

therms) must move upwards into new deposits as they are laid down. This is duly acknowledged in chap. vii. and credited to Babbage, who, I believe, has priority; but should the use of a natural law in building up a theory disentitle the theoriser to the full right of property in his own theory?

It is said, "What's in a name?" To which I answer, A great deal that is bad when it is vague and misleading and perpetuates errors and misconceptions.

T. MELLARD READE.

PARK CORNER, BLUNDELLSANDS, Feb. 9th, 1891.

#### CRINOIDAL STEMS IN ORDOVICIAN OF SWEDEN.

SIR,—I am glad to have elicited from Dr. Holm such interesting information about the crinoid stems that he has found in the Leptænakalk of the Lissberg. But I am sorry that my remark has given rise, perhaps not unnaturally, to some misunderstanding. Dr. Holm in his original notice says two things:—First, that the rock is "chiefly composed of corals, cystids, and crinoid stems;" secondly, that he himself has found there "erinoid stems belonging to at least two species." Now I never doubted that so experienced a palæontologist as Dr. Holm had very good reasons for this latter statement; his letter shows how sufficient those reasons were. Nor did I "without having seen a single one of them," venture to assert that all the stem-fragments belonged to Cystidea. I was indeed well aware of the sessile nature of the majority of the Cystidea from this locality. But, remembering as I did how often stems undoubtedly cystidean had been referred to Crinoids, and knowing that not a single Crinoid had been recorded from the Ordovician of Sweden, though 23 species of Cystidea showed the possibility of their preservation, I merely wished, as indeed I still wish, to suggest that some of these ossicles might have pertained to the long and exceedingly crinoid-like stem of *Caryocrinus*. So inevitable did this seem that, though I did not so far forget either myself or Dr. Holm's very valuable works as to call him a mere collector, still I did express myself in a manner which now seems to me to need an apology, and this I trust, Sir, you will here permit me to offer.

F. A. BATHER.

5 Feb. 1891.

#### MOTION OF LAND-ICE.

SIR,—As I have paid some attention to Glaciers,<sup>1</sup> I should like to make a few remarks on the paper by Mr. Goodchild on "The Motion of Land-ice" in the *GEOL. MAG.* for January last, pp. 19–22.

1. The expansion and contraction of ice for changes of temperature below 0° C. and under a pressure of one atmosphere is but an example of the general law for solids, which has been recognized for many years in physical science; and the power of ice to resist tensile strain is (as Helmholtz has pointed out) so small as to furnish an explanation of the formation of crevasses, though these are not by any means always produced by contraction due to lowering of temperature. But this very property of ice shows that contraction

<sup>1</sup> Q.J.G.S. vol. xxxix. pp. 62–71, "On the Mechanics of Glaciers"; also 'Nature,' vol. xxvii. pp. 553, 554, "On Solar Radiation and Glacier-motion."

of the upper layers can neither help (*per se*) the downward flow when the temperature rises again, because the expansion being equal to the previous contraction (*cet. par.*), it must result merely in the closing-up of the shrinkage-cracks; nor can it tend to produce a curvature of the lower layers (after the fashion of the balance-wheel of a chronometer) as seems to be suggested later on in Mr. Goodchild's paper. In the former case water, produced by the melting either of the surface of the glacier or of the snow-mantle overlying it, during hours of sunshine, and flowing into the cracks, would certainly by its expansion in freezing do some work; but what becomes of its *latent heat*? The distribution of this in the neighbouring ice needs to be considered.

2. If by 'cold-waves' Mr. Goodchild means (as I take it) 'flows' of heat by conduction from the warmer interior to the surface whose temperature is below 0° C., it is a pity he did not speak of it as such. Cold, like darkness, is a negation; and we can only speak of a wave of either metaphorically.

3. We had no need to go back nearly half a century to Brunner's investigations to convince us that the 'sole' of a glacier moves down a slope; the observations and measurements by Tyndall, Helmholtz, Forbes, Agassiz, and others have made that pretty certain.

4. As to the "uphill movements" postulated by Mr. Goodchild and many writers who have preceded him, I have for a long time been very sceptical, as may be seen from my papers referred to above; and the researches of Penck<sup>1</sup> on the glaciation of the Northern Alps have converted that scepticism into positive disbelief, since all that I have read or heard alleged, as evidence of such movements, is more rationally explained by the *overflow* of the glaciers beyond their valley-sides during periods of maximum glaciation, and by the phenomena of stranded lateral moraines during the recession of a glacier, with which every Alpine observer is familiar (*e.g.* the Morteratsch).

5. The theory of "isogeotherms" continued through the lower parts of a glacier will not work, except for a hypothetical case where the adjacent rocks and the ice were all considerably below 0° C., because no heat at any higher temperature could be conducted through or into the ice, as seems to be imagined: it would become latent in the melting of the ice at the contact. This is probably the reason why previous writers have "overlooked" the fiction which Mr. Goodchild has now gravely put forward as a "fact."

6. Mr. Goodchild has overlooked three factors essential to the construction of any sound physical theory of glacier-motion; (i.) liquefaction under pressure and regelation; (ii.) the "greenhouse-principle," the application of which to glacier-motion was demonstrated by myself in the paper in *Nature* already referred to; (iii.) the latent heat of water, the 80 gramme-units of heat given up by every gramme of water at 0° C. in the act of solidifying under ordinary pressure.

A. IRVING.

WELLINGTON COLLEGE, BERKS, Jan. 8.

<sup>1</sup> See "Die Vergletscherung der Deutschen Alpen," reviewed in the *GEOL. MAG.* Dec. II. Vol. X. p. 174, *et seq.*