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:—

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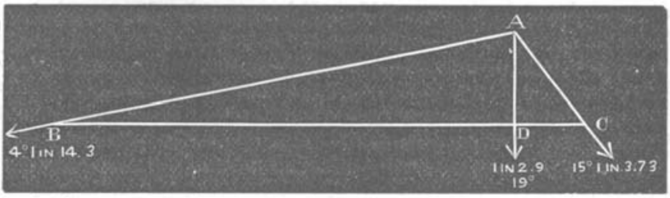
Let the dips observed along two lines *A B*, *A C*, Fig. 1, be 1 in *m* and 1 in *n*; make *A B m* units, *A C n* units in length; join *B C*, and draw *A D* perpendicular to *B C*.

Then *A D* is the direction of the full dip, and if *A D* contain *d* units of length, the full dip is 1 in *d*.

Fig. 1.



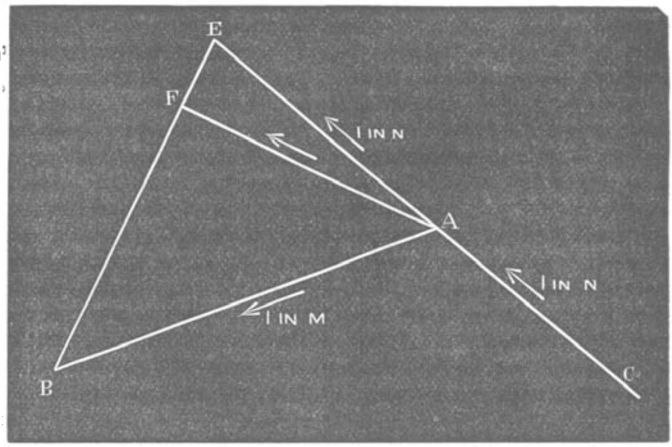
A similar construction will give the apparent dip in any direction when the full dip is known. Let *A D* be the direction and 1 in *d* the amount of the full dip; *AB* the direction in which the dip is required.

Make *ADd* units in length, draw *DB* perpendicular to *AD* meeting *A B* in *B*.

Then if *AB* contain *x* units of length, the dip along *AB* is 1 in *x*.

If the two dips, instead of being both towards or both away from *A*, be one towards and one away from *A*, produce one of the lines *AB* or *A C* to *E*, Fig. 2, make *A B*, *A E*, *m* and *n* units in length, draw *A F* perpendicular to *B E*, and *A F* will give the direction and amount of the full dip as in the first case.

Fig. 2.



For small angles the value of the dip expressed as one in so many which corresponds to a given number of degrees, may be obtained approximately by finding how often the number of degrees is contained in 60. In the table below the first column gives the value correct to two places of decimals for the angle opposite; in the second column are the values given by the above approximate rule; in the third the angle to the nearest minute that corresponds to the values in the second; and in the fourth the error committed by following the approximate rule. It will be seen that for angles less than 15° this error is always less than one degree.

	(1)	(2)	(3)	(4)
	1 in	1 in		
1°	57·29	60	58′	2′
2°	28·64	30	1° 55′	5′
3°	19·08	20	2° 52′	8′
4°	14·30	15	3° 49′	11′
5°	11·43	12	4° 46′	18′
10°	5·67	6	9° 27′	33′
15°	3·73	4	14° 2′	58′
20°	2·75	3	18° 26′	1° 34′
25°	2·14	2·4	22° 37′	2° 23′
30°	1·73	2	26° 33′	3° 27′
35°	1·43			
40°	1·19			
45°	1			

Explanation.—3° is actually 1 in 19·08; the approximate rule gives it 1 in 20; 1 in 20 is 2° 52′; error from following approximate rule 8′.

In Fig. 1 the dip along *AB* is 4° or 1 in 14·30, the dip along *AC* is 15° or 1 in 3·73: *AB*=1430, *AC*=373, dropping the decimals. *AD* is found to be 290. Hence the full dip is 1 in 2·9 or 19° very nearly. A. H. Green.

Leeds, *July* 13, 1876.

MR. MILNE ON FLOATING ICE.

Sir,—When comparing the altitude of an iceberg above water with the depth immersed, Mr. Milne has not sufficiently considered the conditions of stable equilibrum. A berg of the shape figured on page 307 could not remain in that position, but must turn over. That this would be the case may be seen by placing a boxwood tetrahedron (out of a set of models of crystals) in water, where it will float only with one of its *angles* downwards.

The position of stable equilibrium depends on the shape of the floating body, and on its specific gravity. The specific gravity of boxwood being about 0·95, is so nearly the same as that of ice, that the positions assumed by a floating mass of either substance will as a rule be almost identical. O. Fisher.

THE OLDEST FOSSILIFEROUS ROCKS OF NORTHERN EUROPE.

Sir,—The evidence brought forward by Prof. Linnarsson in the June Number of the Geological Magazine, as being opposed to the views advanced by me, has been already disposed of to a great extent in the papers in which these views have been propounded.[1] That Prof. Linnarsson is unable to put forward stronger evidence in opposition to these views, is clearly a powerful argument in my favour, and I doubt whether he would have raised the objection at all had

[1] Quart. Journ. Geol. Soc. vol. xxxi. p. 552 *seq.*; Geol. Mag. Dec. II. Vol. III. Nos. IV. V. VI.