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# THE EMERGENCE OF PLATE TECTONICS: A PERSONAL VIEW

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## THE PROBLEM

In the early nineteenth century it was clear that the continents were very old and had a complex history. Ideas about the time scale were vague but the bulk of well-informed opinion spoke of millions or hundreds of millions of years or, more vaguely, of there being “no vestige of a beginning, no prospect of an end” (Hutton 1795). Of course there were many, particularly among English clergyman-geologists, who still tried to fit earth history into the time since 4004 B.C. allowed by Bishop Usher, but the effort was by then clearly hopeless (Rudwick 1972). The view of Hutton and of Lyell that the time scale was long enough for processes observable today to have caused the changes that had occurred in the past was widely accepted by the 1830s.

The geologists of the nineteenth century were enormously successful; they devised methods of elucidating the history of particular areas in great detail and setting them in an ordered time sequence (a time scale without dates). They were much less successful in giving a global picture of events; in fact, the subject tended to fragment into local monographs. It is only in our own day that the pendulum has swung back and the center of interest shifted to the relation of local phenomena to global processes.

This review attempts to describe the course of this change of emphasis. The central theme is the relation of the continents to each other and to the floor of the ocean. In the absence of knowledge about the ocean floor a variety of hypotheses could be entertained. The oceans could be considered as like the continents but eroded from material originally above sea level, or as flooded low lying continents, or as subsided continents. Alternatively they could be regarded as essentially different from the continents but as of the same range of age and with an equally complicated history. All of these views are certainly incorrect and of all the possible hypotheses the one that seemed to many the least likely has obtained a nearly universal acceptance. This is the hypothesis of sea-floor spreading and continental drift. The editor has asked me to tell the story of this change in ideas from a personal

point of view. This seems to me appropriate since a revolution in scientific opinion is not only a question of new data and new theories, it has also its emotional and psychological aspects.

## BEFORE WEGENER

Reading the literature with hindsight reveals a number of passages that can be regarded as prefiguring the idea of mobile continents. A passage from Bacon (1620) is frequently quoted but this appears to me to be merely a comment on the similar shape of the west coasts of Africa and South America, with no implication of movement. The first undoubted reference seems to me to be in Snider's (1858) book *La création et ses mystères dévoilés*. This book is a rather cranky attempt to square geology with Holy Writ and, even when first published, can have contained little of interest to serious students of geology. It does, however, contain a diagram showing how well the west coast of Africa fits the east coast of South America. The diagram (Figure 1) is a rough sketch and the fit has been obtained by distorting the coastlines; in fact the coastlines do not fit, the good fit is between the continental edges (that is, the outer edge of the continental shelves). The distortion is not trivial



Figure 1 Snider's reassembly of the continents [a redrawn version from Hallam (1973)].

as the Argentine continental shelf is 400 km wide. In fairness to Snider it should be said that he could have had only the vaguest idea of where the edge of the shelf lay, and could not have known that it is the true edge of the continent. Snider's diagram was reproduced by Pepper (1861) in an admirable book on minerals and metallurgy intended primarily for schoolboys. By a strange chance the page heading is "The drift theory," but the text shows that the drift theory referred to concerns the formation of coal seams from driftwood.

So far as I know, the writings of Snider and Pepper had no influence on scientific opinion. The same can be said of the other nineteenth and early twentieth century references to continental movement. A number of these are listed by Du Toit (1927) and Meyerhoff (1968); they will not be discussed here. The best known is Taylor's (1910); his movements are primarily away from the pole and are not closely related to later work.

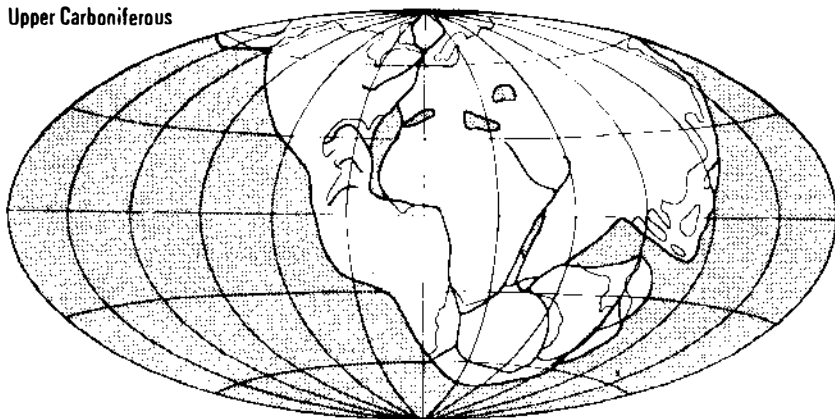
## WEGENER AND THE DISPUTES OF 1920-1940

