PHILOSOPHICAL TRANSACTIONS OF THE ROYAL SOCIETY A
MATHEMATICAL, PHYSICAL & ENGINEERING SCIENCES

Discussion

Harold Jeffreys, F. A. Vening Meinesz, P. N. Kropotkin, J. D. Bernal and M. G. Rutten

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XXV. Discussion

[The three following written contributions were read to the Meeting by Professor S. K. Runcorn.]

Sir Harold Jeffreys, F.R.S., writes:

I have been asked to make a written contribution to the discussion, but have nothing new to say. My views will be found in the 1961 version of *The Earth* and in my lecture to the Royal Astronomical Society last October, which will be published in their *Quarterly Journal*.

My main points are that the only type of imperfection of elasticity considered in convection and drift theories is the elastico-viscous law, which has been found to lead to numerous contradictions when confronted with actual evidence. Different phenomena led to values of the effective viscosity differing by factors of millions. On the other hand, a modified law, chosen to fit two quantitative data and applied far beyond the range of periods related to those data, has steered its way nicely among the other evidence for some sort of imperfection of elasticity, without giving any contradiction. But it does forbid convection and continental drift.

I should be disposed to agree that inability to explain an alleged phenomenon is not necessarily a disproof of that phenomenon; but it does require a higher standard of scrutiny of the evidence for that phenomenon. The standard actually applied to evidence for continental drift seems to be considerably lower than is usual for a new phenomenon, and is not associated with any alternative explanations of things that can be explained.

F. A. Vening Meinesz, For.Mem.R.S. writes:

A few of the remarks I should like to make have regard to the hypothesis of convection currents in the mantle of the Earth. They do not concern the main subject of continental drift.

In 1930 I was led to assuming mantle convection-currents; otherwise no satisfactory explanation could be given of the gravity fields in the Indonesian and Caribbean archipelagos. A great many other arguments in favour of such currents can be given. For these arguments I may, for instance, refer the reader to a paper by the writer (1961 *a*).

All considerations of this subject lead to the view that the mantle is crystalline (otherwise we should not be able to explain the passing of the current through the transition layer between 500 and 900 km in depth, in which the density increases about 0·6 more than would correspond to the increase of pressure; if the mantle is crystalline the density increase could mean a transition of the orthorhombic modification of the mantle matter to a denser modification, probably a cubic one, and this would enable the convection current to break through this layer). The crystalline character of the mantle would imply that the stress-deviator (which causes the 'flow') has to exceed a strength limit, needed to overcome the atomic forces, before movement is possible and that the movement has the character of continuous deformation of solid matter; we shall call it 'pseudo-flow'.

This leads to the following conception. If the cooling of the upper mantle has created a potential instability it does not directly lead to a turnover of the mantle; a secondary phenomenon is required for setting this movement going, but once started it accelerates for the first quarter turn and slows down during the second quarter turn. This secondary phenomenon may for instance be a horizontal temperature gradient in the upper mantle, which can be caused by differences in the amount of radioactive constituents in the crust; the continental crust is usually richer in such constituents than the oceanic crust. This would lead to mantle convection currents rising under the border of the continents and subsiding under the adjoining ocean areas.

The concept given above leads to half-turn mantle convection currents which need not occur exactly periodically during the Earth's history. We shall call them episodically occurring mantle convection currents. They lead to episodical orogenic periods at the surface of the continents. This is in harmony with the geological history of the Earth.

The conception here advocated is also based on the extreme smallness of the temperature conduction in the earth, which during the 50 to 100 My the half-turn current lasts does not much disturb the temperature distribution; we may say that the temperature is carried along by the mantle matter and, therefore, also the thermic density deviations. The concept was first published by Griggs (1939).

After the half-turn convection current is accomplished, a long period of several hundreds of million years elapses before a new half-turn mantle current can occur. This period is no doubt shortened by smaller convection cells which quicken the cooling of the upper mantle layer and the heating by the core of the lower mantle layer. The spherical harmonic development of the Earth's topography gives remarkable information about these smaller cells, for which the reader may be referred to a paper by the writer (1961 *b*).

The half-turn mantle convection currents bring the cooling of the Earth down to the surface of the core. The pattern of these currents must bring about a pattern of cooling at the core surface, and this might affect the distribution of the currents in the core. As there do not appear to be other ways to influence the core currents, it seems likely that we must attribute to this cause the changes of sign of the Earth magnetism—if we must indeed assume that those changes of sign have occurred.

