From the Author

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NOTE ON

THE OCCURRENCE OF ERRATICS

AT HIGHER LEVELS THAN THE ROCK-MASSES FROM WHICH THEY HAVE BEEN DERIVED.

By JAMES GEIKIE, F.R.S.E., F.G.S.,

Of H. M. Geological Survey of Scotland.

(Read 11th Dec., 1873.)

AIRD AND COGHILL, GLASGOW.

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Among the many puzzling phenomena which glacialists have endeavoured to explain, not the least perplexing is the occurrence of loose erratics and perched blocks at considerably higher levels than the rock masses of which they once formed a part. Such erratics have been noted, not only in this country, but in glaciated regions elsewhere; perhaps the most remarkable examples being those which Scandinavian geologists have made known to us. Mr. Törnebohm, of the Geological Survey of Sweden, informed me that he had found blocks at a height of 4500 feet, which certainly could not have come from any place higher than 1800 feet. Mr. Darwin, many years ago, attempted to explain such anomalies in the glacial phenomena of our own country, by supposing that the erratics had been floated about on massive icebergs during a period of depression. He conceived that, as the land sank down, the bergs stranded or dropt their stony burdens at higher and higher levels of the drowning country. This ingenious explanation compels us, of course, to make one of two assumptions—namely, either that the movement of depression was rapidly effected, or that the icebergs were endowed with extraordinary longevity. Or, again, we might combine the two assumptions, and hold both that the downward movement was rapid, and that the

icebergs took long years to melt away. I fear, however, that the theory is more ingenious than satisfactory. There are many considerations that militate against it, among which I shall only mention this, that we have no evidence of such a degree of submergence having obtained either in this country or in Scandinavia as would be necessary to account for the elevated position of many of the erratics in question. There are no doubt other objections to Mr. Darwin's explanation that will occur to a geologist familiar with the phenomena of glacial deposits in this country; but it is hardly necessary, at least for my present purpose, to specify the reasons that have led me to the belief that another explanation of the difficulty must be sought for.

The opinion held by Scandinavian geologists, and by many glacialists in this country, with whom I have conversed upon the subject, is that the erratics under review have been deposited in their present positions by land-ice. No one, however, has yet succeeded in showing how land-ice could have carried these large blocks from lower to higher levels. It is obviously impossible that the boulders could have travelled upon the surface of the ice in the usual way, nor is it credible that they could have been pushed on underneath along with the *moraine profonde* or till—for frequently they show no marks of abrasion or striation, but are in all respects similar to the large angular blocks that fringe the sides of Alpine glaciers.

Recent researches of my own having directed my attention more particularly to this curious problem, I am induced to believe that an explanation of the difficulty may eventually be found in certain phenomena which have been observed in Alpine glaciers. Among the late Principal Forbes' numerous letters on glaciers, addressed to Professor Jameson, there is one bearing date 12th December, 1846, in which some remarkable observations are recorded. I am not aware that the observations to which I am about to refer have ever been considered by geologists, nor do I know that any physicist has attempted another explanation of the phenomena than the one which seems to have satisfied Forbes. The phenomena in question relate to "the supposed tendency of glaciers to reject impurities, and the undoubted fact that stones are always found near or upon the surface of the ice." "It is strange," says Forbes, "that it should not have occurred to every one who sought to explain the appearance of stones on the surface by the *ablation* of the ice, that

in order to arrive there at all, the blocks must previously have been embedded in the virgin ice, where popular belief, and, generally speaking, more accurate observations also, give them no place." Forbes then proceeds to give several examples of the phenomena, which I cannot do better than quote in his own words. Referring to the extruded stones on the Glacier du Nant Blanc, near Chamouni, he remarks that "the right bank of this glacier is at first bounded by rocky summits, but in the lower part of its course by a mound-like moraine of the usual form. The surface-blocks can only be derived from the precipices near the origin. Yet they do not even appear on the surface opposite the rocks, but only opposite to the moraine; and they increase in number and quantity towards the lower end of the glacier, where they almost blacken the surface of the right side, the left side remaining almost clean. It is difficult to believe that this accumulation is not due to the gradual denudation of the blocks by the melting of the ice in which they have been in some way or other embedded; but it is scarcely less difficult to admit that having fallen from the rocks above the *névé*, they should have remained unperceived in the ice during all the intermediate space.

