Letters to the Editor.

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The Theory of Geological Thermal Cycles.

IN a review of the second edition of Prof. J. Joly's "Surface History of the Earth" (NATURE, Feb. 14, p. 227) Prof. A. Holmes makes the rather sweeping statement that the proposed mechanism for the alternating accumulation and discharge of heat seems to be physically unacceptable. He appears to justify this view from Dr. Jeffreys' opinion, who originally based his condemnation of the theory on the fact that there is no periodic solution to the equation giving the heat flow in a solid medium. When, however, change of state occurs in the medium, we have to allow both for the motion of the medium itself and also for the latent heat of fusion, and obviously the original differential equation will have very little bearing on the result. Dr. Jeffreys, in his second edition of "The Earth", has simply solved this problem by assuming that if at any depth the radioactive materials were sufficient to raise the medium above the melting point, then the resulting convection currents would exactly carry off the excess heat and a steady state would again be attained. Mathematically, this is equivalent to assuming that the effective conductivity of the medium would adjust itself to give the required heat flow at the required temperature gradient. It is not often that Nature is so accommodating.

It is possible, nevertheless, to discuss the problem mathematically rather more fully, and I have attempted to do so in a paper on "The Thermal Instability of the Earth's Crust" published in the *Scientific Proceedings* of the Royal Dublin Society, vol. 19, No. 32. In this paper, it is shown that, assuming a basaltic crust with a definite melting point and latent heat, and a melting point pressure curve steeper than the adiabatic for the liquid state, conditions which ordinary basalt apparently fulfils, then periodic partial melting and resolidification of the crust, if it exceeds a certain depth, appear to be physically inevitable, quite apart from any tidal or other effects. This result certainly must be applied with caution if the crustal materials have no definite melting point. Personally, I consider that if these materials are capable of existing over a certain range of temperature and pressure as two phases which differ greatly in viscosity and total energy content, then it is probable we are justified in doing so. If, on the other hand, they pass slowly from the liquid to the solid or highly viscous glassy state, so that their viscosity and energy content at a given temperature and pressure are always single valued, then probably Dr. Jeffreys' assumption would be correct. Enough, however, has been said to show that Joly's original theory of thermal cycles cannot be dismissed as in any way physically impossible or improbable. The whole question ultimately hangs on the real properties of the substratum.

Turning to a second point in Prof. Holmes's review, he objects to Joly's theory on the ground that it leads to an alternation from world-wide tension to world compression, which he says he finds geologically unacceptable. To get over this difficulty he has proposed a system of large-scale convection currents in the substratum in which the stresses in the surface crust are due entirely to this current

motion. In this, he neglects the fact that if thermal energy is accumulating at any time in the subcrustal materials, they must tend to expand, due either to ordinary thermal expansion or to increase of volume on fusion, and that therefore such epochs must on the whole result in increased tension in the outer crust. The only escape I can see from this impasse is to assume that the progress of energy accumulation and discharge is not simultaneous over the whole earth. It would appear easier to do this on Joly's hypothesis than on Holmes's modification of the theory. Besides, it is not at all certain that there is no evidence for world-wide tension and compression. It is usually held that mountain-building epochs, that is, states of compressional strain, are more or less world-wide and contemporaneous, and if the evidence for world-wide tension is not so obvious, this may be due to the fact that it has largely been taken up in the ocean floor, where its effects would not be so open to our inspection, as Joly has already pointed out. It is perhaps also not without significance that the so-called common fault in geological science is a tension fault.

To refer to another matter: the connexion which Holmes has emphasised between the state of the outer shell of the earth and terrestrial magnetism appears to be most important. On Larmor's convection current theory of the origin of the earth's magnetic field, we might expect the strength of the field to increase largely in times of revolution when the subcrustal materials are most fluid. Mercanton, of Switzerland, has already attempted to determine the direction of the field at various geological epochs by investigating the direction of magnetisation of lava flows of different ages, and it is just possible that by measuring their intensity of magnetisation some information as to the strength of the earth's field at the time of solidification might be obtained. The problem is not a simple one, since the intensity of magnetisation would depend, not only on the field strength, but also on the chemical composition of the various lavas, and possibly other variables. It indicates, nevertheless, a new avenue of approach to the problem, which might yield some interesting results.

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WHEN I wrote my review of the second edition of Prof. Joly's book several months ago, I had not seen Dr. Poole's paper, but the ideas which he there treats mathematically were by no means new to me, for I had already envisaged them in a qualitative way in my paper "Contributions to the Theory of Magmatic Cycles" (*Geol. Mag.*, 1926, p. 315 *et seq.*). So far from denying that they have important applications, I have since attempted to develop the principles involved, especially in their relation to problems of petrogenesis. In stating that "Joly's mechanism of alternating accumulation and discharge of heat seems to be physically unacceptable", I had particularly in mind the probable distribution of radioactivity through the whole of the earth's substratum, that is, down to a depth of 2900 km.

Joly and Poole appear to assume the existence of a thick layer of material of basaltic composition through which successive waves of fusion develop, pass upward, and die out, this layer being underlain by a lower layer of crystalline rock. The latter conception has hitherto been tacit, but Poole has given it definite expression in the paper to which he refers. On p. 405 he speaks of "a lower infusible layer, such as probably occurs in the earth's crust". In the light of all the relevant evidence, I can see no reason