.

1. Sand and Silt of the old shore ... ..	17 feet.
2. Upper Boulder-clay ... ..	15 "
3. Middle Sands and Gravels ... ..	6 "
4. Lower Boulder-clay ... ..	6 "

(Bunter Sandstone)

"The whole of these subdivisions repose, each in succession, on the Bunter Sandstone at that part of the section nearest the old coast-line.

"The Lower Boulder-clay contains a much greater quantity of