Sir Henry Howorth and "Geology in Nubibus."

SIR HENRY HOWORTH, in his reply to Dr. Wallace and Mr. LaTouche, concerning the excavating power of ice, remarks that he is "speaking to every man of science, geologist or

otherwise." Indeed, from the tone of his letter he would appear to be defending modern science against the attacks of certain unscientific persons who hold extreme views on glacial questions. As one who has taken a great interest in this subject for a number of years, I trust that I may be allowed to add a few words to the discussion.

We are required by Sir Henry Howorth to establish two postulates. "(1) That ice can convey thrust for more than a moderate distance. (2) That glaciers, such as we can examine and report upon, are anywhere at this moment doing the excavating work . . ." Dr. Wallace postulates.

In reply to the first, we have the undoubted fact that in hundreds and thousands of instances striated rock surfaces do occur hundreds of miles from existing glaciers. On this point he remarks: "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." So the glacial period resulted from elevation, and all glaciated regions conveniently rose together to produce it, and as conveniently sank down again. I was quite unaware that this was the accepted view. We have no proof whatever that the striated slopes down which the old glaciers moved were steeper in glacial times than they are now. Indeed, the proof is all the other way, and we may consider it as proved that at long distances from their sources, and on comparatively level plains, glaciers have moved, and have polished, ground, scratched and *grooved* the rocks over which they passed. The only point about which there may be legitimate discussion concerns the possible extent of the abrasion.

In his mechanics Sir Henry Howorth is, I am afraid, rather unsound. There are really two factors upon which the possibility of motion in a viscous body depends. One is, of course, the slope of the surface over which it passes, and the other is the slope of the upper surface of the viscous body. Fracture and regelation have little to do with the question, for fracture only occurs near the surface, and fracture must not be confounded with *shear*. Sir Henry Howorth makes one statement which seems to account for the conclusions he has come to. It is "a viscous body, unless the viscosity approaches that of a liquid, cannot move by mere hydrostatic pressure." In fact he assumes, without adducing a particle of evidence in support of the assumption, that there is an inferior limit to the stress required to deform *glacier* ice. I always regarded viscosity as something which retarded motion, but did not in any way interfere with the ultimate result. I have personally made mechanical tests of ice, and also of many thousands of samples of steel, iron, copper, brass, tin, &c. All these substances yield elastically and permanently under stress, some of them under very small stresses, but ice is the only one of them that yields continuously from the moment the stress is applied until it is again removed.

It is not, properly speaking, pressure from behind that forces the ice forward. Ice being viscous, every individual particle moves in the direction of least resistance at a rate depending upon the stress and the viscosity. Sir Henry Howorth may term this "Geology in Nubibus," and call it unmechanical; but I would point out to him that I regard the question from the point of view of a *mechanical engineer*, which I am afraid he does not.

During the past summer I had the pleasure of seeing some of the Norway glaciers, and also of crossing the Folgefond snow field. It was interesting to note that although the streams coming from the hills and uplands free from ice were quite clear, those escaping from the glaciers were charged with sediment. In this connection I would call attention to a calculation made by Prof. G. F. Wright, giving the rate of erosion of its bed by the Muir Glacier. From the volume and turbidity of the water he makes the figure one-third of an inch per annum over the whole of the 1200 square miles of area occupied by the glacier. In fact, erosion goes on much more rapidly when the rocks are covered by moving ice than when they are not. Although we may feel absolutely certain, both by fact and reason, that the erosion beneath glaciers when they are moving with relative rapidity is very great, and be as sure as we reasonably can be of most things that such erosion must result in the formation of lake basins, I am afraid that we shall be unable to satisfy Sir Henry Howorth on the point. We cannot remove a glacier, and if there should prove to be a rock basin below measure of its depth, then replace the ice, and measure again, say, in a thousand years. This is the kind of proof the second postulate seems to demand.

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