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Geological Evidence Bearing upon Continental Drift

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II. Geological evidence bearing upon continental drift

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Several kinds of geological evidence bearing on continental drift, apart from palaeomagnetism, will be discussed. The most obvious is the fitting of continental margins, as around the Atlantic, with excellent shape-fit and with truncated geological structures and distributions dating from before the drift becoming continuous, when supposedly sundered continents are restored to presumed original relationships. Fold belts, areas of similar sedimentary and petrographic history, and other criteria may be used; geochemical provinces may also have value. Negative evidence may include the lack of records of marine invasions from the directions of the present adjoining oceans.

After separation a new pattern of sedimentary accumulation in marginal and transgressive seas may become dominant; such sediments often blanket the older evidence.

Palaeontological evidence must be used with caution. So must the present distribution of plants and animals. Some criteria of fossil evidence likely to be of value will be discussed.

Indirect evidence bearing on the problem includes the implications of large scale faulting, especially transcurrent (strike-slip) faults, and of other large scale tangential movements of the crust, and the overwhelming evidence from many aspects of geology of subcrustal plasticity, a necessity if drift is to occur.

Any single piece of geological evidence falls short of proving drift. The most useful information comes from the convergent evidence of many independent facts that may individually be regarded as having only a modest probability of being due to drift, but which in combination can hardly be explained in any other way.

1. Introduction

Continental drift, surely one of the most striking theories concerning the history of the Earth, has a very respectable pedigree going back at least to Francis Bacon. Its modern development stems from the work of Taylor (1910) and Baker (1911), but especially from that of Alfred Wegener (1912, 1915, 1924), whose remarkable synthesis concerning the breakdown of 'Pangaea' in Mesozoic and later times became the subject of heated controversy. The many telling criticisms of aspects of his very comprehensive theory have not diminished Wegener's very special position in the history of the subject. Many discussion meetings and symposia on continental drift have revealed a great cleavage of opinion among geologists on almost every aspect of the subject, from the assessment of the basic evidence for former continuity and 'fit' of continents to the nature of ocean floors and the possibility of land bridges, or from the dating of the supposed movements in the geological time-scale to the validity of any suggested mechanisms. Some idea of the variety of views expressed may conveniently be obtained from van Waterschoot van de Gracht (1928), du Toit (1937, excellent coverage of previous history), Longwell *et al.* (1958) and Munyan (1963). Opposition by geologists to the general concept of continental drift has often involved several deep-seated geological prejudices and limiting factors—notable being the strong appeal of uniformitarianism, the caution natural to good field-geologists who are well aware of numerous uncertainties of interpretation in the complications of regional geology, where individuals can hope to study only a small part of the evidence, and the deference accorded to geophysicists of great repute by non-mathematical geologists.

A standard work of enormous influence for some decades has been Jeffreys's *The earth* (1952, 3rd ed.), in which geophysical arguments seemed to many geologists to disprove the possibility of crustal movement of the kind required. Equally, as will be seen, too enthusiastic support for drift by geologists and biologists has involved uncritical reliance on the distribution of extinct and living organisms, selective use of parts of the available evidence, and of course the desire to controvert the conservative pundits. It is, however, a very pertinent fact that among the most stalwart upholders of drift are geologists with a good first-hand knowledge of geological phenomena in two or more of the supposedly sundered continents.

2. The reality of 'fit'

Perhaps the most fundamental, and historically the earliest, approach to the problem is the accuracy of 'fit' along the lines of supposed fracture and separation. Ideally, one should be able to see a complete correspondence, both in shape and in geological patterns, when the continents are restored to their original position. For reasons now to be outlined, this ideal cannot be fully attained.

It is now well known that continents extend beyond their coastlines. The main problem is to define the submerged margin accurately. While it is a good generalization that the 'continental shelf' is essentially the submerged margin of the continent, which begins to break down into the steeper 'continental slope' near or below the 100-fathom (200 m) isobath, this is neither universal nor sufficiently critical, since the submarine surfaces have received much sediment since any possible drift occurred. In practice, however, the 200, 500 or 1000 m (or the 100-, 500- and even 1000-fathom) isobaths are so close and nearly parallel, because of the comparatively steep gradient of the average 'continental slope', that one may use any one of them without gross error. It is essential that any work should be done on an accurately scribed globe of respectable size, with cut-outs representing the continents made from thin sheets accurately moulded to the same curvature. When this is done the accuracy of fit in certain cases is truly remarkable and would alone be difficult to explain by pure chance; thus one can get an excellent fit of South America and Africa, allowance being made for the more recent Niger delta, in spite of Jeffreys's claim (completely refuted by Carey 1955*a*, 1958) that there is a 15° disparity in shape. Similarly, the Labrador–Greenland–northwest Europe concordance of fit is exceptionally clear. It is not possible to obtain a unique fit by shape correspondence where the respective margins are more or less 'straight' or evenly arcuate, and the borders of Antarctica have posed very special problems because of ice-cover.

