The white colour of the berg is due to innumerable air bubbles in the ice, and not to snow on the surface. An iceberg is very deceptive in this way. While it looks quite soft, the ice is so hard as to make it difficult to chop with an axe. The ice water which I prepared for drinking on board ship with iceberg ice effervesced like soda-water, merely due to the liberation of the air from the melting ice. It is possible that the sudden disappearance of bergs with a loud report is due to their explosion from accumulated air in the interior. One berg which I studied was casting off small pieces, apparently by the pressure of the pent-up air.

Effect of Land.

While icebergs send the temperature of the sea up, the coast-line sends it down. I believe this to be due to the action of the land in turning up the colder under-water. My observations show this effect not only here, but on the English and Irish coasts.

From the point of view of the safety of our St. Lawrence route, the effect of land is most important. The iceberg causes us very little worry because we have only a very short ice track, but to find means whereby the presence of land can be determined is of the greatest importance. A full account of my experiments is being published by the Canadian Department of Marine. H. T. BARNES.

McGill University, November 16.

The Bending of Long Electric Waves Round the Globe.

I HAVE just noticed (very belatedly) that in your reprint of Dr. Fleming's admirable opening of the British Association discussion of the problems of wireless telegraphy, there occurs a passage that raises an objection to a certain mathematical result of mine. Dr. Fleming's opinion in all matters radio-telegraphic is of such great weight that his objection, whether sound or not, is sure to prejudice the fair consideration of a hypothesis I have based on the mathematical result in question, and since the objection has obtained the wide publicity of your columns while my own account of the matter has not, I trust you will allow me space to comment upon it. Comment seems especially necessary on account of Dr. Fleming's eloquent advocacy of certain rival hypotheses.

Put briefly, the theorem is to the effect that the velocity of long electric waves through air containing charged ions is greater than the velocity through un-ionised air, and this leads to a hypothesis for explaining, among other things, the propagation of electric waves over the convexity of the globe. In forming the electromagnetic equations I took the average dielectric constant of the ionised air to be the same as that of the un-ionised air, following in this respect the example of previous writers on similar problems. It is to this customary assumption that Dr. Fleming's objection applies.

In rebutting the objection there are several plain courses. For example, I might recall that the formula I deduced for the increase of velocity may also be obtained from the accepted theory of "anomalous" dispersion—a theory in which the influence of a finite change of the dielectric constant is considered to be negligible. But in the present instance it seems preferable to take another course, and to ask, plainly, Why should the presence of electrified molecules in the number required by my hypothesis affect the dielectric coefficient used in the differential equations? It must be noticed that the concentration of the ions demanded for bending a ray to fit the curve of the earth is of the order 10^5 ions per c.c., assuming the ions to be molecular in size; and thus the proportion of ions to molecules is of the order 10^{-13}. It appears to me most unlikely that such a small proportion of ions can affect the real dielectric coefficient of the medium, especially in view of the fact that there does not seem to be any direct or indirect evidence based on experimental or theoretical knowledge of gases that can be held to support such a view.

I may add that I am quite well aware of many real difficulties confronting the hypothesis. I am not now writing in reference to any of those, but wish merely to point out that the objection urged by Dr. Fleming is, so far as I can see, a remotely conjectural one. W. H. ECCLES.

University College, Gower Street, W.C., December 2.

The Specular Reflection of X-rays.

IT has been shown by Herr Laue and his colleagues that the diffraction patterns which they obtain with X-rays and crystals are naturally explained by assuming the existence of very short electromagnetic waves in the radiations from an X-ray bulb, the wave length of which is of the order 10^{-9} cm. The spots of the pattern represent interference maxima of waves diffracted by the regularly arranged atoms of the crystal. Now, if this is so, these waves ought to be regularly reflected by a surface which has a sufficiently good polish, the irregularities being small compared with the length 10^{-9} cm. Such surfaces are provided by the cleavage planes of a crystal, which represent an arrangement of the atoms of the crystal in parallel planes, and the amount by which the centres of atoms are displaced from their proper planes is presumably small compared with atomic dimensions.

In accordance with this, the spots in Laue's crystallographs can be shown to be due to partial reflection of the incident beam in sets of parallel planes in the crystal on which the atom centres may be arranged, the simplest of which are the actual cleavage planes of the crystal. This is merely another way of looking at the diffraction. This being so, it was suggested to me by Mr. C. T. R. Wilson that crystals with very distinct cleavage planes, such as mica, might possibly show strong specular reflection of the rays. On trying the experiment it was found that this was so. A narrow pencil of X-rays, obtained by means of a series of stops, was allowed to fall at an angle of incidence of 80° on a slip of mica about one millimetre thick mounted on thin aluminium. A photographic plate set behind the mica slip showed, when developed, a well-marked reflected spot, as well as one formed by the incident rays traversing the mica and aluminium.

Variation of the angle of incidence and of the distance of plate from mica left no doubt that the laws of reflection were obeyed. Only a few minutes' exposure to a small X-ray bulb sufficed to show the effect, whereas Friedrich and Knipping found it necessary to give an exposure of many hours to the plate, using a large water-cooled bulb, in order to obtain the transmitted interference pattern. By bending the mica into an arc, the reflected rays can be brought to a line focus.

In all cases the photographic plate was shielded by a double envelope of black paper, and in one case with aluminium one millimetre thick. This last cut off the reflected rays considerably. Slips of mica one-tenth of a millimetre thick give as strong a reflection as an infinite thickness, yet the effect is almost certainly not a surface one. Experiments are being made to find the critical thickness of mica at which the reflecting power begins to diminish as thinner plates are used. The reflection is much stronger as glancing incidence is approached. W. L. BRAGG.

The Cavendish Laboratory, Cambridge, December 8.