Coming to the subject of continental drift, we may conclude that such phenomena can only occur during the periods of half-turn currents in the mantle or, in other words, during the periods of orogeny.

An important point in this discussion is the question whether we can hope to derive the pattern of mantle convection currents from satellite data. The writer of this short note does not think there are any possibilities in this direction. As far as it is known, the gravity field at the Earth's surface does not show any correlation with the mantle current pattern, and this implies that the outside potential field of the Earth cannot provide any useful indication. The same is, therefore, true for the satellite data. The reason for this lack of correlation must probably be looked for in the fact that we must expect that the readjustment of isostatic equilibrium—which the crustal movements after the last glacial period show us to be rather quick—prevents us from locating the thermal density distribution in the earth. As an instance of this lack of correlation the writer may point to the gravity field over the Azores Archipelago which shows a mean positive anomaly of the order of +30 mgal, although we must assume this archipelago to be situated above the rising limb of the mantle currents, that is supposed to be present below all the mid-ocean rises; we may, therefore, admit the

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presence below this archipelago of mantle matter of higher temperature and lower density. Many other instances of the lack of correlation may be given.

To conclude this short note I shall point out a cause of systematic positive anomalies and, therefore, of a tendency towards an outward bulge of the geoid. It is found in active volcanicity, which provides a tendency towards an increase of pressure above the mean value at that depth; for details see Vening Meinesz 1963 *a*. As active volcanicity may be as well expected in areas above rising mantle currents, that sweep off below the crust to both sides and which, therefore, may be expected to cause tension in the crust, as in island arcs where the inner arc is usually actively volcanic but where the crustal downbuckling in the outer arc indicates crustal compression, there cannot exist a direct correlation between active volcanicity and the pattern of mantle currents. Of the two cases the mean positive gravity anomaly above the Azores represents the first case, the mean positive gravity anomaly above the Indonesian Archipelago the second one.

A last point I should like to mention concerns the main object of the discussion: continental drift. The greatest difficulty I feel about this subject is the question how is it possible that the oceanic crust gives way so much more than the continental one, where on the other hand the oceanic crust shows so many straight lined features, as for example, the Menard Escarpments, which stretch from the American continents to far out in the Pacific. How are these features compatible with the idea of continents moving through the ocean-floor? It seems to me that perhaps a way out may be found by the assumption that indeed the oceanic crust is also rigid (as the continental one is) but that a continental geosyncline leads to a downbuckling of the crust over only a few tens of kilometres, but that such a geosyncline in the ocean-crust leads to a downbuckling that can nearly indefinitely continue because the crustal density is so near to that of the mantle, so that, practically speaking, the crustal matter is swallowed up by the mantle. But where are in that case the oceanic geosynclines? For instance near the westcoast of the Americas? (See Vening Meinesz 1963 *b*.)

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Vening Meinesz, F. A. 1961 *b* Continental and ocean floor topography. *Proc. Acad. Sci. Amst.* B, **64**, 512–527.

Vening Meinesz, F. A. 1963 *a* Volcanicity cause of equilibrium disturbances of the geoid. *Proc. Acad. Sci. Amst.* B, **66**, 1–2.

Vening Meinesz, F. A. 1963 *b* Relative movements of continents. *Proc. Acad. Sci. Amst.* B, **66**, 3–7.

Dr P. N. Kropotkin writes in a letter to Professor Bernal:

In the Soviet Union a series of investigations is being carried out which may help in the solution of the problems of continental drift, that is, of major horizontal movements of blocks of the Earth's core.

(1) Recent lists of paleomagnetic data for the U.S.S.R. have been published in the following articles: Kramov & Shmalevo (1963), Kropotkin (1961) Kalashnikov (1961). These paleomagnetic data agree with the paleo-climatic data and show that the Siberian and Russian tectonic platforms approached each other during the Paleozoic by some 3000 km.

In this interval of time, the formation of the folded system of the Urals, Western Siberia and Kazakstan, which are distributed between these two platforms, proceeded. Changes in the difference of paleomagnetic latitudes of points lying on the same meridian shows that in the course of the Mezozoic and Kinozoic, the Indian platform approached Kazakstan and Turkmenia and the Australian platform approached the Siberian platform.

Folding in the geosynclines of Tethys could be connected with these very significant displacements, which are perhaps about 5000 km.