Shape fit is merely the first criterion. There should also be evidence of truncation of geological structures already existing before sundering. It is surely no accident that all the continental margins concerned in the most likely reconstructions are 'Atlantic-type' margins. The detailed evidence one would wish to have to prove corresponding structures at the margins is unfortunately largely concealed in the actual marginal strips by the sea, by sediments on the continental shelf, or by sediments of coastal plains and similar deposits. Nevertheless, various geophysical exploration techniques, as for example along the eastern seaboard of the United States, have confirmed and extended borehole evidence that an 'old' continental surface can be traced towards the continental slope beneath a veneer of sediments of strictly limited backward range in time. It is potentially possible to map the gross distribution of certain materials of differing physical properties under such cover by geophysical methods. In a few cases submarine topography of the shelf, combined with dredged samples, gives strong indications of continuity of geological structures such as belts of folding from the exposed continental surface towards the edge of the shelf. The mapping of the continental structures under these marginal areas is, however, largely a matter for future research.

It is thus evident that *detailed* matching of structures is not yet possible. To assess the validity of the comparisons that can be made, it is necessary to have a very good knowledge of the whole geological structure and history of the continental areas adjacent to the marginal veneers of sediments and seas, and to assess the degree of continuity and structural and historical similarity that they show with corresponding areas on the other side of the presumed break. Since the hidden marginal strips may often be a few hundred kilometres in aggregate width, it is equally necessary to have a good knowledge of the persistence of characteristic structural units (such as fold-mountains) over similar distances, with detailed information concerning changes in direction, fold styles, and geological dating of the movements and other phenomena involved. The accumulation of such evidence is slow, and very different standards of precision have to be accepted. Furthermore, the interpretation of the structure and structural history of even a well defined fold belt is liable to change with new methods of research. What was regarded fifty years ago as a reasonable summary of the history of sedimentation, folding, magmatism and metamorphism of 'fold-mountains' such as the Alps and the Scottish Caledonides is now known to be drastically oversimplified and in some ways quite incorrect. It follows that comparisons are often made on published evidence of different quality, and representing different schools of thought.

So far the evidence considered has been essentially structural; continuation of fold-mountains (however reduced by erosion) of similar structural history, etc., old basement complexes with similar structural, metamorphic and magmatic history backed by adequate radiometric age determinations, and possibly the similarity of geochemical provinces within such basements, may be acceptable evidence suggesting former unity of supposedly sundered continents. But none of these alone is worth much as *proof*. This is especially so if the ideas of Stille (1924, 1936, 1940, etc.) concerning the wide contemporaneity of short episodes of earth-movement are accepted (but see, for brief review, Westoll 1954). If indeed there is a real 'pulse of the Earth' (Umbgrove 1947) then parallel sequences of generalized earth-movement may be valueless in themselves as evidence in our context, though the *expression* of such movements in different intensities, fold styles, linear extension, etc., may still be valid. Indeed it remains very highly probable that a series of close similarities in spatial and temporal expression of earth-movements of the same kinds, distributed along and on either side of a geometrically matching fit-line, could not be ascribed to chance, even if Stille's ideas have some validity.

3. Sedimentation, stratigraphy

Structural units of the kind already considered are generally belts of deformed rocks or confined areas of deformed rocks. A different kind of evidence is provided by regional stratigraphy and sedimentation; both positive and negative aspects may be distinguished.

The negative aspect, naturally the most fragile since it may be destroyed by future discovery (though in well known areas this possibility is slight) concerns the absence of signs of repeated marine invasions near what is now an oceanic coast, prior to the presumed drifting. For example there is no evidence of any marine invasions around the southern part of Africa (with the exception of the Bokkeveld beds, etc. and the transient Upper Dwyka incursion in the Cape area) before the Jurassic at earliest, and the earliest widespread marine deposits near the present coasts are of Cretaceous age. (The Bokkeveld beds represent an episode in the filling of what du Toit has called the Samfrau geosyncline, and the distribution of the immediate correlatives (with essentially the same fauna) in South America and the Falkland Islands has been used by du Toit in his reconstruction. This geosynclinal episode, according to du Toit, was finally terminated by folding and the resulting fold belt fragmented by drift.)

Along the eastern seaboard of the United States there is a marginal strip with a veneer, wedging to thin edges westwards, of marine and coastal-plain deposits of Lower Cretaceous and later age, the whole resting on an eroded basement of Palaeozoic and more ancient rocks, sometimes with the intervention of continental Triassic deposits. There is no evidence for widespread Jurassic incursions, which might well be expected if the North Atlantic in its present form had been in existence. In the Palaeozoic the evidence of sediment distribution strongly implies the existence, not of an Atlantic Ocean, but of a high landmass ('Appalachia') to the southeast of the present shores. It is, to say the least, extremely doubtful whether there is room for such an area, however reduced by erosion, within the confines of the present continental shelf.