The source of the controversies of the 1920s and 1930s is two lectures given in 1912 by Alfred Wegener (1880-1930) in Frankfurt and Marburg. These were published (Wegener 1912a,b) and later appeared as a book (Wegener 1915). Wegener brought together a great collection of evidence from diverse fields of earth science to show that the continents had formerly been collected together into a single land mass, Pangea. The main arguments were: (a) that the continents would fit together as a jigsaw puzzle; (b) that not only the shapes but the geological structures fit; (c) that longitude determinations showed Greenland to be moving away from Europe; (d) that the distributions of past climates (particularly the Permo-Carboniferous ice age), salt deposits, and corals were incompatible with the present positions of the continents, but compatible with the former existence of Pangea; and (e) that the distributions of animals and plants were also consistent with the idea of Pangea.

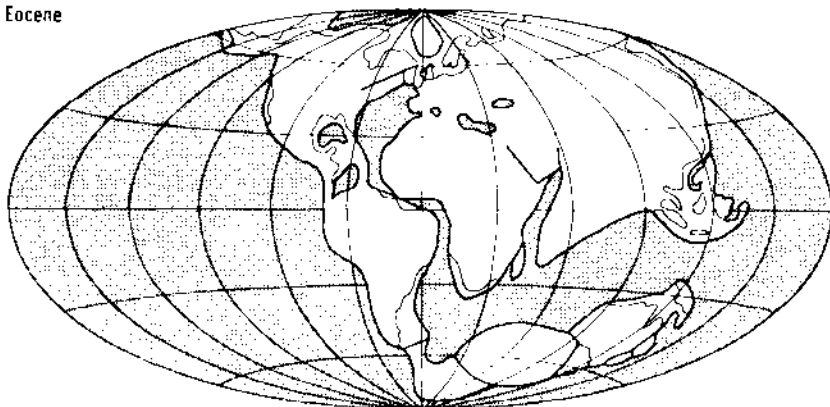
The publication of Wegener's book was followed by 20 years of controversy which was ended more by the exhaustion of the possible arguments and of the contestants, than by any firm conclusion. It is interesting to consider why Wegener's arguments did not carry conviction, since it is now clear that many of them are, in principle, sound. The reasons were, in part, associated with the nature of Wegener's presentation. He argues too hard and was often accused (e.g. by Lake 1922) of advocating a cause rather than seeking truth.

Wegener laid great stress on the fitting together of the continents which, he tells us, was the original source of his ideas. The crucial illustration, reproduced here as Figure 2, is a mere sketch in which the continents are substantially distorted. For example Alaska and Asia are left in contact at the Bering Straits, which is impossible if the Atlantic is closed and the continents left intact. India is elongated to about three times its present length to fill the Indian Ocean, and it is supposed that most of it was crushed to form the Himalayas and the mountains of central Asia. North and South America are joined by a large continental area, whose origin is not clear. The whole argument was much weakened by Wegener's belief that the southern limit of the moraines of the Quaternary glaciation were continuous in his

Upper Carboniferous



Eocene



Lower Quaternary

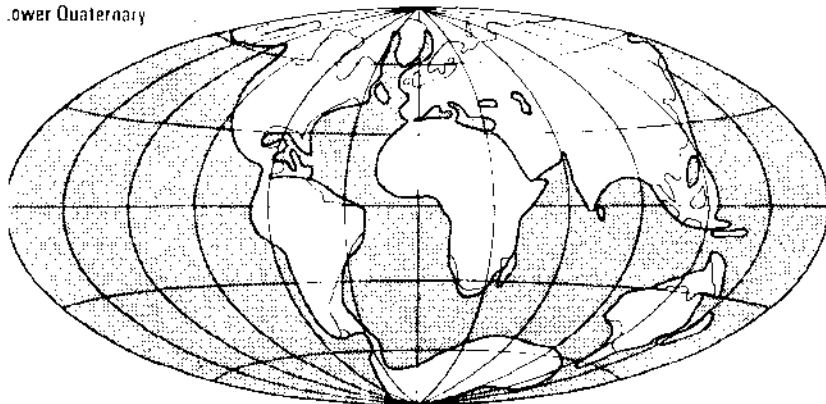


Figure 2 Wegener's reassembly of the continents, redrawn by Hallam (1973).

reconstruction and indicated that the separation of Greenland from Europe had occurred 50,000 to 100,000 years ago. This implied a motion of Greenland away from Europe at the almost incredible speed of 30 m/yr. The longitude measurements that were supposed to support this conclusion were never convincing, as they involved a comparison of results from the old method of lunars with modern observations.

