lordship will no doubt be glad to learn that so far as this Department is concerned, scientific specimens sent by sample post, and addressed to places abroad, will not be stopped in future; but I must state that this Department cannot guarantee the delivery of such specimens abroad, inasmuch as they do not come within the definition of sample packets as prescribed by the Postal Union." I may add that within the last month I have, on two occasions, sent specimens abroad by sample post with perfectly satisfactory results.

All naturalists will feel grateful to the Academy of Natural Sciences of Philadelphia for agitating in this matter. But it is to be regretted that the United States Postal Department should, in another way, continue to maintain a barrier against cheap transmission and interchange of specimens. The sample post can, in any case, only be used for small packets, but larger packages can now be sent to nearly all foreign countries by parcel post, the introduction of which was an inestimable boon. The United States Government stands almost alone in persistently refusing to co-operate in this respect. It is not for scientific men to inquire into what contracts that Government may have entered into with private carrying companies, or how far it may be influenced by hyper-protective susceptibilities; they can only regret the facts, and deplore the result.

Lewisham, December 8. R. MCLACHLAN.

"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.

30 Collingham Place, Earls Court.

The Viscous Motion of Ice.

IS not Sir H. Howorth wrong in assuming that there is no transmission of hydrostatic pressure in ice? Certainly Forbes was of opinion that such transmission existed, and was necessary to explain the remarkable parallelism between the motion of ice and of viscous fluids. It is a question of scale. Even a cup of treacle will not flatten out indefinitely; still less will a barrel of pitch; but I have no doubt a cubic mile of ice would flatten out, but to what extent is a question for calculation, not for dogmatic assertion. Unfortunately the first requisite of such calculations is wanting, as no determination of the coefficient of viscosity exists. Canon Moseley's experiments are clearly out of court, and in the interesting experiments of Mr. Coutts Trotter in 1883, the length of the portion of ice which took part in the shearing motion is not given.

May I add that the paragraph in Sir H. Howorth's letter of November 23, in answer to Mr. LaTouche, is distinctly erroneous so far as our limited evidence goes.

If Sir H. Howorth will draw to scale the observations of Prof. Tyndall at the Tacul on the side of the Mer de Glace, or those of Prof. Forbes, given on page 554 of his own book, he will see that while the velocity of the ice is greatest at the surface, the viscous yielding or differential motion is greatest at the bottom; and the curve into which a vertical line in the ice is thrown by the motion, is always convex towards the direction of motion, is relatively flat above, and strongly curved towards the base. This is exactly what we should expect on the viscous hypothesis, and justifies the application of hydrodynamical treatment to the problem, if only the necessary data were to hand.

29 The Boltons, S.W. JOHN TENNANT.
December 12.

Chemistry in Space.

IT may be of interest to your readers to know that the idea of the arrangement of atoms in space, which is looked upon as quite a modern one, is clearly put forth by Wollaston in his paper entitled "On Super-Acid and Sub-Acid Salts" (Phil. Trans. vol. xcviii. 1808, pp. 96–102).

He discusses the constitution of the two oxalates of potash; and I make the following extracts, but must refer your readers to the original paper for the full context. . . . "when our views are sufficiently extended, to enable us to reason with precision concerning the proportions of elementary atoms, we shall find the arithmetical relation alone will not be sufficient to explain their mutual action, and that we shall be obliged to acquire a geometrical conception of their relative arrangement in all the three dimensions of solid extension. . . . when the number of one set of particles (combined with one particle), exceeds in the proportion of four to one, then, on the contrary, a stable equilibrium may again take place, if the four particles are situated at the angles of the four equilateral triangles composing a regular tetrahedron. . . . It is perhaps too much to hope, that the geometrical arrangement of primary particles will ever be perfectly known." Thus Wollaston's conception of the combination of four particles with another is exactly the same as our modern idea of the arrangement of four monovalent atoms (or groups) in combination with a carbon atom. The same idea is also developed somewhat later by Ampère in his "Letter to Berthollet" (*Annales de Chimie*, 90, p. 43–86, 1814), in which he considers the molecules as forming various geometrical figures dependent on the number of atoms contained therein.

JOHN CANNELL CAIN.

The Owens College, Manchester, December 14.