Furthermore, the comparative stratigraphy of the Palaeozoic of the North Atlantic borders is extremely difficult to explain if an Atlantic Ocean existed throughout the Palaeozoic. It is best explained by postulating elongated Lower Palaeozoic seaways, partly 'geosynclines', that come into remarkable congruence when the North Atlantic is closed up, and which run more or less obliquely to the present continental margins. During the Lower Palaeozoic there is a very remarkable similarity of sedimentary history in the New England–St Lawrence area, for example, as compared with Britain and Scandinavia. For example, in the Cambrian, areas with essentially arenaceous and carbonate Lower Cambrian followed by carbonates with few fossils grading up into carbonates with uppermost Cambrian and Lower Ordovician fossils are found on both sides of the Atlantic and form a significantly congruous distribution if the continents are reassembled. To the southeast of such areas Cambrian rocks of more varied but essentially clastic character occur, and the two main facies are characterized each by their own suite of fossils. Such a distribution, if there were an Atlantic Ocean then in being, is extremely difficult to understand, especially when taken in conjunction with many similar pieces of evidence. There is, of course, at least one other hypothesis besides persistence of ocean basins on the one hand, and continental drift on the other, that can be entertained—a former continuity of structures across the present Atlantic, the present ocean being the result of foundering. This will be separately examined later.

The most detailed and wide-ranging examination of stratigraphical as well as structural evidence for former direct contiguity is still that of du Toit for South America–South Africa (1927, 1929, 1937). Here the evidence from the distribution, both of thick formations and groups of sediments and of the lateral variation of facies within them, is clearly interpreted as strong evidence of drift, since many of his boundary lines cross the re-assembled continents congruously, as do the older structural lines. Since many of these sedimentary formations are clearly essentially epicontinental deposits, this evidence, if accepted, is of particularly weighty value.

It would be idle to suppose that evidence of the kind mentioned is universally accepted. In particular, any single item of comparison, even when the similarity is clear, might be a mere chance resemblance. It is only where a very large number of similarities can be shown, and where opposing evidence is weak or can clearly be well explained in other ways, that stratigraphical evidence for former contiguity can be important. What is badly needed is fresh assessment of a lot of the evidence; this will require extensive field-work, considerably more demanding in terms of men, time and money than, for example, the collection of samples for palaeomagnetic studies.

One particular kind of stratigraphical evidence has loomed very large in this field, the evidence of former extensive glaciations. Wegener even supposed that the North Atlantic separation had been completed in Pleistocene times, and used as evidence the extent of Pleistocene ice-sheets. This is quite certainly incorrect, and has no more than historical interest; yet it does indicate that caution is needed in using similar evidence in the past.

Two other cases of the use of evidence suggesting former glaciation have played a considerable part in the literature of continental drift—the Permo-Carboniferous glaciation of Gondwanaland, and the late Precambrian glaciations, especially round the North Atlantic. The Permo-Carboniferous glaciation was fairly certainly not a single episode. It is generally agreed that somewhere about the very end of the Carboniferous extensive continental glaciation is clearly documented in South Brazil–Uruguay, the Falkland Islands, the southern half of Africa, India and Australia, and that either rapidly successive ice-sheets or competing ice-lobes produce a complex picture in the best known areas. The evidence that a true glaciation is involved is in many places overwhelming; the deposits, and the existence of typical ice-moulded glaciated pavements, compare very well with Pleistocene occurrences, and far travelled erractics are well known. In several areas, notably in the Cape Province, there is the good evidence suggesting the lateral passage of original continental tills into subaquatic deposits peripheral to ice-sheets. These may take the form of 'glacio-marine' deposits. Such deposits are now known to be very difficult to distinguish from others, as will be shown. Prior to the late Carboniferous Itarare glacials of South America, several early Carboniferous 'glacial' deposits have been described (see Caster (1952) and King (1958) for general reviews with key literature). Only a very few of these are widely accepted; nearly all require further sedimentological and regional study. Similarly, in eastern Australia the Permian 'Upper Marine Series' includes deposits often called 'tillites' or 'glacio-marine' beds but which are by no means certainly so. The very late Carboniferous to very earliest Permian glaciation (Itarare of south Brazil, etc.; Dwyka of South Africa; Talchir of India; Bacchus Marsh and corresponding deposits in Australia) remain valid, and their significance for continental drift has been recognized since the time of Wegener, and is well summarized by du Toit (1927, 1937, 1954), Maack (1952), Caster & Mendes (1948), Caster (1952) and King (1958). The combination of stratigraphy and palaeoclimatology is very strong.