"To take another example. The Glacier of the Rhone is distinguished by the extraordinary purity of its surface, and the consequent absence of lateral moraines. But this general freedom from stones on the surface is subject to one exception which is remarkable. *Stones begin to appear at the surface on the terminal slope at a considerable height.* How came they there? Not a stone the size of the fist can be seen on the surface farther up; and in examining a number of the crevasses I could not see any engorged in the ice. The explanation seems to be, that these stones are actually introduced into the ice by friction at the bottom of the glacier, and forced upwards by the action of the *frontal resistance* which produces the *frontal dip* of the veined structure, and they are finally dispersed on the surface by the melting of the ice."*

This explanation is of course bound up with the "viscous theory," and those who hold that the subsequent researches of Dr. Tyndall, and other physicists, have overturned Forbes' theory of the origin of the ribboned structure of glaciers, may be inclined to put these observations aside. But the facts recorded by Forbes

* *Edinburgh Philosophical Journal*, January, 1847.

have an interest for geologists apart altogether from their bearing on the cause of glacier motion. Whether, as Forbes supposed, stones are carried upwards through the solid mass of a glacier by the filamentary sliding of the particles of ice—the curves of ejection corresponding exactly with those of forced separation—is a question for physicists to consider. It is enough for the geologist to know that stones apparently do travel upwards through the ice, nor will *ablation* alone suffice to explain the fact. If, for example, a boulder which has fallen into a crevasse and become embedded at a depth of 80 or 100 feet, should reappear upon the surface of the glacier at a point farther down the valley, it is evident that its reappearance at a relatively higher level in the glacier cannot be due to the melting of the ice alone. At one place it lies at 80 or 100 feet below the surface, but after the glacier has flowed for some distance, it reappears at the surface; that is to say, it seems to have risen 80 or 100 feet through the ice. If 80 or 100 feet of ice have been removed by melting and evaporation, then this loss has been again made good, but not by the accumulation of snow upon the surface of the glacier—the ice has been kept at its general level by the new supplies which are continually pouring into the valley at its origin. In other words, the ice in front rises just as fast as the ice advances from behind; it is thus that the general level of the glacier is maintained. Now the line of ejection along which the boulder has travelled cannot possibly dip at the same angle as the slope of the valley; for if that were the case, the boulder would always remain embedded in the ice, at the same distance from the underlying rock, until it was ejected at the terminal front of the glacier. But although the line of ejection must always dip at a less angle than the valley, yet it will in most cases still have a more or less perceptible dip in the same direction as the valley. A great deal will depend upon the boldness of the obstructions that impede the flow of the glacier. If these are numerous and formidable, then the lines of ejection will approximate more and more to the horizontal, and even at last curve upward, so as to dip *up* instead of *down* the valley; and thus boulders introduced into the ice at a given point will, as they are borne down the valley, not only rise as it were through the glacier, but eventually be extruded at a level which may be many feet, or even many yards, higher than the point in the valley from which they originally started.

It seems to me that such must be the case, no matter what theory of glacier motion we hold. Forbes may be wrong in having maintained that the curves of ejection correspond with those of "forced separation," and his explanation of the veined structure may also be entirely wrong, although this can hardly be said to have been established, for not a few of the objections to his theory proceed upon the assumption that "the fracture and regelation theory" of glacier motion is demonstrably true. Tyndall has shown that the ribboned structure is due to pressure, and this may be freely admitted by those who may still cling to the "viscous theory." It remains to be proved, however, that there is no such filamentary sliding of the ice particles over each other as Forbes contended for. The experiments of the late Canon Moseley seem indeed to cut away the legs of both Forbes' and Tyndall's theories, but the molecular theory recently advanced by Mr. James Croll obviates Canon Moseley's difficulties, and may eventually show us that Forbes was not so far wrong when he insisted that there is a motion of the ice particles parallel to the direction of the blue veins in glaciers.