(2) According to the method worked out by A. V. Vvedenskaya of the Institute of Earth Physics of the Academy of Sciences of the U.S.S.R., the orientation of strains in the foci of earthquakes has been determined. It appears that in the Pacific Ocean and in the Alpine Himalayan belts under young folded formations and arcs of islands down to a depth of 150 to 600 km, compression prevails oriented almost horizontally and perpendicular to the spreading of the folds. Under the seismic areas, the ridges of the Indian and Atlantic oceans and the ridges of Schmidt extending from Iceland to the Laktev Sea and also under Lake Baikal and East Africa in the foci of earthquakes, tensions prevail perpendicular to the ridges and depressions.

These results were published by Vvedenskaya (1961), Balakina (1962), Shirokova (1961, 1962) and Misharina (1964). Analogous conclusions were obtained by Enescu in Rumania, H. Honda, M. Ishikawa in Japan and W. Stauder in the U.S.A.

Thus has been discovered planetary systems of zones of compression and tension, their distribution over the Earth and the orientation of the strains in the foci of earthquakes, in broad outlines, agree with the geological data and with the schemes published by Runcorn (1962), Wilson (1963) and Girdler (1963).

Support of the seismicity and the strains inside the Earth demands the existence of corresponding systems of subsurface flow about which Runcorn, Wilson and Girdler write. However, it is possible that the origin of the drift of continents and the subsurface flows is not convection but change in the radius of the earth as was proposed in the geotectonic theories of Academician Obrutschev (1940), M. A. Ysov and W. Bucher. This question is examined in connexion with the most recent theories of gravitation in work published by myself & Trapisnikov (1963).

(3) Changes in the difference of geographical latitudes of observatories of the Pulkova near Leningrad and at Ukail, western U.S.A. according to astronomical observations from 1900 to 1950 show that these points are approaching each other with the speed of about 10 cm/y (see Kropotkin 1960).

In my opinion, the drift of continents represents a problem in which the co-operation of scientists of different countries can be especially fruitful, and I believe that it would be possible to arrange such a programme. As Professor J. T. Wilson has observed, it has great significance for several branches of science.

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Professor J. D. Bernal, F.R.S. (Birkbeck College, London):

Most of what I have to say has already been said by Professor Orowan. The subject is the question of movements on the two dissimilar sides of continental masses.

Figure 1 is a picture of what I thought of the structure of the Earth about three or four years ago (*Nature, Lond.*, **192**, 123–125)—not altered very much since, in my opinion.

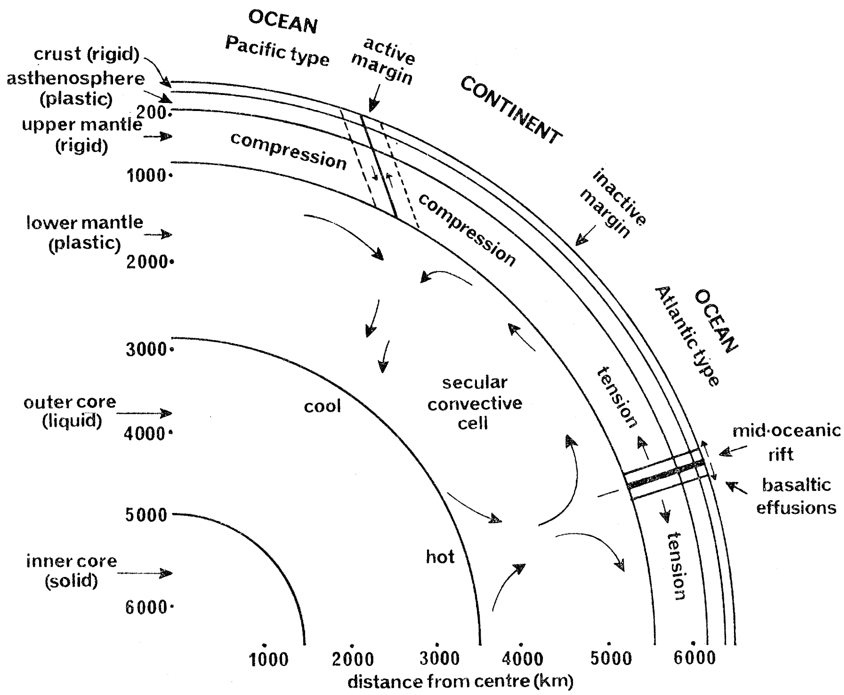

FIGURE 1. Schematic Earth section showing one secular convection cell in plastic lower mantle and its reactions on the more rigid upper mantle and through the asthenosphere to the crust. On the right is shown, corresponding to a rising mantle current, a mid-oceanic rift through which magma derived from the lower mantle, directly or indirectly, exudes to the surface through successively opened and consolidated tension cracks. On the left is shown, corresponding to a sinking mantle current, an advancing continental edge with trench and mountain building system with the oceanic side of the upper mantle block thrust down into the lower mantle. Between is the inactive continental edge marked only by an increased thickness of the crust and the corresponding thinning of the asthenosphere.