A much more difficult problem is posed by the late Precambrian boulder-beds, remarkably similar to one another in character and stratigraphical environment, developed around the North Atlantic–Greenland, Ireland, Scotland, Norway and Spitsbergen. These boulder beds are usually interbedded in a sedimentary succession rich in carbonates, and usually display neither a striated pavement beneath, nor many of the sedimentological features of true tillites. They have, however, frequently been regarded as glacial in origin, presumably 'glacio-marine'. They show very remarkable resemblances to deposits in the Congo long regarded also as tillites, which have recently been re-described by Schermerhorn & Stanton (1963) as 'tilloids' (Pettijohn's term) resulting from submarine or at least subaqueous sliding of unconsolidated sedimentary material. The evidence against glacial origin is strong, and can equally be applied to the circum-North Atlantic occurrences, but there are also difficulties in interpreting the deposits as mud-slides (see, for example, discussion following Schermerhorn & Stanton's paper). It may be added that such deposits have been claimed as evidence for a remarkably widespread glaciation in late Precambrian times on the assumption that all are of late Precambrian age and can be correlated. The evidence for such correlation is far from reliable, particularly since more than one boulder-bed is present in many cases, but assuming for the moment that it is valid the deposits concerned are truly remarkable in their extent both locally and in their world distribution. While the late Carboniferous 'Gondwanaland' tillites correlate very well with high geomagnetic latitudes derived from palaeomagnetic studies, the late Precambrian occurrences round the North Atlantic emphatically do not, and workers with extensive field experience of them, like Mr W. B. Harland, have frequently suggested in recent discussions that they record a practically world wide, possibly multiple, glaciation reaching very low latitudes, which would, in our present knowledge, be unique. The present writer accepts the very remarkable nature of the whole sedimentary sequences embracing these 'tilloids' as calling for some very unusual mode of origin; he has even considered seriously the possibility of extremely violent tsunamis, resulting either from earth-movements of rare intensity or from impact of quite large meteorites, as a means of stripping almost everything moveable from shore areas and continental shelves and transporting vast masses of material into the basins, and incidentally leaving for some time conditions of poor availability of clastics. The nature of such deposits requires much further elucidation. Nevertheless, around the North Atlantic they are so developed in the late Precambrian, especially in relation to the facies of the succeeding Cambrian, as to provide fairly strong evidence for a former close connexion.

4. Deposits of palaeoclimatological significance

In addition to the possible evidences of past glaciations, certain kinds of sedimentary deposits have been much used in attempting to outline former climatic zones (presumably related in a general way to latitude), and suggesting therefore a more congruous and significant pattern of distribution when 'drifted' continents are re-assembled. A very convenient summary of many of these is presented in Nairn (1961); only brief comment on a few is needed here.

Extensive coal deposits of various ages were first noted by Wegener to attain a significant distribution in his continental reconstructions. That the Carboniferous coals of Europe and east and central North America were deposited in relatively low latitudes seems indeed reasonable, since many other factors including palaeomagnetic results suggest the same. On the other hand, the Ecca coals of South Africa, or the Rio Bonito coals of Brazil, were almost certainly formed in comparatively high latitudes. At the present day extensive peat deposits occur in many places around the Arctic, and the mere existence of coals can have little palaeogeographic value. Preliminary studies of the petrographical and chemical characteristics of coals of the same general age (say late Carboniferous + early Permian) from widely separate areas have been undertaken by myself and Dr Murchison and other colleagues in the Coal Petrology unit in the department of Geology at Newcastle, and it seems possible that a rather broad and not very precise distinction may be drawn between 'high-latitude' and 'low-latitude' coals (based on a reassembly of continents and backed by palaeomagnetic data), but a very great deal of further work is necessary to determine whether 'coalification-tracks' are significantly different.

Extensive and thick salt deposits and evaporites were also used by Wegener as evidence of ancient belts of hot semi-arid conditions bordering the equatorial belt, and have been used by most later 'drifters'. Green (1961) gives a convenient recent summary of the occurrences and concludes 'that the distribution of evaporites through geological time is explicable on the basis of a gross trend in climatic change, an explanation which necessitates large scale change neither in the relative positions of continental masses nor in positions of the rotational poles of the earth'. However, it must be stressed that a highly significant correlation of evaporite localities with 'contemporary' palaeomagnetic latitudes determined on the same continental area can be shown from the same data. The distributions in space and time in fact accord better with continental reassembly than they do with an unchanging geography and rotation—a conclusion forcibly expressed by Professor Blackett at a recent symposium at Newcastle.

Finally the distribution of 'desert deposits' has received much attention. The recognition of thick aeolian sand deposits in the geological record is by no means so simple as was once assumed, and one must always bear in mind that before the development of an extensive cover of herbs and grasses the continental surfaces were much more liable to the effects of wind. Opdyke (1961) has adequately summarized the possibilities inherent in the study of desert sands, both from the point of view of space distribution and of the reconstruction of ancient dominant wind-current patterns. Such evidence by itself is not of great value as a proof or disproof of drift; it comes into consideration only in relation to any particular reconstruction as a check of somewhat secondary value. Thick red-bed sequences have long been fashionably regarded as being indicative of desert conditions. van Houten (1961) provides a critical assessment that indicates very clearly the need for thorough geochemical and sedimentological study before the climatic implications of any red-bed sequence can be reasonably deduced.