The argument from the distribution of the Permo-Carboniferous ice age in the southern continents was a strong one, but in an age when travel was by ship, most geologists resident in the northern hemisphere had not seen the evidence and were fairly easily able to explain it away, doubt it, or ignore it. Paleontological arguments are usually not, in themselves, convincing; for example, if the Mesozoic reptiles of Europe and North America are similar but the Tertiary mammals are different, this is compatible with separation of the continents in the Cretaceous, but it is also consistent with the former existence of a land bridge which foundered into the Atlantic. The evidence does not point uniquely to relative horizontal movement of the continents.

It is easy to see why there was such strong opposition to Wegener in the 1920s and 1930s. If weak or fallacious arguments are mixed with strong ones, it is natural for opponents to refute the former and to believe that the whole position has been refuted. There is always a strong inclination for a body of professionals to oppose an unorthodox view. Such a group has a considerable investment in orthodoxy: they have learned to interpret a large body of data in terms of the old view, and they have prepared lectures and perhaps written books with the old background. To think the whole subject through again when one is no longer young is not easy and involves admitting a partially misspent youth. Further, if one endeavors to change one's views in midcareer, one may be wrong and be shown to have adopted a specious novelty and tried to overthrow a well-founded view that one has oneself helped to build up. Clearly it is more prudent to keep quiet, to be a moderate defender of orthodoxy, or to maintain that all is doubtful, sit on the fence, and wait in statesmanlike ambiguity for more data (my own line till 1959).

More sanguine defenders of orthodoxy may be driven by similar motives to quite violent and logically indefensible attacks on the innovators. Examples of this class of argument are Chamberlain's (1928) approving quotation from an unnamed colleague: "If we are to believe Wegener's hypothesis we must forget everything which has been learned in the last 70 years and start all over again". Termier (1925) also commented upon it as "... a beautiful dream, the dream of a great poet. One tries to embrace it, and finds that he has in his arms a little vapour or smoke. . . ." [Termier was, of course, a Frenchman; Du Toit (1937) says, a little inappropriately, that this remark "reveals the gloomy spirit of its author".] Another geologist quoted, but not named, by Gevers (1950) said that in Wegener's views he saw "only a drunken sialic upper crust hopelessly floundering on the sober sima". Such irrational reactions are not unique to earth scientists. There is a celebrated story, whose origin I cannot at the moment trace, about a pure mathematician who was so incensed by the attempts, popular 60 years ago, to refine the definition of a function that he referred to "certain young men who have introduced into analysis new and extraordinary

functions whose sole purpose is to make a mock of definitions that have been found adequate by generations of mathematicians" (I cannot quote the exact words).

The main arguments for and against Wegener's position were conveniently brought together in the published account of a symposium held in New York in 1926 at a meeting of the American Association of Petroleum Geologists (van Waterschoot van der Gracht 1928). The most systematic attack came from Charles Schuchert. He attempted to show that Wegener's fit of the continents around the Atlantic is illusory. He does this by moving plasticine models of the Americas over the surface of the globe and gets gaps of 1200 miles. The illustrations are so bad that it is difficult to trace the reason for this extraordinary and quite false result. It is probably mainly due to leaving Africa fixed relative to Europe. It is only fair to say that Wegener had also done this and that his diagram suffered some distortion to get as good a fit as he did. It is interesting that Schuchert so desired to refute Wegener that he did not see what an excellent fit could be obtained by separating Africa from Europe by widening Tethys at its eastern end. In 1926 this would have been an important discovery and might greatly have affected the development of ideas. As it was he quoted a friend who said that the fit of Africa and South America was "made by Satan" to vex geologists. He even reproduced a diagram from Behm (1923) showing a widened Tethys and a good fit (I have not seen Behm's book). He remarks that Behm's map is distorted, which it is, but misses the hint of how to close the Atlantic. The whole story of the fits is an illustration of the sloppy way in which new ideas can be treated by very able men when their only object is to refute them.