The first point I want to make to back up Orowan is that there are really two parts of the mantle and we ought to distinguish them. The main distinction occurs at between 700 or 900 km down, below which earthquakes do not occur. In this upper part of the mantle, we can distinguish two sets of forces, a compressional force under the mountains at the edge of the continents, which operates across the thrust plane at about 50° from the horizontal, and a tensional force in the middle of the ocean basins forming the mid-oceanic ridges.

Figure 2 shows a detail of the upper layer. Here I want to put certain questions—I cannot say I have the answers for them. The first question is 'Does continental drift only apply to the crust?; does the crust slide over on the asthenosphere or does the movement extend

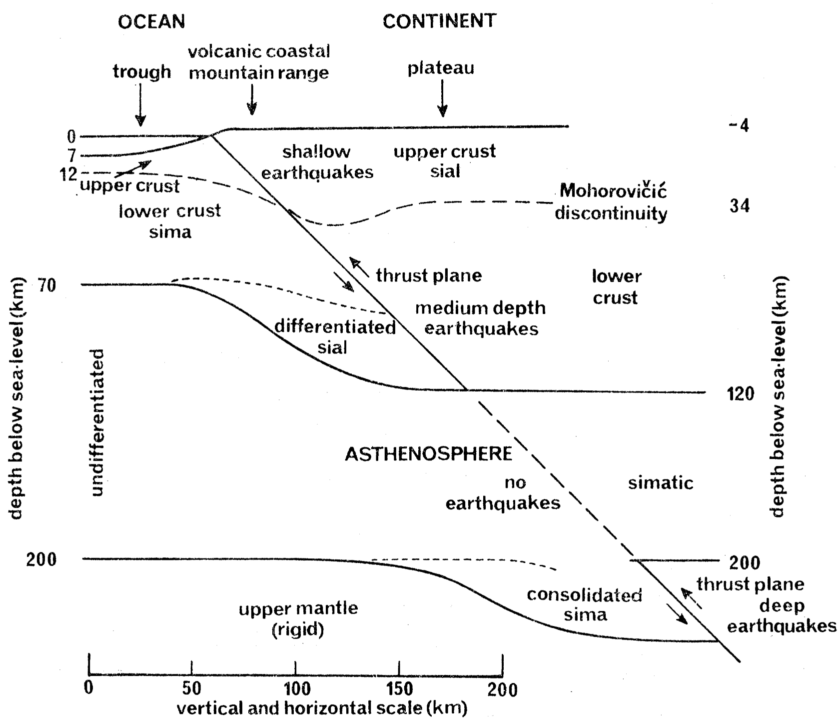

FIGURE 2. Section of an idealized continental thrust plane system illustrating Dietz's hypothesis of continental sialic differentiation. The upper block of asthenospheric sial will in the actual case not be so clearly demarcated from the basal crust as is shown, nor will the corresponding block of sima from the sinking upper mantle. The former, as it is differentiated, tends to rise vertically to jam with, and push up, granite batholiths. The vulcanicity associated with the mountain building is assumed to arise from the activity of the thrust plane itself.

as shown in figure 2, right down to the base of the upper mantle?' There is some evidence for this to be derived from the study of distribution of earthquake epicentres indicating that the plane of weakness cuts right through the asthenosphere into the upper mantle. It would appear that no earthquakes originate in the asthenosphere itself.

Another aspect of the question which I cannot answer, although Orowan might, is: 'Is it possible to maintain a single yield plane operating through two rigid and one plastic layer?' If so then the whole of the mass moves. If not, only the crust moves but the deep focus earthquakes would be difficult to account for. Another related point is the differential chemical composition of the crustal layers between oceans and continents. This has not been discussed at all here so far though it seems to me fundamental to the question of continental drift.