It would seem therefore that the time has long passed when mere records of glacial deposits, coal-bearing sequences, evaporites or 'desert deposits' can be used as crucial evidence bearing on drift, and that much more discriminating studies and classifications of such deposits must be undertaken before they can be of diagnostic significance. It nevertheless remains true that, given the reduced value to any observation implied by this conclusion, the overall picture from these deposits and from confirmed glaciations finds a ready explanation on a sequence of continental movements such as that of du Toit and is broadly consistent with palaeomagnetic results.

5. Distribution of organisms

Perhaps more dubious and unconvincing 'evidence' has been published in this field by enthusiastic 'drifters' than in all the other fields put together, and only a brief comment is possible here. The distribution of more or less closely related living animals and plants, confined to the southern continents, has frequently been quoted as evidence of the former continuity of Gondwanaland. As a good example, the three genera of living lung fishes are confined to Australia, Africa and South America. The general history of this group is fairly well known. In Triassic times, for example, *Ceratodus* and its allies are known to have existed in fresh waters in all continents, and the present distribution is a splendid example of relict distribution. Again, the living mammals formerly grouped as Edentata have been quoted as evidence of the same Gondwanaland pattern of distribution. As used by at least one author they are not even a homogeneous group, and again fossil evidence indicates a relict distribution. The phenomenon of relict concentrations in the southern continents is a very real one, but is not to be explained simply by survival of the aboriginal passengers on the old Gondwanaland raft after it broke apart.

Much of the evidence from fossil distribution also requires careful sifting. Apart from the indication of seas extending for the first time on to continental margins, marine fossils will rarely give much help in the problem of drift. The well known existence of *Mesosaurus* in South Africa and South America in the early Permian merely underlines other and stratigraphical evidence of community of history, since small aquatic reptiles could conceivably have had a shore-wise migration. The most valuable evidence is likely to come from animals confined to a continental habitat. Even here much care is needed. Mobile land vertebrates may be thought of as likely to migrate wherever the land will take them, where the environment permits and given the time to adapt to various conditions *en route*. The resemblance between the mainly reptilian fauna of the Karroo system in South Africa and the faunas of the Permo-Triassic rocks of South America is real but is far from an identity. Elements of similar faunas (*Lystrosaurus*, thecodonts, archosaurs) are found in India, but are surprisingly absent or rare in Australia. Furthermore there is now rather strong evidence of communication in Permo-Triassic times between North America, Eurasia and Africa, since elements of faunal associations often supposed to be characteristic of South Africa occur there. Nothing short of precise morphological and taxonomic study of these forms and new discoveries is likely to contribute to the problem; specific and even generic identity of reptiles from different southern continents would very strongly favour drift.

Perhaps the most useful and valuable indications are those concerning the distribution of freshwater vertebrates and invertebrates. Amphibians, for example, are today almost completely confined to fresh waters, and marine waters would probably kill almost any modern amphibian egg or tadpole. Most fossil amphibians are found in rock layers quite devoid of any trace of marine fossils. One can reasonably assume in many such cases that migration cannot have involved significant exposure to salt waters, and that descendants of a common stock on different modern continents must have used essentially a terrestrial route. Unfortunately one cannot be sure that this was always the case; there were certainly Triassic labyrinthodonts in marine environments, and some of the Carboniferous 'coal swamp' amphibians might well have lived in more or less brackish waters. It is nevertheless probably highly significant of direct continental communication that the late Carboniferous coal swamp faunas of small amphibians and highly specialized fishes like the Haplolepidae are so similar in the eastern United States and western Europe, and are little known elsewhere (Westoll 1944).

In short, fossil evidence of any positive value to the theory of continental drift must be very much more precisely defined than has often been the case in the past. Since our knowledge of past life is so incomplete the best one can do at the present is to assess as fairly as possible the known distributions in space and time in relation to the existing continental distribution and the various pre-drift reconstruction that can be made. It can at least be said that such distributions make as much sense, and often a great deal more, on, for example, the du Toit reassembly as they do on the present continental arrangement.

There is one aspect of the distribution of fossil organisms that needs special mention, since it combines distribution with qualities supposed to be of climatological significance. Ting Ying H. Ma has published voluminously on the distribution of corals showing: (*a*) uniform but rapid skeleton building, thought to be of near-equatorial distribution, and (*b*) more or less strong alternations of rapid loose, and slow compact, skeleton building, thought to be indicative of more or less subtropical belts in which corals can still be numerous and varied today. The present-day distribution of, say, Devonian rugose corals makes no sensible correlation with present climatic zones, but it has led Ma to reconstruct past equators and tropical belts, published long before any palaeomagnetic results, and which on the whole are astonishingly concordant with palaeomagnetic latitudes recently determined, and with continental drift. A wide critical study of present and past distribution of seasonal growth in these and other organisms is likely to provide some rather critical evidence.

