"Geology in Nubibus."—A Reply to Dr. Wallace and Mr. LaTouche.

DR. WALLACE has taught us a great deal, and among those lessons is the supreme virtue in scientific controversy of courage and candour. He must forgive me therefore for answering promptly, and I hope frankly, his last letter in NATURE. In this letter he appeals from your columns to a non-scientific magazine in which he is writing, and where, like the sermon from the pulpit, what is said cannot be answered. This appeal is not to my taste, for I agree with the late Lord Tweeddale, that truth is never so free from difficulty as when the good grain has been thrashed out by the flails of controversy.

The position we are fighting about is too important, however, to go by default, for upon it rests a vast deal of induction in other fields besides geology.

My contention is, and I am speaking to every man of science, geologist or otherwise, that before Dr. Wallace can appeal to ice as the excavator of lake basins on level, or nearly level, plains *far* away from the slopes where glaciers grow, he must establish two postulates. (1) That ice can convey thrust for more than a very moderate distance. (2) That glaciers such as we can examine and report upon are anywhere at this moment doing the excavating work which he postulates. Without these postulates, his appeal to ice seems to me absolutely outside science altogether, and to be a mere resort to some *Deus ex machina*, such as the mediæval schoolmen based their reasoning upon.

In regard to the first postulate the experimental evidence seems to me to be conclusive, and I have quoted it in my work on the glacial nightmare. Mallet, writing on the modulus of ice, says: "A few experiments have been made which show that the height of this modulus cannot exceed a few hundred feet." "Let it be assumed, however, that it is as great as 5000 feet, or a mile. It is then obvious that a mass of ice, no matter how deep or wide, lying in a straight, smooth, frictionless valley, cannot be pushed along by any extraneous force, in the line of the valley, through a distance of more than a single mile, for at that point the ice itself must crush, and the direct force cease to be transmitted further. This, of course, is far from being the whole of the question of the transmission of force through ice, for when and wherever crushing takes place, a certain portion (though a small one) of the direct pressure is transmitted laterally by the crushed fragments, especially if mixed with water. For this to take place however, in the direction of the length of the ice-filled valley, supposes the ice must be considerably more than a mile in vertical depth." Mr. Oldham has carried the question further, and I have quoted his arguments and experiments on pages 596–597 of my book. His conclusion, after postulating a quite transcendant modulus, as tested by observation, is: "The greatest distance to which a glacier could be forced *en masse* is about five miles, so that a glacier debouching on a plain could not exert any erosive power on that plain for more than five miles from the commencement of its level course, and consequently could not scoop out a lake basin of more than that length, whatever its depth might be."

Not only does this conclusion involve the postulating of quite an impossible modulus for ice, but it also supposes that the whole thrust of the ice coming down a slope is available, which it clearly is not. A great deal of this thrust, as Mr. Irving has shown, is expended in overcoming cohesion, in causing the differential motion of a glacier, in forming crevasses which largely intercept the thrust, and in causing the well-known Bergschrund. To quote my own words, "a considerable amount of the force of the gravity contained in a glacier is used up within the glacier itself, and is not available either to give it a forward thrust along a horizontal surface, or for eroding purposes."

So far as I know, this is a perfectly candid statement of the available evidence. Regelation has nothing whatever to do with it. Directly ice crushes, the thrust is dissipated, the greater part of it passing off in the direction of least resistance. To me the case seems conclusive, but, says Dr. Wallace: "All this is beside the question from my point of view. The work of the ice on the rocks is as clear as that of palæolithic man on the flints . . . and there is clear evidence that ice *did* march a hundred miles, mostly uphill, from the head of Lake Geneva to Soleure, whatever transcendental qualities it must have possessed to do so."

This form of dogmatic argument is assuredly incomprehensible. I wonder Dr. Wallace is not afraid of the ghosts of his own recent emphatic pronouncement on the glaciation of Brazil, which he has now entirely abandoned, namely: "If the whole series of phenomena here alluded to have been produced without the aid of ice, we must lose all confidence in the method of reasoning from similar effects to similar causes which is the very foundation of modern geology."

No, true geology is not founded upon hypotheses outside the laws of nature; its secrets, when properly read, must be consistent with those laws. Nor can the geologist who hopes to see his work live, base his reasoning upon a peculiar scheme of mechanics which experiment refuses to verify.

If glaciers travelled further in former days, it was doubtless because glaciers were larger in former days, because they descended longer slopes, and had larger gathering grounds; that is to say, because the country where they grew was more elevated. All this I, of course, admit was the case. That ice could travel then any more than it can travel now over a considerable distance of level ground, or excavate hollows in its track, by virtue of the *vis a tergo* given it in its sloping cradle, is, it seems to me, a subjective dream, and not an empirical conclusion.