The hypothesis I put forward is illustrated in figure 2. The only movement that can transfer material horizontally or circumferentially is that across the thrust planes. It is suggested that it occurred in the differentiation chemically in the asthenosphere, which would tend to form differentiated light sial at the top and sima at the bottom; as the thrust plane moved the sima would be built in under the upper continental crust just beneath the Moho, while the sial would push further, consolidating the materials of the upper mantle. According to my definition, which I think more logical than that generally accepted, I take the upper mantle to be that part of the mantle below the asthenosphere and reaching down to the plastic lower mantle. The part below the Moho, between that and the asthenosphere, I prefer to call the lower crust. As successive thrust planes occurred, in accordance with Van Bemmelen's hypothesis, more differentiation would occur. Gradually a larger part of the undifferentiated asthenosphere would find itself divided, an enriched sialic portion under the continents and a depleted simatic structure in the ocean basins.

This is, of course, only a hypothesis and I am unable to cite any direct evidence for it, but it appears as a plausible explanation of a number of allied phenomena. It would not, of course, be required if the uneven distribution of sima and sial was there at the start.

Another point I want to make concerns the question of the relation of earthquakes to the formation of volcanoes. Some ten years ago I made very rough calculations which seemed to indicate that the amount of energy involved in volcanoes was of the same order as that generated in earthquakes. This was later put in a more precise form by Sir Edward Bullard but later, I understand, abandoned by him. The picture began from the question of what happened to the energy involved in the great movements along the thrust planes. I argued that the only way of generating heat on a large scale from mechanical energy was through plastic deformation. Heating would occur only if there was a large force operating for a large distance. In actual breakage the distance would be too small: in liquid flow the resisting forces would be too small. I was also impressed by the low efficiency of thermal conduction of heat and consequently most of the heat generated by plastic flow would raise the local temperature to such an extent that softening or melting would occur, thus bringing the process to a stop. The resulting heat could produce pockets of magma which would, by stoking processes, work their way up to the surface and form volcanoes.

It was, I understand, Gutenberg's later results which caused Bullard to abandon his hypothesis because the integrated energy of the earthquakes derived from seismography appeared to be of an order or more less than the energy appearing in the volcanoes. Nevertheless, I still think that his original hypothesis was correct and that he abandoned it because of neglecting an effect which is not generally known and which I came across more than twenty years ago in my study of explosives. It appeared that explosive or elastic energy liberated in a solid medium which has a certain degree of plasticity is only partially converted into elastic waves. Most of it would appear as plastic waves of lower speed and heavy and short-range damping. In work with explosives this plastic component, which depends on the nature of the material, could amount to as much as twenty times that liberated in elastic waves. A calculated figure of earthquake energy could be modified by a factor of this order to find the amount of heat required to supply the energy of volcanoes, with, of course, an enormous margin of error.

Such a theory would naturally account only for volcanoes associated with continental edge thrust planes or in the island arcs. Volcanic energy associated with rift volcanoes may have an entirely different origin related to the release of pressure produced by cracks which you have just heard about from the study of Iceland through which basaltic magma was forced up by purely hydrostatic pressure to a high level.

Professor M. G. Rutten (University of Utrecht).

In view of Blackett's introductory remarks on the violently opposed continental drift discussions during the 1920's, subsequent papers of this symposium have shown that, apart from paleomagnetic data, nothing much has been changed. The geological—and this includes geophysical and geochemical—arguments pro and contra drift are still based each on their own particular sets of data. These either 'prove' or 'refute' drift, but cannot be readily compared with those data used in the opposite camp.

To give two examples only, the papers by Wilson and Bullard will be cited. Wilson, to obtain a good fit between continents, does away with the Caribbean and most of Central America. This is done, 'because the area is young, the sea having reached Cuba only during the Jurassic'. Of course, this does not exclude the area being formed by older continental masses, which have since foundered, such as is presumed by Butterlin. This alternative hypothesis could incidentally well explain the rock salt layers supposed to exist underneath the present floor of the Carribean by workers of Lamont.

Bullard, to obtain a nice fit of continents, does away with Iceland, 'because it is young', but keeps the Rockall bank, 'because it fits nicely'. The older part of Iceland belongs to the early Tertiary, as dated on paleobotanical evidence from Heer. This is just as old as the 60 My absolute dates for the Rockall bank supplied by Miller. Moreover, the eastern part of Iceland, as studied by Walker, is truly continental in its volcanics.

The inheritance from the twenties is clear. It still depends on which part of the geological data one finds most strongly heuristic, if one is a 'drifter' or a 'fixist'. It is only the measurements of paleomagnetism which have introduced a really new set of values. The only way to remain fixist now, is to disbelieve paleomagnetism, a position which becomes more and more akward as its methods tend to become better substantiated. In future we shall have to base all of our geological theoremata on the data supplied by paleomagnetism.

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