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

Discussion

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XVIII. Discussion

Professor L. U. De Sitter (University of Leiden) was invited to open the discussion on continental drift and structural geology:

We have heard many excellent arguments in favour of continental drift, based on the most recent results of studies of the ocean floors, the fit of the continents, the palaeomagnetic picture, and several instances of the relation between geological features and the supposed movement of the continents.

It has struck me that these geological features are very restricted in number; they are either the oceanic rifts or wrench faults.

Let us have a look first at the oceanic rifts. They are directly connected, through the Carlsberg Ridge and the Gulf of Aden with the Red Sea Rift and then through the Ethiopian faults with the famous African rifts. The history of the African rift system is relatively well known, and we know for certain that they represent principally vertical movements of the Earth's crust, which have lasted at least from the Tertiary and probably since the Jurassic.

From other rifts, the continuation of the Upper Rhine Rift in the Netherlands, for instance, we know that the movements along its border faults date from the beginning of the Mesozoic. In all that time the total horizontal dilatation movement of the crust across the rift, of which the vertical movements are the result, is not more than a few kilometres at the utmost. The vertical movement is of the same order. Their function in the deformation of the Earth's crust is therefore completely different from that postulated for the ocean rifts.

Large wrench fault movements are difficult to prove, and the Great Glen fault, the Jordan fault and the San Andreas are really the only ones where good evidence for movements of the order of 100 km or more has been brought forward. For this Scottish fault and for the Jordan fault it is certain that the movement happened in one relatively short phase and therefore can be of no significance for continental drifting over long periods.

The horizontal slip of many hundreds of miles which are postulated on ground of the magnetic pattern to have happened along the fracture zones of the eastern Pacific looks rather improbable, because these faults butt against the continent and can not be traced as major faults on land.

The horizontal slip along the fracture zones crossing the Mid-Atlantic Ridge between Brazil and west Africa seems to be well proved, but this does not substantiate the drifting of Africa away from South America; on the contrary.

Besides these wrench and normal fault phenomena we have ample evidence of important thrust-faulting in every part of the world, a phenomenon which apparently is not related to drifting continents since it has not been mentioned so far.

A much more important structural feature is the mountain chain, with which we geologists are mostly concerned. An ortho-orogene consists of a metamorphic core and marginal or external zones on one or on both sides. The total compression is of the order of 200 km perhaps and the history of its formation stretches over many folding phases of at least 20 to 50 My. The energy needed for such a compression of a narrow belt of the Earth's crust is at least 100 times larger than that needed for a rift or even a wrench fault and the heat energy for its metamorphism is even larger. During this metamorphic stage the temperature front rises in domes to very near the surface; enormous rock masses are heated up to 600 to 700 °C, they are eventually melted and then from deep down in the crust intrude upwards as plutons and finally may cause extensive volcanic activity. Altogether a much larger manifestation of heat flow to the surface than any oceanic rift can show. Nevertheless, the orogenes are supposed to be located above the downward directed coolest part of the supposed convection currents and the rifts above the hot upwards directed flows.

The present structural pattern of the continental part of the Earth's crust is primarily defined by the orogenic belts of its younger Alpine history. It shows one circum-Pacific belt and one Mediterranean belt reaching from the Atlantic to Australia. If this pattern has anything to do with continental drift and convection currents, then there must be two convection cells in the mantle, one welling up in the centre of the Pacific and coming down on its borders, the other converging and going down in the Mediterranean belt, which is of course in complete contradiction with the present views about continental drift.

The only conclusion one can draw from these facts is that apparently orogenes are not related to the drift of continents, a conclusion which is substantiated by the fact that Africa, the continent which is the favourite floe with a west–east drift, does not have any young mountain chains except on its northern and southern borders. The deformation of the continental crust apparently has another origin of completely different character. I wonder if one could not correlate the long quiet time intervals between the orogenic periods with drifting and these orogenic periods with the intervals between different sets of convection currents.

Professor H. W. Menard (University of California):

The significance of the evidence for movement of volcanic islands adduced by Professor Wilson may be questioned on several grounds:

(1) In the Pacific, he has suggested an ingenious explanation for the common occurrence of atolls at one end of an archipelago and active volcanoes at the other. The volcanoes are thought to be carried away, by convection, from a hearth in the mantle and to become inactive and subside as they move. A critical test of this hypothesis is given by the Austral or Tubuai Islands. They display the usual atoll to recent volcano pattern above sea level. However, below sea level and between the islands are several large guyots at a uniform and great depth. They must be older than the existing islands and therefore vulcanism and subsidence have not proceeded in any simple manner from one end of the chain to the other.

