"Geology in Nubibus."—Mr. Deeley and Dr. Wallace.

MR. DEELEY will not have anything to say to ice conveying thrust *as a solid body*, which has been the sheet anchor of glacial geology for many a decade. He also repudiates Dr. Wallace's notion that regelation can in some way act as a compensating element when crushing supervenes in ice, and thus enable it under crushing pressure to convey thrust. So far so good.

Mr. Deeley, however, bids me turn to ice acting as a viscous body, a subject on which I have written a great deal in my recent book, which he does not seem to have seen.

There are two ways in which we can conceive a viscous body flowing on a flat plain: (1) by pure fluid, or what is commonly called hydrostatical pressure, in which the upper layers move up and down, and the lower layers alone have a horizontal motion; (2) by its particles rolling over each other. The former depends, of course, entirely upon the difference of level of two connected parts of the mass under consideration; the latter depends upon the slope of the upper surface of the fluid.

I contend, as Forbes contended, that in the case of a body so slightly fluid as ice, motion by hydrostatic pressure is practically impossible. The consistency and mutual support of the parts prevent the indefinite transmission of pressure in this way through ice, and nowhere have I seen or heard that in detached masses of a glacier cut off at either end by crevasses the ice rises in one place, and sinks in another, or that the walls of these ice rifts or the perpendicular ice walls in the arctic and antarctic regions or in scarped icebergs bulge out below in the slightest degree, as must happen if ice were to move in this method.

Forbes' experiments and measurements and patient examination of the problem proved that ice as a viscous body moves in fact by its layers rolling over each other, and that this motion is differential, being greatest at the surface and in the middle, and least at the base and sides of a glacier.

It is quite true that the rate of this motion on a flat plain would depend theoretically on the slope of the upper surface of the ice. It is established by experiment, however, that such motion is very largely confined to the surface layers, and when we approach the nether layers the motion quickly slackens, owing to the internal friction and drag of the ice particles. Even on inclined beds, glaciers have sometimes been found frozen to the ground. The evidence of a large number of observers is conclusive, that as glaciers reach the level ground, the motion, even of their upper layers, gradually stops. The masses of ice that collect on the flat Siberian Tundras do not move at all, nor do the thick horizontal ice beds examined by Dall in Alaska. Argument, experiment, and observation are therefore entirely against Mr. Deeley, upon whom the burden of proof rests. Perhaps he will explain what are the conditions under which he conceives his ice sheets to have been formed, to have been maintained, and to have moved. Mr. Wallace confesses that he does not like to face these mechanical issues, which are presupposed in all his reasoning. This is assuredly building on a quicksand, which is not a profitable experiment. He cannot be serious, either, in arguing that because I believe in Charpentier's view that the Alps were formerly higher, and consequently nursed bigger glaciers, I am therefore committed to Ramsay's extravagant notions, repudiated by nearly all explorers of glaciers, that the lakes of Geneva and Lucerne were dug out by ice. Charpentier's method, in such a case, would have prompted him to first prove the capacity of ice to do the work, and most people will agree that in a scientific argument this method is alone fruitful. H. H. HOWORTH.

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