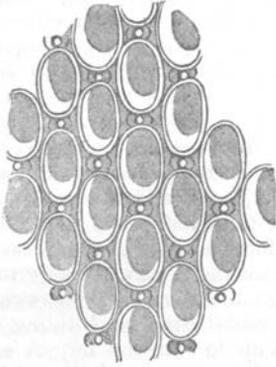


Mr. Macconochie obtained, some short time since, numerous specimens of a *Membranipora* from the Post-Tertiary beds (Carse Series) of the River Forth above Stirling. An examination showed that it was closely related to *M. Lacroixii*, but not quite identical. Examples were forwarded to Prof. Busk, F.R.S., who considers it a new species, and has done me the honour to name it after me. The following is his description:—*Membranipora Etheridgei*, n. sp.—



“Zoæcia disposed in regular quincuncial order. Apertures entire, oval or oblong. Border thin, finely granular, unarmed. Each aperture surmounted by a blunt tubercle. At first sight resembles *M. Lacroixii*, but differs in the perfectly regular disposition of the zoæcia and the presence of the single tubercle above each aperture. This tubercle does not appear ever to present the appearance of an opening. The growth forms lace-like expansions on the surface of shells.” *Loc. and Horizon*, Goodie Water, near its junction with the

Forth, etc.; River Forth near Mid Frew, and other localities in the neighbourhood of Kippen, Perthshire, in Carse shelly clays of the Raised Beach series. R. ETHERIDGE, Jun.

EDINBURGH, Oct. 11, 1876.

THE “SARSEN STONES.”

SIR,—With reference to the fact of the Sarsden or Sarsen Stones of Berkshire being perforated here and there with numerous vertical root-marks, as mentioned in No. 138 of *GEOL. MAG.*, December, 1875, p. 589, permit me to add that I have seen other such specimens since that date, and especially near Marlborough, and at Avebury in Wiltshire. Among the “Grey Wethers” on the Chalk Down, near the former place, I lately saw some good examples; and on a visit to Avebury, I particularly noticed that one of the enormous upright Sarsens, standing among the ricks of the farms, abounds with these once perpendicular rootlet-holes, together with numerous horizontal casts of stems and other plants-remains. T. RUPERT JONES.

YORKTOWN, Sept. 20, 1876.

FORMATION OF ROCK-BASINS.

SIR,—In respect to the challenge thrown down to me in your last Number by my friend Mr. Hugh Miller, I should have been very well contented to have left my defence in the hands of so able a champion as Mr. Bonney. But lest my silence should be interpreted as indicating a lack of courtesy as well as of courage, I respond to the personal appeal which is now made to me.

If Mr. Miller’s article is rightly understood by me, I gather, that while prepared to admit the overwhelming probabilities in favour of the view that the formation of the great Alpine lakes is due to the

combined action of subaerial erosion and subterranean movement, he finds two obstacles to his acceptance of the same explanation in the case of certain smaller lakes. Most happy should I be if any remarks of mine sufficed to meet the difficulties expressed by so candid a reasoner as Mr. Miller.

His first argument is propounded by Mr. Miller in the following terms: "It appears to me that no halting-place can logically be found by those who, with Sir Charles Lyell, allow only 'some mountain tarns' to Prof. Ramsay's demand for lakes; that either a glacier is inadequate to hollow even a tarn, or that it can enlarge it to any reasonable proportions." Need I point out to my friend that everything in this controversy depends on the sense that we are to attach to this highly elastic phrase "reasonable proportions." If any one, for example, were found bold enough to suggest that the rock-basins of the Black Sea, or the two occupied by the Mediterranean, or the larger ones which hold the Atlantic, Pacific and Indian Oceans respectively, were scooped out by glaciers (and really one would not be surprised at anything being claimed for ice-action at the present day), of course Mr. Miller would admit that these were *beyond* "reasonable proportions," to be produced by such a cause. Yet it would certainly be as easy to adduce a series of rock-basins, constituting an insensible gradation from a Scotch loch up to even the largest examples which I have cited, as it would be to construct a similar series downwards into the tiniest tarn. There is surely no more want of logic in stopping at one point rather than at another in this perfectly graduated series. My own faith in the powers of glacier erosion stops short of anything that, without flattery, could be called "a lake" at all; some are gifted with powers of belief that will embrace Cumberland meres and Highland lochs; more strongly constituted minds do not pause before an Alpine lake; and a few (but these must be inspired with "faith that could remove mountains") have claimed that Lake Superior itself may be "glacier-formed." I cannot help thinking that Mr. Miller will, on reflection, perceive the fallacy embodied in the oft-repeated assertion that, granting irregularities of surface may be produced under a glacier, all that is required for the production of the vastest lakes is a sufficient volume of moving ice. Does not a Highland burn wash out many a tiny basin under its waterfalls, and at other points in its course? Yet who would venture to assert that, because the Mississippi has many thousand times the volume of the Highland burn, it could make basins many thousand times as big?

In the second argument raised by Mr. Miller, great stress is laid upon the undisturbed condition of the so-called Cambrian beds of Sutherland. But while, as compared with neighbouring rocks, these appear "nearly horizontal," I have not the smallest doubt that when they come to be carefully studied in detail, their wonderful parallelism and regularity will be found—as in the case of the Alpine dolomites, which present quite as regular an appearance when seen from a distance—to be a mere optical delusion; and that these ancient masses have been subjected to flexures and fractures not a

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