THE question you have allowed me to raise is too important and far reaching to justify its dissipation upon personal issues. It cannot be thought unreasonable that those geologists who propound transcendental theories should justify the mechanical postulates on which they claim to base them. This is all I have asked.

Dr. Wallace asks me to explain what will happen when sufficient pressure is applied to ice not only to crush it, but to induce regelation. I have already explained in my work, that the notion of fracture and regelation taking place in glaciers is at issue with the details of their differential motion as tested by experiment. There is no evidence that ice which on pressure being applied to it has ample room to move, will undergo regelation at all. The pressure when crushing ensues will be dissipated in the direction of least resistance, and most probably upwards. This emphasises Mr. Deeley's statement, and he wrote as a champion of Dr. Wallace, that "fracture and regelation have little to do with the question."

Dr. Wallace then returns to his charge against me that I have in some way committed myself in my work to a position inconsistent with the one I am now maintaining. I can assure him that if he has read this meaning into my words, it was not what they were meant to convey. In giving the history of the "Glacial Nightmare," I entered largely into the views of Charpentier, and in so far as he championed glaciers as against ice sheets I agree with him. I have said that his views "*are for the most part sound and unanswerable*, since *they finally established for the Alpine country and for Switzerland the fact that glaciers were formerly much more extensive*," &c. Beyond this I could not go, since my work was written to prove the unscientific character of the extravagant conclusions of the later glacialists, including Charpentier himself after he became a follower of Agassiz. Apart from this, however, what your readers I am sure would welcome would be an *argumentum ad rem*, and not one *ad hominem*.

In demanding that the advocates of the glacial theory in its extravagant form should justify their premises and postulates, I must not be understood to decline to meet the geological case against the glacial excavation of lobes. I have met it at great length already in my recent work, but not so ably and not so thoroughly as Mr. Spencer met it in his elaborate and crushing examination of the critical case of the North American lakes, which I commend most heartily to the study of enthusiastic champions of omnipotent ice.

The geological question, however, is necessarily contingent upon the mechanical question, and no amount of ingenuity will in the long run enable those who invoke ice as the author of all kinds of geological work to evade the duty of proving its capacity to do that work, and notably to explain how it can travel over hundreds of miles of level country, or suddenly begin to excavate deep and extensive lake basins after it has been moving gently over its own bed of soft materials for many miles, or, indeed, how it can excavate on level ground at all. The first step is to show that ice can convey thrust in a way to compass these ends; the second one is to show whence this thrust is to be derived. Your readers who are committed to no theories unsupported by facts, will not quarrel with the reasonable demand that these first steps should be surmounted before we advance any further. Those who like to traverse cloud-land on the wings of fancy may be otherwise satisfied. To them I would only say that the result cannot be science; it must remain nothing more than poetry.

HENRY H. HOWORTH.

30 Collingham Place, Earls Court, December 30, 1893.