(2) The supposed correlation between age of Atlantic Islands and distance from the Mid-Atlantic Ridge is doubtful in itself and, if correct, does not require migration of volcanoes from a hearth at the crest of the ridge. The spread of points in the correlation graph is large if the whole Atlantic is considered. It might be expected that a better correlation would occur if the best known regions were considered independently. Quite

the contrary is true. For example, at the latitude of Iceland, early Tertiary volcanic rocks occur in the centre and on both edges of the ridge. Likewise, at the latitudes of the Azores, middle Tertiary volcanic rocks occur in the centre and on the flank of the ridge and also at the edge of the ocean basin. These distributions can be explained with greater ease by contemporaneous vulcanism in an extensive province, than by continuing volcanism in a limited area. Even if the oldest volcanoes are significantly concentrated at the margins of the Atlantic basin, this does not prove migration from the centre. Several of the oldest volcanic groups are still active and therefore have not moved from the region of volcanism if they have moved at all.

(3) Another line of evidence suggests that volcanoes have not migrated from the shallow crest of the Mid-Atlantic Ridge to the deep flanks of the ocean basin. This path of movement would produce an average submergence of about 2 km which should drown the shorelines and produce other conspicuous effects which do not occur.

Mr P. Henderson (University of Oxford):

A study of partition coefficients of trace elements between a mineral and the mesostasis of the basalts of island pairs about the Mid-Atlantic Ridge and the East Pacific Rise, may lend further support to the data and ideas presented by Professor Wilson on the increasing age of the islands with increasing distance from the rises; i.e. each island of an island pair should be expected to have very similar or identical partition coefficient values. Furthermore, if the suggestion that the straight chains of young islands are streamlines fed by lava rising from deep within the convection cells is true, then a study of changes in trace element concentration and partition coefficient values, with distance from the rise, may well indicate the trend of chemical differentiation within the mantle and temperature changes of the convection cells with geological time.

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

A general remark is that the area in eastern Iceland where Walker worked is completely different from central and western Iceland in two ways. One is in the occurrence of dyke swarms, the other in the prevalence of acid volcanites, even of ignimbrites, in the east. It somehow looks as if eastern Iceland is truly continental, comparable to, for instance, the Permian Oslo volcanism, whereas central and western Iceland are of a more oceanic character. The transition is formed by a narrow north–south strongly tectonized zone. This runs well east of the present central graben, from Egilstadir in the north, to the eastern border of Vatnajökull in the south. An extrapolation of the results obtained in eastern Iceland over the rest of the island is, consequently, not permitted.

In regard to the theory that the dyke swarms in eastern Iceland indicate rifting due to continental drift, an alternative hypothesis is put forward that they are due to local volcanotectonic phenomena. In favour of the latter idea is that elsewhere in Iceland there is no stringent relation between a thickening of lavas and the dip, hence there is no relation between the total thickness of the lava pile and that of the dykes. Moreover, the dykes are younger than the lavas they intrude by at least one geomagnetic period. Therefore they have not formed in open fissures at the time of the lava formation. In addition, they normally dip somewhat less than perpendicular to the tilted lava pile they intrude, which also points to their later formation in tension cracks opened through volcanic tectonic movements.

Professor M.-J. Graindor (Collège de France, Paris):

The evidence that has been pointed out during these sessions are of great interest for geologists. However, some of the schemes which have been proposed may not be convincing, such as those where continents are considered as if their surfaces have remained unchanged through geological times, while, on the other hand, micro-continents are also recognized. It is my opinion that if we admit the formation of a crust from initial igneous matter, this crust has most probably appeared here and there as crystals in a cooling eutectic. If as geophysicists have pointed out there are convection movements, since the beginning of cooling the different crustal parts have been in constant motion, sometimes joining together, sometimes drifted apart; at no time would there have existed a continuous crust over the Earth's surface. In time the increasing amount of crust and mantle reduce the movements. By the same process the surface of the continents is increasing or tends to increase, but this may be prevented by convective movements pulling down continental crustal parts. The continental increasing is a consequence of internal energy that gives crustal rocks (intrusive, effusive and sedimentary rocks as well).

But whatever may be right or wrong in this hypothesis, there is a fact that geologists must consider more than they have in the past, that the continents are not single masses but are made of a jig-saw of several different parts which have different characteristics and have evolved differently during geological times; they are basically different and have borne the same features only since definite eras, more or less recent. The more we know about the geology of the lower stratigraphical parts of a continent the more complicated does the continent appear. This could be easily proved not only for Fenno–Scandia but also western and central France as well as for the Bohemian area.