But however this may be, it is enough for me, as a geologist, to know that stones introduced into the body of a glacier, whether from above or below, tend to rise upwards in the ice, as the glacier flows on its way. And I now ask whether this fact does not throw some light on the curious problem which forms the subject of this communication? Let us picture to ourselves a mountainous country, such as Scotland or Scandinavia, covered with a wide ice-sheet, or series of confluent glaciers, and endeavour in imagination to follow the course of some hypothetical boulder which has become embedded in the ice. We shall conceive that this boulder has been introduced by friction into the ice at the bottom of some valley in the interior of the country. As the ice creeps outwards, the stone gradually rises, the path which it follows sloping at a less angle than the bed over which the ice flows. Did no obstruction intervene, it is evident that the boulder, while it rose through the body of the ice, would be at the same time travelling gradually to lower levels than the point from which it originally set out. And it is quite conceivable that under such circumstances a stone might rise as it were through a thickness of several thousand feet of glacier ice, and yet be eventually extruded at a lower level than the rock of which it once formed a part. But then we know that

countless obstacles intervened to impede the flow of the massive ice-sheets of the Glacial Epoch; and with every such obstruction the glacier masses must have been forced to bulge upwards by the intense *vis a tergo;* and such upward movement of the ice, being again and again repeated, imprisoned boulders and debris would be compelled just as often to rise to higher and higher levels. Let us suppose that our hypothetical boulder is travelling in a nearly level course towards some ice-smothered hill. As the ice approaches this obstacle, it begins to bulge upwards, and our boulder is, as it were, forced up an inclined plane. Eventually, however, the ice sweeps round and overflows the obstacle, and thereafter continues on its course. There being now less longitudinal pressure, the included boulder will not rise so rapidly, but will travel either in a horizontal line, or down a gently-inclined plane. Let us suppose, further, that such obstructions to the flow of the ice supervene again and again, until at last the boulder is carried into some embayed recess of the ice-drowned country, such as one may see represented in miniature by sheltered hollows in the bed of a river. Now, in such a recess our boulder may linger for a considerable time, but as the ice is always melting and evaporating at the surface, and the loss thus caused is being continually made good by the advance of ice from behind, it is evident that unless the stratum of ice containing our boulder manages to creep out of the embayed recess as an "undertow," it will be forced to rise higher and higher until it becomes coalescent with that portion of the current which overflows all obstacles at a higher level. By this time our boulder, having travelled, let us say, 50 or 100 miles, may have been carried to a level many hundred feet or yards above that from which it originally started. We have now only further to conceive that, after a greater or less number of such vicissitudes, the boulder is at last extruded at the suface, and stranded on the side of some rocky hill that peers like an island above the surface of the far-stretching *mer de glace.*

Now, cases like this must have occurred again and again during the growth, continuance, and decay of the massive ice-sheet that once enveloped Scotland. We know from actual observation that just such obstacles as I have alluded to did really obstruct the flow of the Scotch ice. The confluent glaciers were frequently deflected, now to the right, now to the left, sometimes by opposing high grounds, at other times by masses of ice flowing in a different

direction. This we can read in the trend of *roches montonnées* and striae, in the dispersal of the stones in the till, and the distribution of loose erratics and perched blocks. Nay, we have even clear evidence to show that sometimes the under strata of the ice flowed in one direction, while the ice sweeping along at the surface followed a different route. Hence we can have no difficulty in admitting that such heaping up and bulging of the ice as is required to account for the transport of boulders from lower to higher levels must frequently have taken place in Scotland during the Glacial Epoch.

The generally non-glaciated aspect of the boulders in question is also perfectly accounted for on the theory I have ventured to advance. Once embedded in the ice, stones and boulders might travel for hundreds of miles without suffering abrasion. The well-known incident of the knapsack* which was lost in a crevasse of the Glacier du Talefre on 29th July, 1836, and disgorged by the coalescent Glacier du Lechaud on 24th July, 1846, after having travelled, embedded in the ice, over a distance of 4300 feet, shows how little change a hard lump of rock would sustain in travelling through a mass of glacier ice. Occasionally, however, such an included block might be rubbed against the rocks of a hill-side, and so receive a dressing on one or more faces.

I have thought it worth while to send this note to the Society, as it seems to me to give a feasible explanation of what has always hitherto appeared an insoluble difficulty. As I have remarked above, the acceptance of this explanation does not commit one to any particular theory of glacier motion. So far as it is concerned, all the theories may be wrong, or, what is perhaps more likely, there may be something of truth in each.

* *Edinburgh Philosophical Journal*, January, 1847.