The main features of Wegener's work that contemporaries found unacceptable were: (a) the late date ascribed to the separation of Greenland from Europe, (b) Washington's (1923) statement that the rocks on the two sides of the Atlantic do not match, (c) the improbability that Pangea had survived intact till the Cretaceous and then broken up, (d) the absence of a plausible mechanism driving the movements, and (e) the impossibility of the strong continents being buckled into mountains by pushing through the weaker ocean floor.

The absence of a mechanism may have been unfortunate, but it was not strictly an argument against drift. We believe many things of which we do not know the cause; for example, no one doubts that there have been ice ages; it was known that cholera was caused by drinking dirty water many years before the role of bacteria was understood; and even today the precise mechanism producing the undoubted correlation between smoking and lung cancer is not clear.

The arguments about mechanism were rendered somewhat inconclusive by the almost complete ignorance of mechanics of several of the contestants. Bailly Willis (1944), for example, wrote:

I confess that my reason refuses to consider "continental drift" possible....when conclusive negative evidence regarding any hypothesis is available, that hypothesis should, in my judgement, be placed in the discard, since further discussion of it merely incumbers the literature and befogs the minds of fellow students.... Now, it is a well established principle of mechanics that any floating object moving through air, water, or a viscous medium creates behind it a suction of the same order as the pressure developed in front of it. This law applies equally to airplanes, ships, rafts, and

drifting continents (if there are any). The pressure which could raise the Andes must, therefore, have been approximately equalled by the suction and tension in the rear. Sections of the continent must have been sucked off.

In 1929, J. W. Gregory gave a presidential address to the Geological Society of London. In this he supported an elaborate system of sunken continents beneath the oceans which bobbed up when needed. To believe this he felt he had to dispose of isostasy. He wrote: "If the ocean surface does not conform to a regular ellipsoid or spheroid, if it sags down in mid-ocean owing to the lateral attraction of the water toward the land, or sinks with variations in the specific gravity of the water, ... then the slight differences in the attraction of the ocean floor may be due to the depth being overestimated and not to the higher density of the floor". He ends: "If isostasy be so stated that it is inconsistent with the subsidence of the ocean-floors, so much the worse for that kind of isostasy". (He adds a footnote to say that Harold Jeffreys "fully agrees" with the last sentiment.) Gregory was not a crank, he was among the most knowledgeable and influential geologists of his day. He was the first geologist to see the Kenya rift valley and his two books about it greatly interested me as a young man. It is odd that he could not restrain himself from writing nonsense about things of which he knew nothing. His presidential address is well worth reading both for the discussion of the need for transatlantic connections and for the world view of a distinguished geologist in the 1920s. Perhaps the most remarkable feature is the absence of any appeal for more information about the oceans. Gregory seems very satisfied by what he has; maybe it was convenient to keep the ocean as *terra incognita* about which anything could be assumed.

So much for Wegener's opponents of the 1920s. His most effective supporter was A. L. Du Toit (1878–1948) who was a distinguished South African geologist. In 1923 he spent some months in South America with the express purpose of comparing the rocks with those of South Africa (Du Toit 1927). He was deeply impressed by the similarity of the stratigraphy. Of South America he wrote, "To a visitor from South Africa the resemblances to that country are simply astounding.... I had great difficulty in realising that this was another continent and not some portion of one of the southern districts of the Cape." In fact, the evidence for former connections between the continents is much easier to see in the southern hemisphere than it is in the northern. This is largely due to the general tendency of the Caledonian orogeny in the northern hemisphere to run parallel to the Atlantic margins while in the southern continents the split has crossed tectonic lines more nearly at right angles. The Permo-Carboniferous glaciations of the southern hemisphere also provided strong arguments. Du Toit's unrivaled knowledge of South African geology [R. A. Daly is said by Gevers (1950) to have called him the "world's greatest field geologist"] enabled him to write a book (Du Toit 1937) which had a great influence on subsequent opinion. The biographical notice by Gevers (1950) gives a most interesting account of the development of Du Toit's views and their relation to other aspects of his work. It is strange that Gevers felt it necessary, even in a laudatory obituary notice, to half dissociate himself from heresy. He summed up Du Toit's work in these words: "... notwithstanding the zealous and valiant efforts of du Toit and others, there has in recent years been a marked regression of opinion

away from continental drift. . . . The greater the indignation to which the orthodox minds are roused by revolutionary heresies, the greater the amount of unsympathetic attention. This again stimulates the zeal and ardour of the heretics and their disciples. . . . Of late, however, the obstacles to smooth continental drifting are being more strongly felt in many quarters previously sympathetic."

