

Geologists will probably look to the author to show that, as he has proved the inadequacy of the contractional theory, as usually understood, so he will establish by some calculation, based on probable values of the thickness of rocks affected, and of the changes of density due to hydrothermal action, that the inequalities of the earth's surface are not of greater magnitude than his theory would account for.

CORRESPONDENCE.

THE SILURIAN ROCKS OF THE LAKE DISTRICT.

SIR,—My time at present is too much occupied with my official duties to answer Mr. Hicks's letter in the last *GEOLOGICAL MAG.* at any length. Besides, the results of my survey of the Silurian Rocks of the Lake District must be recorded on the maps and in the memoirs of the Government Geological Survey. But I should like to know from Mr. Hicks if it is from the fossil evidence or from a careful stratigraphical survey of the rocks of the Lake District that he speaks so confidently of their position. If from the former, there is the danger of arguing in a circle. Certain beds were supposed to be Lower Silurian, therefore the fossils collected from these beds were considered Lower Silurian fossils; and so now these beds are called Lower Silurian, because they contain Lower Silurian fossils. But if a careful stratigraphical survey went to show that these beds were connected with the Upper and not the Lower Silurian, the fossils in these beds would no longer be characteristic of Lower Silurian.

I feel confident that the Stockdale shales of the Lake District are the equivalents of the Tarannon shales of Wales, and the Coniston grits and flags equivalent to the Denbighshire grits and flags, and that the Tarannon shales of Wales overlies the Llandovery rocks.

H.M. GEOL. SURVEY, KENDAL,
July 8, 1876.

W. TALBOT AVELINE.

THE EROSION OF LAKE-BASINS BY GLACIERS.

SIR,—Some of Mr. Fisher's interesting remarks on the excavatory action of glaciers seem to call for a few words of reply. It is quite true that the "possibility of the erosion of a great lake by a glacier has not been disproved," and that observations made on the Swiss glaciers now cannot do it; but these observations (and that is all I have ever contended) may render the theory probable or improbable. I doubt whether, in the sense in which we both should use the word, *proof* would ever in any case be possible. Mr. Fisher, however, seems not to have apprehended the point on which I have always laid most stress in my reasoning (which did not fall within the scope of my last communication to you), viz. that the great majority of Alpine valleys show no tendency to lake-basins, in places near, but above, the present lakes; where the conditions of glacial action must have been as nearly as possible the same as over the area of the basin, that is to say, that if you ascend a valley from the head of a lake, you often find it throughout a true valley of

river erosion, only very superficially modified by glacial action. Surely the glacier would have "tried its prentice hand" now and then before, for example, excavating Como.

I demur to Mr. Fisher's statement that if a glacier would be competent to deepen a lake basin, it could no doubt originate it. This may be only to say, "if a thing can be done under very favourable conditions, it can be done under all conditions," which, as it seems to me, is not a safe conclusion. Besides, if a basin exists into which the glacier descends, *What made the basin?* I may grant that a whetstone sharpens a razor, but doubt whether it is usually the tool with which razors are made.

With regard to the latter part of Mr. Fisher's letter, the hypotheses which he advances are such as it is almost impossible to disprove; for it is very difficult to understand what would be the procedure of the subglacial water in a lake-basin. I do not, however, think that under the circumstances there would be much abrading action exercised by the water (below the level of the rim of the basin) which is passing between the rock and the ice. A subglacial stream usually either drills out a tunnel through the ice or furrows a channel in the rock below, so that its erosive action is limited to a small area; what it would do in the case of a lake-basin I can hardly say. Possibly it might continue to act in the same way, but if it did not, and a layer of water were introduced between the rock and the ice, throughout the basin, I imagine there would be little motion in this, and it would rather be unfavourable to denudation and favourable to accumulation of sediment.

With regard to Mr. Hugh Miller's letter, I may remark that he misses the point that I, and I think that I may venture to say my friend Mr. Judd, have always maintained—viz. that because glacial erosion may seem the simplest explanation of certain *tarns*, therefore it is to be applied to certain *lakes*. Further I may remark that the infrequency of *sharp* synclinals does not militate against the subsidence theory of lakes. Those who uphold this theory do not require sharp synclinals, as Mr. Miller will find if he will draw the lakes on a true scale. He has forgotten an argument often used by his friends.

T. G. BONNEY.

ST. JOHN'S COLLEGE, CAMBRIDGE, June 12, 1876.

TRUE AND APPARENT DIP.

SIR,—Mr. Hill is a decided improvement on Mr. Penning, and I think I can improve a little on Mr. Hill. There are many excellent geologists to whom Trigonometry is a sealed book, and who not unnaturally look upon sines and cosines, tangents and cotangents, with a mixture of suspicion and dislike. But all geologists know what is meant when the dip of a bed is said to be 1 in 6; and it will remove the alarm which trigonometrical symbols are apt to raise in the minds of non-trigonometrical geologists, to be told, that, if a bed dips 1 in 6, the cotangent of the angle of dip is 6.

Bearing this in mind, Mr. Hill's construction may be thus simply expressed:—