phenomena, and then asks me whether I can name a single one of these who did not become convinced of their genuineness. Surely this is a case of begging the question, as these are the particular five who did become convinced; but what about the others? What about Faraday, Tyndall, Sir David Brewster, and Dr. Carpenter, among those who are dead, and Prof. R. W. Wood of the U.S.A., Sir E. Ray Lankester, and Sir Bryan Donkin among those who are living? All these and many others have, I believe, made sufficiently serious investigations into the subject, though naturally, having come to the conclusion that there was nothing genuine in the phenomena warranting further research, they did not publish so much as other no more eminent, though perhaps more credulous investigators.

It may also be mentioned that, in 1908, a committee, including such eminent photographic experts as R. Child Bayley, F. J. Mortimer, and E. Sanger-Shepherd, though assisted by such a well-known spiritualist as Mr. A. P. Sinnett and others, failed to secure proof that spirit photography is possible.

Dr. Tillyard suggests that I should visit the National Laboratory for Psychical Research, but I must confess that I am not attracted by its name, which with its suggestion of parallelism with the National Physical Laboratory, seems to me to be *suggestio falsi*. I am informed that it is a purely private concern, with nothing national about it whatever. Apart from this, however, in my opinion thermographic phenomena in connexion with mediums are more a matter for a physiologist than for a physicist. That emotional disturbances affect the temperature of the body is, I think, fairly well known, and there does not seem to me to be any reason for dragging in such supernormal and incredible phenomena as the production of ectoplasm and such like in order to explain what are only commonplace matters. But then, all psychical researchers seem to delight in the maxim *omne ignotum pro magnifico*.

A. A. CAMPBELL SWINTON.

40 Chester Square, London, S.W.1,
September 10.

The Structure of the Continents.

As all the continental discussions of the observations of near earthquakes have been carried out by graphical methods, and as I could not satisfy myself as to the precision obtainable by these methods, I have recently carried out a rediscussion of the principal series of data by the method of least squares. These refer to the Kulpa valley earthquake of 1909, the Wurtemberg one of 1911, the Tauern earthquake of 1923, and the Oppau explosion. The results indicate very definitely that there is an upper layer that transmits compressional waves with a velocity of 5·6 km./sec. (though a velocity of 5·4 km./sec. would fit the Oppau explosion slightly better) and a lower one where the velocity is 7·8 km./sec. In addition, the Tauern earthquake gave rise to a wave with a velocity of 6·2 km./sec., which must have travelled in an intermediate layer. The probable error of all these velocities does not exceed 0·1 km./sec. The result for the upper layer corresponds to that found for granite by E. D. Williamson and L. H. Adams. The recent work of L. H. Adams and R. E. Gibson gives a velocity of 6·4 km./sec. in basaltic glass, and of 8·4 km./sec. in dunite, at ordinary temperatures and at pressures corresponding to depths of some tens of kilometres. If we allow for the higher temperatures within the crust, the basaltic layer below the granite may be in a glassy state, as Daly has suggested, and the lower layer may well be dunite. The evidence indicates that there is no further sudden change to a depth of about 1200 km.

The times of arrival of all the waves were linear functions of the epicentral distance; the consistency of the observations was great enough to give good determinations of the gradients of these functions, and hence of the velocities, but it was not enough to establish any departure from linearity. Hence there was no material for a determination of the depths of the foci or of the variation of velocity with depth in the various layers. By combining the results for near quakes with those for distant ones, however, it was possible to estimate the rate of increase of velocity with depth in the lower layer.

The observations permit a rough determination of the depths of the granitic and basaltic layers. The former may be about 12 km., the latter about 20 km., but both are subject to an accidental error of about 4 km. In addition there is a possibility of systematic error. Uncertainty as to the depth of focus may allow the thickness of the granitic layer to be doubled. On the other hand, the movement on the seismogram due to the indirect waves starts more gradually than that due to the direct one, and this may cause a slight delay in their measured time of arrival, especially as most of the observations seem to have been made on instruments recording on smoked paper. On this ground the depths determined may require some reduction.

I think, therefore, that determinations of the depths of the layers by means of near earthquakes are not more reliable than those based on the earth's thermal state, isostatic balance between continents and oceans, and the group-velocities of surface waves. All of these are affected by uncertainty concerning the thickness of the basaltic layer, but the uncertainties of the method based on the compressional waves from near earthquakes appear more serious. The results, taken as a whole, are as consistent as can be expected; a thickness of 10 to 15 km. for each layer would be within the range of uncertainty of every method.

HAROLD JEFFREYS.

St. John's College,
Cambridge.

Curved Path of Wireless Waves.

IN a recent number of the *Proceedings of the Royal Society* (Series A, vol. 111, N.S. 757) there appears under the title of "Discussion on the Electrical State of the Upper Air," a paper giving the views of several of the authorities who have contributed to the examination of this subject.

The discussion really turns on the question as to why wireless waves follow the curvature of the earth instead of spreading into space.

Heaviside supposed that this was due to a hypothetical conducting layer of the atmosphere existing at a great altitude above the earth's surface, which would act as a reflecting barrier and would compel the wave to remain within the envelope formed by it. Only one contributor to the discussion referred to refraction and diffraction as possible causes.

If it were assumed that the speed of long waves is the same as that of ordinary light, and depends in the same way on the density of the air, then refraction would account for, roughly, one-tenth of the observed deflection. I believe, however, that there are no direct measures of the velocity of long waves, and there is no sufficient knowledge of the nature of the 'ether' or of its relation to ponderable matter to allow of any certain, or even probable, theoretical deductions on this point.