I believe that one of the most important contributions that geologists could make to the question of continental drift would be the identification of the genuine parts of each continent. Between these different parts are cicatrices that can be recognized either by linear magmatitic structures or by large cuts appearing at the surface of the Earth as large faults (transcurrent faults, strike-slip faults and thrust faults). When we can get evidence of the geological evolution of each side of these mains features it is often clear that this evolution is similar only more or less recently; lower, in the geological scale, great differences appear; so the evolution can be considered to be identical only since the joining of the two parts. Naturally, other explanations have been proposed to explain such differences, especially palaeogeography, but many of them are not satisfactory and are seldom in general agreement.

One of the most valuable contributions, then, for geologists to make would be in connexion with the several ways of research proposed in this meeting, an exhaustive analysis of the different cratonic parts of each continent. When these different parts had been identified, then could be successfully applied the different methods of palaeomagnetism, palaeoclimatology, etc., from which, one may be sure, evidence would more clearly appear. Western Europe seems to me one of the most convenient areas for such an attempt on account of its well known geology; it is one of the most well known parts of the world.

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Mr W. B. Harland (University of Cambridge) discussed crustal compression in relation to convection currents:

Economy of hypothesis requires that if convection currents are invoked to move continents, by evidence other than compression structures, then compression structures (which in sufficient degree demand continental drift independently) should also be explained by convection. Indeed, every hypothesis of convection currents has included some degree of tectogenesis as a consequence, whether or not continental drift has been invoked on a considerable scale.

Wegener viewed the continents as sialic rafts floating and travelling in a simatic sea. This aspect, perhaps more than any other, accounted for the opposition his hypothesis encountered, since in its extreme form the sima (basaltic crust and upper mantle, with a higher melting point and a lower radiometric content) was too strong to allow sialic rafts passage. Some additional confusion arose in so far as the terms *sial* and *sima* (geochemical composition concepts) also came to be used loosely for crust and mantle (seismological elasticity and density concepts), and for lithosphere and asthenosphere (mechanical strength concepts).

These confusions have now been largely resolved and the concept of convection currents in the Earth is generally understood to mean the convection of the whole mantle, on which the crust is transported passively as on a conveyor belt, and without distortion except in those zones where the 'mangle' squeezes out the buoyant sial from the downward current of sima.

Alternative (e.g. contracting and expanding) theories of the Earth have usually involved different mechanisms for mountain building. Intense tectogenesis has been regarded as either primary or secondary, depending on whether the mechanism is one of local crustal compression or the result of (secondary) gravity sliding from some (primary) uplift. Opponents of hypotheses of continental drift were strongly pressed on the amount of permissible crustal shortening, since it generally had to be explained otherwise as the result of contraction. Thus Jeffreys allowed a total of 300 km in his early edition of *The Earth*, and in the later editions he found a further 400 km to make 700 km. Even so, for the whole of Earth history this is little enough, considering that some authors have claimed 400 km for the shortening of the Alps alone and others have claimed more than 1000 km. Lees (1953) was one of the few recent writers who honestly accepted the abundant evidence of crustal compression and attempted to fit it to a history of substantial contraction, yet without any adequate theory to account for this.

Reluctance to admit continental drift therefore led to a tendency to underestimate crustal shortening, and in particular to the encouragement of hypotheses of gravity sliding. A whole range of hypotheses accounted for compression structures as secondary tectogenesis, either as a result of primary compression and uplift (see, for example, Jeffreys 1935), or as a result of differentiation and generation of undations or asthenoliths, of which the latest major exposition was by Van Bemmelen (1954).

On the other hand, assuming continental drift by convection, all shortening of strata by folding, etc., is simply compensated for by extension elsewhere (Holmes 1933). Yet there has been some reluctance among supporters of continental drift by convection to exploit the evidence of compression structures; for example, recent statements by Bullard (1964) and Wilson (1963) outline at some length the evidences from extension and transcurrent shear zones, but hardly consider compression as useful.

If areas of crust are to move with respect to each other, one would expect marginal zones of extension, strike-slip, and compression. Moreover, if two or three of these can be demonstrated surrounding a particular area, its drift is more easily inferred.

Extension structures have been most difficult. Continental graben were always suspected of a minor role and the Red Sea rift system was generally suggested (Wegener, Holmes, du Toit) as incipient ocean. Thus the main extension could only have taken place under the oceans where, until recently, evidence was inconclusive.