The supporter of Wegener who came nearest to modern views was Arthur Holmes (1929, 1944). His ideas are most conveniently discussed later, in the context of plate tectonics. Daly (1926) flirted with the idea of large movements but does not mention the matter in a later book (1942).

## THE RE-EMERGENCE OF CONTINENTAL MOVEMENT: 1945–50

On the whole Wegener's opponents had the best of the prewar arguments. During the 1930s and 1940s it was unusual and a little reprehensible to believe in continental drift. It is easy now to see that what was needed was not further disputes about the old arguments, which had been demonstrated not to carry conviction, but new evidence. The new evidence, when it came, was of two kinds. Paleomagnetism provided virtually direct proof of movement, and the study of the ocean floor provided an entirely new insight into the relations of continents to oceans and into the geometry of the movement.

It is here that my personal experience begins. I migrated from physics to geophysics in 1931, shortly after Wegener's death, and spent some years learning the techniques of applying physics to the earth and trying to understand the modes of thought of geology. My initial idea was that geophysics should be used to solve specific geological problems, the paradigm being the applications to prospecting for oil. Gradually I realized that, important as such applications were, I was more interested in major problems of earth structure and history. I gradually came to see how weak the underpinning of geological hypotheses was, and I began to look around for experimental projects that were really significant and yet within the capabilities of our small group. We were fortunate in some of our choices, such as gravity in East Africa and Cyprus, and heat flow in England, Persia, and South Africa, but our real piece of good fortune was to turn to the study of the geology of the ocean floor.

## MARINE GEOLOGY: 1936–1960

The way in which we came to study the ocean floor is worth recounting. The initial marine project was for B. C. Browne to measure gravity over the continental margin to the west of the British Isles, using the pendulum apparatus of Vening Meinesz and a submarine of the British navy. At about the time this was being planned, in 1936 I think, I met Richard Field of the Geology Department at Princeton. Field was a remarkable man; he was in a large degree the founder of marine geology. He explained to me that what was wrong with geology was that it studied only the dry land and that you could not expect to have sensible views about the earth if you studied only one third of its surface. The critical problem was to study the ocean

floor starting from the land and working outwards into the ocean. The idea was simple and must have occurred to many people, but in Field it was combined with the burning zeal of an Old Testament prophet. He would not take no for an answer, he would not stop talking, he had no doubts, he was embarrassing and sometimes a nuisance, and yet he struck the match that set earth science alight. For some accounts of his life see Hess (1962a) and Bullard (1962); for his own views see Field (1938). He invited me to the United States in 1937 (and, I suspect, paid my fare), he drove me hither and thither in his car, tried to teach me geology, and sent me out to sea with the Coast and Geodetic Survey and with Maurice Ewing. After a few days he was preaching to the converted, but he did produce a sense of urgency that was new to me in science. By similar methods he recruited Maurice Ewing and Harry Hess (the latter carried his marine enthusiasm to the point of being simultaneously a professor of geology and an admiral).

Marine geology has many threads going far back into the past (Deacon 1971), but its development in the 1950s was largely the work of the groups at Columbia, the Scripps Institution, Woods Hole, Princeton, and Cambridge. Of course there were also many other groups such as Hans Petersson's Albatross expedition and the work of Gaskell and Swallow in H.M.S. *Challenger*. It is remarkable how, after the vigorous start of marine geology in the voyage of the original *Challenger* (1873–1876), the subject had been allowed almost to collapse and had to await the arrival of a new generation with new ideas about the amounts of money it was appropriate to spend on research [on this, but in a more general context, see Bullard (1975) and other papers in the same symposium].

Before the war the continental shelf was the main area of interest. Enormous and unexpected thicknesses of sediment were discovered. These have proved of immense economic importance and it is of interest that they were first found, not by an oil company, but by groups of young men interested in the earth. The paper by Ewing, Crary & Rutherford (1937) describes the beginning of a worldwide investigation which combines in the happiest way great scientific interest and a start towards the discovery of enormous resources. My own first publications described work with T. F. Gaskell in the western approaches to the English Channel (Bullard & Gaskell 1938, 1941).