6. Magmatism

Here it is only necessary to indicate that vast outpourings of basic lavas, especially 'plateau basalts', have been regarded as by-products of early stages in separation of continents. Such cases as the (?) early Jurassic Drakensberg–Bushveld amygdaloid–Batoka basalts of southern Africa, the late Cretaceous–early Eocene Deccan traps of India, and the Cainozoic Thulean province of the North Atlantic are often quoted in this respect. However, other and very similar lava fields occur in the upper Cainozoic of northwest U.S.A. and in the lower Carboniferous of Scotland where no continental separation has occurred. It should be noted that such outflows are not regularly associated with other supposed fracture-and-drift continental margins. Of possible significance is the association of the Scottish Carboniferous lavas with crustal movements in a graben with considerable resemblance to a rift valley, which could have been related to an early stage in the opening of the North Atlantic, and of the various Cainozoic outpourings in northwest U.S.A. with other phenomena including crustal tension and also the probable importance of strike-slip faulting, which may be brought into the general context.

7. Rift-valleys, transcurrent (strike-slip) faults, etc.

There are many aspects of geology that are only satisfactorily explicable if the lithosphere is underlain by material which, whatever its properties when subject to short-period stresses such as seismic shock-waves, behaves in a non-rigid manner over longer periods. This degree of 'plasticity' has been recognized by such names as the asthenosphere. It is not possible here to review this evidence in detail; some of it is of profound significance to geology because it should lead to a much better understanding of the processes of origin and differentiation of magmas, the origin of continental materials, and even of the ocean and atmosphere. It is sufficient to notice that many of these phenomena are strong evidence against long-term rigidity of the mantle as postulated by Jeffreys, and imply relaxation to stresses other than those of short period or duration, on a plastic, a visco-elastic or a viscous model.

One of the most remarkable phenomena of geology is the existence of great faults which are essentially of large lateral displacement; they are variously named—e.g. strike-slip faults, major wrench-faults, etc., and Carey's term megashear (see, for example, Carey 1958) is a very convenient one. Carey also points out the intimate relation between megashears and other large scale features. The existence of such megashears as the San Andreas and Great Glen faults makes the acceptance of some other categories of crustal movement recognized by Carey almost obligatory, and in turn this implies a degree of crustal movement that cannot be accommodated without some degree of drift.

Furthermore, it is becoming increasingly recognized that shear-distortion of parts of the continental crust is widely distributed. 'Structural failure' of the crust often involves the production of a pattern of cognate shear joints. Cumulative movement across a belt of such close-set joints may reach surprising proportions. It is very desirable that a systematic study of all nearly rectilinear joint patterns should be made to determine displacements along the joints; in the Bewcastle area, admittedly one of modest tectonic complexity, Dr A. S. Campbell and the writer have seen a fossil log displaced sinistrally by three close joints by 1 ft. 2 in. (0·35 m) in a total length of 5 ft. (1·5 m), and there was nothing otherwise to indicate that these three joints planes were in any way remarkable in themselves, or that they differed from the many thousands of other joints in the nearby area. If even a moderate degree of adjustment to movement, respectively dextral and sinistral, along two sets of cognate shear-joints is admitted, then some at least of Carey's other reconstructions (1954–1958), involving 'plastic' shape-readjustments as well as large scale drift, cannot be dismissed by thoughtful geologists. And such movements seem necessarily to imply reponses by a more or less rigid lithosphere to plastic or viscous movements beneath, wherever they are on a large scale. (Small scale effects, of course, may be controlled by more or less plastic or viscous flow within the lithosphere, down to the dimensions of alternating clays and hard beds, where there has been tectonic movement.)

An important place in the consideration of continental drift is held by the rift valley systems of Africa, the Red Sea and adjoining areas. These have often been claimed as stages in the early breakup of a single continental mass. Geological opinion on the nature and mode of origin of rift valleys has been very varied. It was for a time fashionable to think of them, not as tension features, but as highly compressional features bounded by ramp faults; this explanation of typical rift grabens is now less likely (e.g. Girdler 1963 *b*). Two points should be made—first, that rift faults may not always occur as facing pairs, one fault may in effect be replaced by a downwarping; and secondly that there is sometimes evidence, variable in quality, of transcurrent, strike-slip movement between the blocks overlooking the rift. In the case of the Red Sea rift the evidence strongly suggests the opening of a narrow rhombochasm (Quennell (1958) and ensuing discussion; Carey (1958); Girdler (1958–1963 *b*)). Opinion has differed as to the importance of transcurrent movement in these fault systems; there is now in my opinion quite a strong case for cumulative sinistral movement of about 100 km in the Red Sea, and this must of necessity involve transcurrent movement on some at least of the branches of the system. There is indeed in this region as good a case as could be expected of incipient drift—as good as could be expected because a number of geologists will have none of it!

