evidence of the fact might be found in excavated moraines, such as has been described in the Nelson Province. Be this as it may, the cessation of the work of piling up the terminal moraines clearly points to the occurrence of marvellously sudden ameliorations of climate. The absence of marks of slow retreat—in morainic matter—of the ice from the lowlands appears to tell a similar tale of an earlier age. How such sudden changes were brought about is a question on which I can offer no opinion; but I think the condition of the alpine lakes, with their high and steep moraines, clearly proves that the ice retreated from them with great suddenness. If this were so, as it appears, the lesser changes registered by the loess may likewise have been sudden changes—that is, occupying but a brief time compared with the duration of each fixed phase.

Art. XXXII.—*On Glacier-motion.*

By J. Hardcastle.

[*Read before the Philosophical Institute of Canterbury, 2nd October, 1890.*]

The latest authoritative deliverances on the subject of the mode of motion in glaciers of which I am aware state that "the problem of the cause of glacier-motion cannot yet be considered to be satisfactorily solved," and "the solutions accepted are not perfectly satisfactory." Whilst endeavouring some time ago to work out a particular case of the problem, using as a principal factor a physical property of ice which underlay some interesting experiments of Professor Tyndall's—viz., its plasticity under pressure—I obtained what appeared to me to be a full, clear, and simple solution of the whole problem of ice-motion. When, however, I again referred to articles on the subject I found that my solution did not fit the alleged facts to be explained, in one important particular. It is asserted that "the top of a glacier moves faster than the bottom." The conclusion at which I had arrived was generally incompatible with this. There is no ground for impeaching the correctness of the observations from which that generalisation was drawn, yet the generalisation may be erroneous. It may be true of a part or parts only of a glacier that the top moves faster than the bottom; and, if this is so, a true theory, in order to explain those observations, should show to what limited extent, and under what circumstances, the surface of a glacier does move faster than the bottom.

Professor Tyndall's experiments, to which I have alluded, show that wet ice—that is, ice at and near its melting-point—behaves, under sufficient pressure, as a plastic or quasi-fluid substance. The ice at the bottom of a glacier is under both conditions—it is wet, and under pressure. I do not know what pressure is necessary to cause plastic flow in ice perfectly free to move, but it is not very great. Suppose we say, for the purpose of illustration, it is 100lb. per square inch, equal to the weight of a column of ice, say, 300ft. high. If, then, we have a portion of a glacier 400ft. deep, the base of which is perfectly free to move, the lower 100ft. will be forced to flow away by the weight of the 300ft. above. Supply a resistance to movement everywhere equal to 100lb. per inch, and the glacier must be 700ft. deep in order that the lower 100ft. may be squeezed away.

It appears to me that, taking the glacier as a whole, or any average cross-section of it, the ice at the bottom flows plastically under the weight of the ice resting upon it. In flowing it will obey, however tardily, the laws of hydrostatics, flowing from a region of greater to one of lesser pressure, and, obeying also the law of gravity, will flow preferably downhill. In other words, the glacier and the *névé*, or icefield, each consists of two mentally separable portions, moving in distinctly different ways. The lower portion is caused by the weight of ice above it to move as a viscous fluid; the upper portion remains solid, and is borne along by the living stream beneath, just as a mass of drift-ice or of logs is borne along by a river of water.

It is stated that "the surface-phenomena resemble those of a solid in a state of flow." But every description I have read (it has not yet been my good fortune to see a glacier) shows that this is true only of those portions of the surface which are encountering obstructions. Elsewhere the surface-phenomena, as described, are those of a solid in a state of floating transport. Crevasses indicate a spreading of the floating load, due to a spreading of the stream beneath: they are closed by subsequent compressions of that stream. Where the surface-ice encounters obstructions, such as a narrowing of the channel, a sharp bend in it, or an island, the force of the streaming ice beneath sets up horizontal pressures in the surface sufficient to produce plasticity in the ice immediately obstructed. All the described peculiarities of surface-movement are explicable as the consequences of the motion of a stream-borne load of a substance hard and brittle when free, but plastic under pressure.

According to this view it cannot be generally true that the surface of the glacier moves faster than the bottom. Nevertheless it must be true of a certain part or parts of each

glacier. Glaciers either thin out near their terminations, or are dammed by high moraines. In the former case certainly, in the latter case usually, the lower part of the glacier will not have weight enough to maintain its base in a hydrostatic condition. The terminal end of a glacier is therefore a stranded solid, which acts as a dam to the flowing and floating ice above. The pressures which these exert upon the dam may be considerable, and, as the upper part of the dam will yield most readily, it will be pressed forward over its base. In respect of the stranded terminal portion of the glacier, then, it is true that the top moves faster than the bottom. Similarly, and for the same reasons, it is true of any other portion of a glacier which is "stranded," as on a shoal, or where it approaches a fall. But it is clear that such portions can form no large part of the total length of a glacier. Is it too much to assume that the observations from which it has been inferred that the upper strata move faster than the lower were made on some stranded portions of glaciers? I think not; for naturally more importance would be attached to observations which gave a positive than to those which gave a negative result. There is one observation by Principal Forbes which must always have been given great weight, in which he found a considerable acceleration of the upper ice in a vertical side of a glacier exposed as it flowed past a cliff. This, however, can be explained otherwise than by a general law that the surface moves faster than the bottom; and must be otherwise explained, since it is nearly everywhere seen that the surface does *not* flow, and that forces cannot be found to make it do so.

No theory of ice-motion which assumes greater mobility of the surface as a normal condition will explain the scooping-out of rock-basins or fiords. The theory here offered explains it readily, as it transfers the scene of greatest activity to the base of the glacier, and the deeper the ice the more energetic will be its action on the rock beneath.

So far as I can judge, this theory "fits all the facts." It may be summarized thus: Glaciers and icefields flow through the lower portions, being reduced to plasticity or quasi-fluidity by the weight of the upper portions, and the former in flowing away bear the latter with them. The pressure necessary to effect such reduction at any point, and therefore the critical depth of the ice at that point, depends upon the sum of resistances to hydrostatic movement at the base — chiefly upon *distance to a point of no resistance*, gradient of bed, and *amount of obstruction* presented by the form of the channel or course of flow.