the embayed outcrops abreast of the features on either hand, the basin is filled up, and the pre-existent surface approximately restored.

I believe that these are not isolated cases of rock-basins demonstrably eroded. Semerwater, in Yorkshire, which may be considered as an outlier of the Lake District proper, is seemingly bounded by strata nearly horizontal and obviously undisturbed, and might well form a link between the small Northumberland lakes and those larger basins of the origin of which no stratigraphical evidence seems obtainable.

The comparative infrequence of sharp synclinals even in some rocks of great age seems to militate strongly against the subsidence theory of lakes. The extraordinary regularity of the Cambrian Sandstones of the western districts of Ross and Sutherland I believe to be quite irreconcilable with the great numbers of rock-basins in these regions on any theory save that of erosion. From Suilven more than 200 lakes and tarns can be counted, yet the present surface is but one of many that may have existed during the progress of that denudation of which Mr. Judd's Scottish researches so eloquently tell. If the movements in the crust had been half as rife in past geological ages as Mr. Judd's views of lake formation require for the recent one, the Cambrians could scarcely have shown a uniform dip for ten yards together.

In conclusion, rock-basins I believe may be ranged under three heads. 1. Those demonstrably due to subterranean movements. 2. Those demonstrably due to erosion. 3. A large number of which the origin is, and in some cases may always remain, doubtful.

WARK-ON-TYNE,
May 6th, 1876.

HUGH MILLER,
H.M. Geol. Survey of England and Wales.

OLDEST FOSSILIFEROUS ROCKS OF NORTHERN EUROPE.

SIR,—In a letter inserted in the May Number of the GEOLOGICAL MAGAZINE, Mr. Hicks makes some objections to my paper on the Oldest Fossiliferous Rocks of Northern Europe in the April Number. There are especially two points in which Mr. Hicks thinks that my views are incorrect and endeavours to corroborate his own.

Firstly, Mr. Hicks attempts to demonstrate that the Swedish area cannot have been depressed at as early a period as the British, by reasoning as follows: The Cambrian and Lower Silurian rocks of Sweden have an average thickness of 1000 ft., while the British are 30,000 ft. in thickness; there is no reason why the Swedish area should not have been depressed at the same rate as the British; the lowest Swedish beds must, therefore, be younger than the lowest British, for, if that be not the case, we must suppose that before the close of the Lower Silurian period the Swedish sediments were deposited in a depth of 29,000 ft. of water, which is impossible, as the characters of the faunas indicate very similar conditions in the British and Swedish areas. If we draw the full logical consequences of this Mr. Hicks' reasoning, we come of necessity to the conclusion that all the strata which we have in Sweden been used to refer

to the various divisions of the Cambrian and Lower Silurian systems are nothing but equivalents of the uppermost 1000 ft. of the British Caradoc group. I think that Mr. Hicks himself will shrink from such a conclusion. But, if the conclusion is false, there must be some fault in his own reasoning, on which it is based. We must, therefore, suppose that the depression took place far more slowly in the Swedish area. I think that this must be so, because the Swedish area was more oceanic and more remote from volcanic districts, where it is natural that more sudden depressions take place.

Secondly, Mr. Hicks objects to my proposal to unite the Upper Harlech beds with the Menevian group. As to this, I will not deny that it may be convenient to separate them as local groups, but I still hold the opinion that the difference, in palæontological respect, between the Harlech and the Menevian group, is not comparable, for instance, to that between the Menevian group and the Lingula flags, or to that between the Lingula flags and the Tremadoc group. If the term Menevian were to be transferred to other countries than Britain, I think that it ought to have its range extended so as to comprise all the strata of which Mr. Barrande has formed his *Phases à Paradoxides*.

As to Mr. Hicks' general assertion, in the beginning of the letter, that the facts brought forward by me, far from invalidating any of his views, tend strongly to confirm them, I look forward to the continuation of his paper in the Geological Magazine, where the meaning of these words will probably be more fully explained. At present, I must own that I cannot understand how, for instance, the fact that the lowest Russian beds underlie a horizon which in palæontological and stratigraphical respect corresponds to the British Lower Tremadoc (or, according to the classification of Mr. Salter, to the uppermost Lingula flags) can very strongly confirm Mr. Hicks' opinion thas they are of Arenig age, nor how the fineness of the sediment in the Paradoxides and Olenus beds of Sweden can prove them to be shore deposits as they ought to be, at least for a large part, according to Mr. Hicks' views—not to refer to any more examples.

G. Linnarsson.

Stockholm, *May 15th*, 1876.

P.S.—Erratum.—There is an Erratum in my last communication which appeared in the Geological Magazine for April last at p. 149, 20 lines from foot of page, for "older than any metamorphic rocks of Scandinavia," etc., it should read "older than any *non*-metamorphic or '*clastic*' rocks, etc.—G.L.

Conchoidal Fracture of Flint.—It may be interesting to notice in connexion with this subject, alluded to in the Report of the Geological Society of Stockholm, (see *ante* p. 281), that the late Mr. C. B. Rose, F.G.S., read a paper thereon at the Meeting of the British Association at Norwich, 1868. The structure is well known in the Chalk Flints of Norfolk, and is developed by hammering. The "dressed" surfaces of old flint walls, Bishop's Palace Garden, Norwich, show it admirably well.—Edit. Geol. Mag.