The Red Sea–Gulf of Suez–Gulf of Aqaba to Jordan Valley rifts provide an important parallel to a case that could be substantiated in great detail for a comparable stage in the history of the North Atlantic. An outline of this case may be given here. It postulates that in Lower Palaeozoic times Labrador, Greenland and Norway were closely associated. Many features of distribution, sequence and structural history of the Precambrian, so far as it is known, are concordant with this reconstruction—among them the late Precambrian boulder-beds mentioned above. The distribution of shelf and geosynclinal deposits in the Lower Palaeozoic is again consistent with a system of geosynclines crossing the area, and the distribution of many important fossil groups is quite directly explicable by migration along specific seaways and shorelines. Earth-movements of various intensities and at somewhat different times affected various sectors, and seem to have reached their last climax in Lower Old Red Sandstone (Lower Devonian) times. The first fractures foreshadowing the present North Atlantic seem to have developed between the Lower and Middle Old Red Sandstone; the Greenland–Norway fracture seems to have split southwards, one branch between southeast Greenland and the continental margin northwest of Scotland, a second forming the Great Glen fault (possibly continuous into the Cabot fault of Newfoundland), and a third curving branch along the Skagerrak. The main sinistral transcurrent movement along the Great Glen fault (about 100 km) can be dated on very reasonable stratigraphical evidence as post-Lower, pre-Middle Old Red Sandstone. Such movement can hardly have occurred without opening a rhombochasm farther north between north Norway and Greenland; it is not possible to date other early movements so clearly, but it is remarkable that in belts along the lines of the fracture system there is a wholly individual development of non-marine sediments of Middle and Upper Devonian age with a considerable faunal community. Throughout the Carboniferous most of this system of rift-like depressions, and erosional depressions along the lines of fracture, seems to have been the site of essentially continental sedimentation, with only slight marine invasions. By Upper Permian times, however, the depressions were deeply invaded by marine transgression from the north, which spread over large tracts of northwest Europe and gave rise to broad nearly land-locked seas, in which vast quantities of evaporites were deposited, communicating with open waters northwards only via the Greenland–Norway rhombochasm. The same avenue of invasion was only partly occupied in Triassic times, and during the rest of the Mesozoic was repeatedly flooded, giving rise to the 'boreal' Jurassic and early Cretaceous, with their 'Russian' faunas, in northwest Europe. There is strong evidence, after the early Cretaceous, of marine invasions spreading still further along the fracture system much further southeast (e.g. east coast of the U.S.A.), and the progress of complete separation was rapid thereafter.

It is quite probable that a number of other events, implying additional stages in the early separation, may be recognized. Thus while the late Palaeozoic (Hercynian = Armorican) movements in the south of Britain are mainly the products of compressional forces directed north–south, in the north of England and in Scotland they cannot be interpreted by such a direction of maximum compression. Instead, the detailed picture is growing of *westerly* transport, and differential transport at that. Such conditions probably correlate with further widening of the Greenland–Norway rhombochasm.

In such a synthesis the nature of the Skagerrak is of great interest. It seems to be, in Carey's terminology, a narrow sphenochasm, and its continuation towards the Baltic is almost certainly masked by sediments. A detailed gravimetric and magnetic survey of this structure is long overdue. The Oslo Graben and the Rhine Graben may also be involved, as results of east–west dilation at later stages in the westward drift.

Such a reconstructed history, which in my opinion is very strongly supported by numerous lines of evidence, suggests a condition in late Devonian or Carboniferous times remarkably similar to the pattern of the Red Sea complex today, oriented in almost the opposite direction. If this is significant, then the Red Sea complex, and presumably by extension the African rift-valley complexes, are strongly confirmed as an incipient or 'frozen' stage in continental fragmentation.

8. The 'new oceans'

If extensive drift has occurred, new ocean basins must have been formed—perhaps in the first place as very narrow rhombochasms or sphenochasms. A few comments on the geology of the Atlantic and Indian oceans in particular may be made.

One of the most remarkable features of recent oceanographical and geophysical research is the discovery and mapping of extensive systems of mid-ocean ridges not restricted to the 'new oceans'. They frequently have narrow but sharply defined rifts along their length, and are apparently transected by shear-faults not infrequently. In at least two cases these oceanic rifts seem to run directly towards or into important continental structures; one is the Gulf of California (interpreted as a rhombochasm by Carey (1958)) and continued northwards in the San Andreas megashear, the other is the Gulf of Aden–Red Sea system already mentioned. There is also a fairly intimate relation between another rifted ridge and the Alpine Fault system of New Zealand (the New Zealand megashear of Carey 1958). Where the North Atlantic ridge crosses Iceland there is very clear evidence of active dilatation across it. It thus appears that these 'mid-oceanic' structures share at least some of the properties of the rifts which may today in Afro–Arabia be seen as early stages in potential drift—they are in places dilatational, they may pass into spheno- or rhombochasms, and they may pass into, or be very directly associated with, megashears. The significance of such oceanic structures has been much commented upon, particularly by Ewing, Girdler, Menard and Wilson (see other contributions to this Symposium).