Strike-slip faults are typically high angle, and repeated movement may increase the structural contrast on either side and produce a fundamental fault which is liable to move on any subsequent occasion under renewed stress, being a zone of weakness extending through the right crust. There is, indeed, no physical limit to the amount of relative horizontal displacement possible along a transcurrent fault system. And the larger the amount of displacement the more difficult may it be to match once contiguous structures if they were widely separated and then underwent different histories of vertical movement, deformation, etc. Because of the concentration of so much movement in a single zone, larger transcurrent faults may be extremely dramatic but are also liable to be obscured and completely missed.

In this respect strike-slip differs from dip-slip, for the latter cannot shear indefinitely along one surface—indeed, new faults form successively under prolonged stress. Thus extension and compression structures are more generally distributed, each shear being of smaller displacement.

Compression structures may thus affect a substantial volume of continental material, so that through considerable areas, and at successive denudation depths, evidence of local shortening is manifest, even in older Precambrian rocks.

The stratigraphical dating of compression structures is easier because older and younger rocks are generally distinguishable and there is a great volume of rock in which to seek the youngest deformed strata and the oldest undeformed strata. Moreover, radiometric ages date associated metamorphic events and also, to some extent, plutonic igneous events which may be closely associated.

In estimating the amount of crustal compression, and thus the direction of relative movement, there are three hazards. (i) Gravity sliding may have folded rocks at the foot of a slope with a corresponding extension at the original top of it. An element of gravity sliding is inevitably present in large tectogenes, for some uplift produces unstable slopes. But this can be discounted in the deeper structures which have been later uplifted. (ii) Plutonic addition may have squeezed rocks to one side. (iii) Tectonic thinning of strata may exaggerate the apparent shortening. The style of folding, however, will suggest caution where necessary. On the other hand, experience shows that most areas on closer investigation yield more complex structures with consequent increase in the evidence for shortening. Arguments about this may in some cases be inconclusive but in many the evidence is good for a minimum figure.

Whereas local crustal shortening demonstrates at least so much continental drift, drift is not necessarily accompanied by equivalent evident compression—which depends on suitable sialic rocks (e.g. a geosyncline) being squeezed, uplifted and eroded to view. Evidence of compression beneath the oceans may be sunk without trace.

Tectogenesis, then, gives an indication of the *minimum* amount, direction, and time of displacement, so the distribution of compression structures gives at least a pattern of drift and is important because of this. Any palaeogeological reconstruction must begin by taking such compression into account and restoring the deformation. Many anomalies between palaeomagnetic results may be explained by some degree of compression and distortion within a continent and, indeed, palaeomagnetic estimates of crustal movement may be tested against, and be amplified by, the distribution and intensity of fold structures.

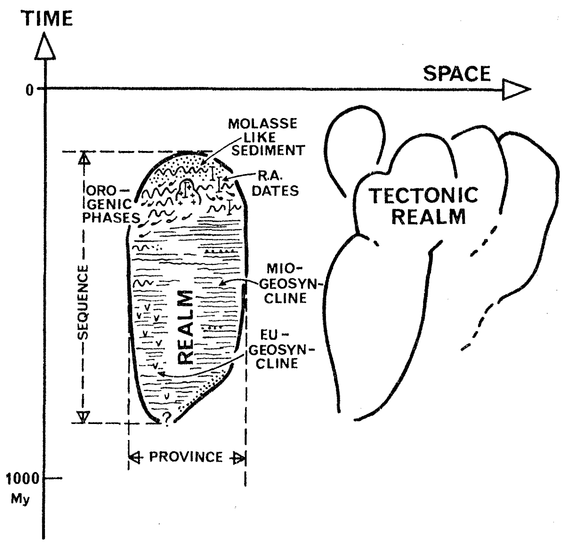

FIGURE 1

A good example is that of the Tertiary closing of the Tethys by the northward drift of Africa and India. Palaeomagnetic data suggest that each moved north independently and (on meeting Eurasia) each twisted—Africa clockwise and India anticlockwise. A consideration of the stratigraphical tectonics of the Alpine Himalayan fold belt in, say, Oligocene time suggests maximum compression in the Alpine and Himalayan regions but very little severe compression of that age in the Middle East. At the same time the rift system in east Africa was opening up. This story can be followed out in more detail and continental movements can thus be tested against palaeomagnetic and tectonic data quite independently.

Hutton first recognized the geostrophic cycle of successive revolutions and Hall and Dana in the 1850's demonstrated the association between geosynclines and orogeny. This intimate connexion, which gives to a structural province its distinctive sequence for a large span of geological time, is probably explicable in terms of convection cells, potential and actual. We must seek subcrustal units in space and time commensurate with the tectonic realms recognizable at the surface. A typical realm shows a long geosynclinal sequence culminating in compression and is illustrated diagrammatically in figure 1. This calls for a distinctive mechanism such as cooling and contraction beneath the geosyncline (?aided by phase change intermittently) until the contrast of hot and cold columns was sufficient to turn the convection cell.