So much for the first postulate necessary to establish Dr. Wallace's conclusion. In regard to the second, I have little to say. Glaciers exist in many countries. In some they have retreated in historical times; in others, we can travel underneath them for some distance. I know of no case, under any conditions, where it can be shown that they have excavated rock basins, small or big. If Dr. Wallace can quote any, it would be an important addition to the case he makes. I must therefore conclude that, so far as our evidence goes, ice cannot excavate lake basins on level plains, and that it is contrary to the laws of the mechanics that it should do so.

Dr. Wallace says, "No glacialist of the extremest school would claim the rock basins of Bahia as proofs of glaciation." This is an extraordinary statement. Why, the report on these basins made by Mr. Allen, and incorporated by Hartt, was among the most powerful pieces of evidence adduced by the latter for the former glaciation of Brazil, which evidence Dr. Wallace urged upon us a short time ago was completely unanswerable. Lastly, in regard to Tasmania I do not quite follow him. He says, "No doubt the conclusions of the various writers will be fully harmonised by a more complete study of the whole subject." They are harmonised already. *They all agree* that on the plateaus and in the central district of Tasmania, where the lakes abound, there are no traces of glaciation. So far as I know, the only person who disputes it is Dr. Wallace himself, who has never been there. What needs to be harmonised is his theory with the facts as observed by all observers.

I have replied at some length to Dr. Wallace's letter, not only because I consider the issue a most critical one, but also because of the distinction of its writer, who on so many questions has taught us lasting lessons, but who on this one seems determined to set himself against the general conclusions of those geologists who have most closely and laboriously studied ice at work.

I must now turn to Mr. LaTouche, whose courteous criticism of my views appeared in a previous number of NATURE. I am not quite sure how far we differ, for he apparently repudiates the theory favoured by Ramsay and by Dr. Wallace, that the great Alpine and Scotch lakes were excavated by glaciers. He limits himself to certain rock basins in highly glaciated regions. In regard to these having been excavated by ice, Mr. LaTouche reminds me that ice is a viscous body, and moves, as Principal Forbes argued that it does, almost entirely as a viscous body. If Mr. LaTouche had favoured me by looking into my last book, he would have found a long and very laborious chapter devoted to establishing this very conclusion, but I do not see how it assists his position. A viscous body, unless the viscosity approaches that of a liquid, cannot move by mere hydrostatic pressure, since the internal friction and the resistance and mutual support of its particles prevent it. The viscosity of ice is very slight indeed, hence we cannot postulate for the nether layers of a glacier with an uneven surface the movements we should postulate in a liquid under the same conditions. With the force known to be requisite to make it shear, it seems to me that ice cannot be supposed to move by hydrostatic pressure.

Its actual motion is due almost entirely to its layers rolling over each other as they do in pitch and other viscous bodies. Now this movement in thick ice we know is appreciable at the surface, but the same conditions of friction and of drag, already quoted, retard each successive layer as we go down, until when we reach the lowest layers the motion due to viscosity is exceedingly slight if it is even appreciable. Hence I cannot see where the mechanical agent is to come from to excavate basins, and how it is to work.

When ice is moving on a slope, and the viscous movement is helped by gravity, then no doubt the ice-foot shod with stones becomes a tolerable *eroding* agent; but I cannot understand under what conditions it can become an excavating one, and how it can hollow out basins, &c.

When ice moves away from the slope which gives impetus to a glacier, the motion rapidly slackens and presently stops. The distance travelled over the level ground is a function of the weight of the glacier, of the amount of the slope, the friction of its bed, &c., *i.e.* of the elements making up the *vis a tergo*; but in the very largest glaciers, so far as observation goes, the motion rapidly ceases on level ground. This is the evidence wherever the phenomenon has been observed and reported upon.

This being so, I altogether question not only the arguments of those who champion the excavation of lake basins by ice, but also of that larger school who invoke movements of ice over level plains of many hundreds of miles in extent in order to explain the drift phenomena. They do it, so far as I know, on the ground that they cannot appeal to any other cause without doing injustice to that modern metaphysical bogey, "The Doctrine of Uniformity." My small boy might just as well, on the same principle, attribute the excavation of his porringer to the porridge in the bowl. True rock basins were no doubt very largely due to the weathering of rocks which exfoliate, and whose structure is not homogeneous. This is a very old explanation, but like many sober old inductive truths it is not so attractive nowadays as an appeal to the imagination, combined with a good, sturdy, consistent loyalty to some *à priori* postulate, which would have won the hearts of the old schoolmen.

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