There are also indications, from clear proof in the case of the Seychelles to inferences from gravity measurements and petrology of erupted rocks, that there are shreds and patches in the Atlantic and Indian oceans in particular of material of more or less obviously continental origin. Some, like the Seychelles, appear to be strictly 'micro-continents', but this term is also recently current for bodies of material of far less freeboard above the Moho than continents. A possible explanation for some of these rather diffuse patches may be advanced here. The outer lithosphere on the whole behaves in a brittle manner under tensional or other stress. Geological evidence is strong that at not very great depths in the crust the mode of yielding involves flow, whether 'plastic' or 'viscous'. Put in another way, the stress relaxation times probably are extremely low in geological terms at depths of a few hundred kilometres, but rise to higher values near surface; Carey (1955) suggests stress relaxation times of the order of 1 y at 500 km, 100 y at 200 km, and 10000 y at 100 km; in 'brittle' rocks as popularly understood the value would of course be indefinitely long so long as the mass held together. When a continental raft is fractured and pulled apart the brittle fracture would not extend very far down. At a great depth essentially viscous flow would predominate; between the two a more or less plastic regime, with probably fairly rapid gradation on either side. With fracture of the brittle superficial portion, the plastic layer would stretch and thin to some extent, and as separation continued portions of this plastic zone might well become separated in the growing gap. Such remnants would probably include material of the same general composition as the deeper parts of the continental crust.

Present geological and geophysical knowledge strongly opposes acceptance of continental masses bobbing up and down; crude Atlantis-type theories are untenable. The land bridges so beloved by a former generation of palaeontologists, and introduced to explain supposed migrations of fossil organisms, are also difficult to accept in many cases. There remain some instances of supposed depressed land surfaces, such as the region around Indonesia, that can still plausibly be interpreted in this way, and demand much more concentrated study. But it is now in the highest degree unlikely that any ancient land surface has been submerged beneath, say, the North Atlantic depths.

There are some consequences of the nature of the 'new ocean' margins that are worth emphasizing. The excellent fit between say South America and Africa, or of the continental masses in the North Atlantic area, can only mean that relatively very little has been 'lost' from the margins of the sundered continents, or at least from their more superficial parts. Secondly, it is impossible to explain this phenomenon by subsidence of strips or wedges of material between 'parallel' faults of wide separation. Thirdly, the kind of fit obtained clearly sets a limit to contraction of the earth (as with most nineteenth-century theories, and given great weight by Jeffreys) or to expansion of the earth following the ideas held by Hilgenberg (1933), Halm (1935), Egyed in many publications since 1956, Carey (1958) and others. Using accurate techniques of translation round a spherical globe, such excellent fits can only be understood if the continental margins were separated when the Earth had its present radius within very narrow limits of error. Carey, in particular, has constructed a remarkable synthesis (1958) from which he concludes that not only the Atlantic and Indian oceans, but also the Pacific Ocean, have actually expanded. The geological ages of the structures round the Pacific which he claims to show this effect are in the same age range as those of the presumed start of drifting elsewhere. Indeed he considers that the diameter of the Earth in the Late Palaeozoic was only about three-quarters of the present diameter, its surface area about half the present, and its density more than twice the present density (Carey 1958, p. 347). This seems quite opposed by the excellence of fit on the present globe; any expansion (or contraction) since drift would be very small indeed. On an expanding earth, adjustment of form of a skin-like continental layer is necessary; apart from other effects, a pre-expansion fissure will tend to 'gape', and so would the sundered margins of continents if re-fitted on the expanded globe. It should be possible with careful 'best-fitting' to estimate the maximum expansion possible since drift.

9. Conclusions

Purely geological evidence taken piece by piece cannot prove or disprove drift. Many kinds of evidence, individually supporting the drift theory with only a low probability, may collectively form a linked network that argues powerfully in its favour. As one of those geologists who have long been convinced of the reality of drift, I, and no doubt they, welcome palaeomagnetic evidence while realizing that some of it may have to be revised or discarded as untrustworthy, and welcome also the new geophysical attack on the problems of the Earth's interior that has in recent years sanctified the geologists' requirements for a non-rigid mantle, and has also gone far to justify Arthur Holmes's use of convection in the mantle to illuminate geological problems. What we would welcome even more is research funds on the scale of geophysics research funds to prosecute geological investigation without which the detailed historical development cannot be elucidated.

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