It seems that tectonic history is largely expressed at the surface in a series of related, developing, anastomosing and decaying tectonic realms. A diagrammatic pattern is suggested in figure 2. This may reflect a changing pattern of convection currents.

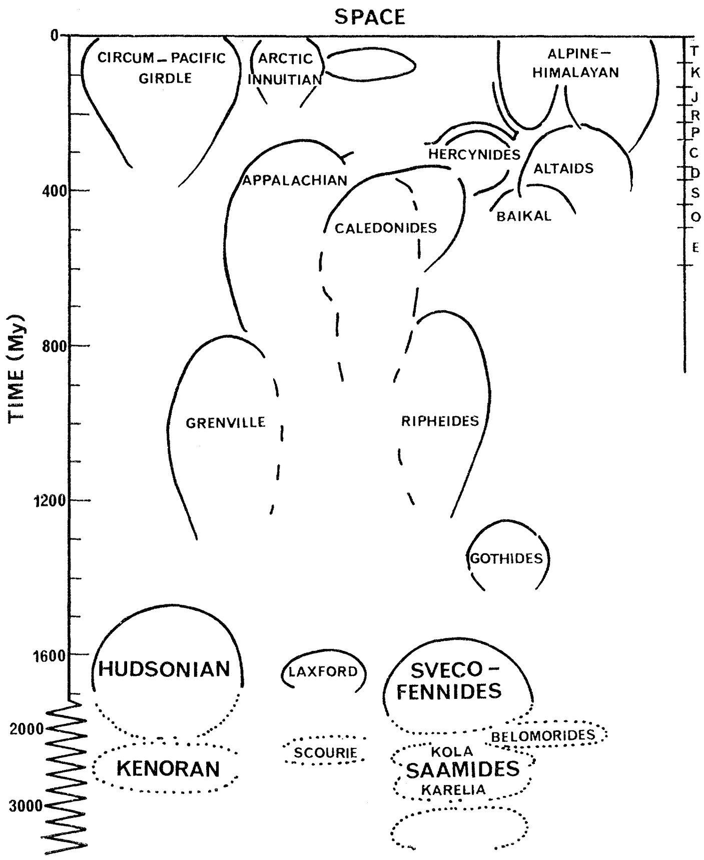

FIGURE 2.

References (Harland)

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Dr M. H. P. Bott (University of Durham) emphasized the importance of partial fusion in relation to the mantle convection hypothesis. Conditions exceptionally favourable to partial fusion of large scale would be expected to occur in the topmost section of a rising convection current in the mantle, since temperatures at a given depth should be a maximum here and since the fusion gradient is an order of magnitude higher than the adiabatic gradient.

Some of the magma thus generated should move upwards (owing to its low density) to form magma chambers and networks at a shallow depth, explaining anomalously high heat flow values and abundant igneous activity of those regions (such as Iceland and the Mid-Atlantic Ridge) which have been thought to overlie rising convection currents. The heat of fusion of magma remaining trapped in pockets within the convection cell would contribute to reducing the progressive falloff of heat loss as the current flows horizontally. Partial fusion may also reduce the strength and effective viscosity of the boundary layer.

It was also pointed out that it is difficult to understand the present pattern of oceanic heat flow on the convection hypothesis unless the convection cell is overlain by a non-convecting layer, which magmatic considerations place at about 70 km in thickness for present heat flow.

Dr J. A. Miller replied to the question regarding the validity of including the Rockall Bank while removing Iceland in the reconstuction of the North Atlantic:

The reason for including the Rockall Bank in the reconstruction of the North Atlantic is given in my paper. As is stated, the evidence is by no means conclusive but it does suggest a geochronological affinity with the Tertiary granitic rocks of Scotland. Further information might be yielded by an investigation of the initial $^{87}Sr/^{86}Sr$ ratio of the rock which could indicate whether the material is re-worked continental basement or the differentiate of an oceanic basalt.

With regard to Iceland, Walker (this Symposium) has shown that the island has dilated about 30m in the last 3000 to 5000y. Recent experiments in the Cambridge geochronological laboratory have shown that many lava flows date from less than 1·5 My. It is therefore safe to conclude that the island as a whole has become larger during the Tertiary period. To include Iceland at its present size in a reconstruction of the North Atlantic would not be valid. In view of the fact that the exact size of 'pre-drift' Iceland is not known, it has been neglected completely.