In the years immediately before the war both Ewing's group and that at Cambridge turned their thoughts to using the seismic method to measure the thickness of sediments in the deep ocean. This was not as easy as it now appears; work on the continental shelf involved putting the explosives and the instruments on the sea floor which was, at that time, a new and complicated procedure. Just before the war it occurred to Gaskell and me (Bullard & Gaskell 1941) that the instruments could be in the water near the surface of the sea. Some trials were made in the late summer of 1939. After the war this, with the explosions also near the surface, became the usual method of operation.

After the war a great effort was put into work in the deep sea, the work of Ewing at Columbia and of Hill at Cambridge being particularly significant. At first the only equipment available in surface ships was the echo sounder, the corer, the dredge, and the seismic gear. The Meinesz pendulum equipment could also be used, but only

in a submarine. Soon a great discovery was made, or rather gradually became apparent; the oceans are quite different from the continents. At sea all the hard rocks are basalts; all the hills are volcanoes; the sediments are usually less than 1 km thick and often much thinner; the Moho is at a depth of about 10 km compared to 30 km under the continents; and, most important of all, the rocks recovered were all Cretaceous or younger. The fact of the youth of the ocean floor took some time to sink in. It is not unexpected that the sea floor should be covered with young sediments, and it was only gradually realized that we had not only recent sediments, but also many samples of all ages back to the middle Cretaceous and, before that, nothing. When this was realized great efforts were made to find older rocks by dredging on fault scarps, but none were found; it seemed clear that the history of the oceans went back to 100 m.y., but no further. The contrast with the continents and their great areas of 2000 to 3000 m.y. old rocks was a critical discovery and showed how prescient Field had been in believing that the study of the oceans was what was missing from geology.

The oceans were formed recently, but the results of the gravity measurements and the shallow depth of the Moho made it certain that they were not sunken continents covered with lavas and recent sediments. A whole range of speculation on paleogeography was excluded. If the paleontologists needed land bridges between now-separated continents they must fit them into gaps between surveys which became narrower and narrower. I suppose the only candidates left today are Lomonosov Ridge in the Arctic Ocean and Broken Ridge sticking out from Australia into the Indian Ocean and ending in the deep sea.

It is not practicable in a brief account to trace all the ideas of the 1950s to their sources in the literature and, instead, I shall use a series of review articles that I wrote. These give convenient summaries and, while they are inevitably colored by my own interests and beliefs, they do, I think, give a fair picture of the development of ideas. The first of these reviews was written just before the war (Bullard 1939): it opens with a sceptical statement about our ability to decide, using information then available, between theories of the history of the oceans; it mentions permanent oceans, continental foundering, and continental drift as possibilities. The rarity of deep water sediments on land suggests that oceans are not converted to continents. The key is "to study the form and nature of the ocean floor and, if possible, its structure.... The first requirement... is to develop instruments and methods for collecting information about the submerged rocks." A project is suggested for a detailed survey of part of the mid-Atlantic Ridge using moored buoys, to determine "whether the topography consists of submarine volcanoes, of undenuded fold mountains, or of fault scarps". The rapid rate of accumulation shown by cores of deep ocean sediment, obtained by Piggot, is said to suggest impossibly great thicknesses of sediment if the oceans are old. Hopes are expressed that the seismic method may show how much sediment is present in the deep sea and the work on the continental shelf is described. Surface waves and the nature of the rocks from islands and from dredging are said to suggest the absence of granite in the oceans. The gravity results of Vening Meinesz and the locations of earthquakes in the East Indies are discussed and it is suggested, following Visser (1936), that there is an

inclined thrust plane along which the continent and the island arc are thrust over the ocean. The review concludes that "the difficulties lie in organisation and finance rather than in technique".

This review is interesting in that it emphasizes a major problem. How much of the ocean floor is truly oceanic and how much is continental fragments? In particular, what is the nature of the mid-ocean ridge? Daly (1942) and Holmes (1929) both thought that the ridge was a sialic, continental feature. If this were so, large pieces must have broken from the continents in the course of separation and no fit is to be expected. Studies of the propagation of surface waves, showing that the Atlantic had a structure intermediate between that of a continent and the Pacific Ocean, seemed to confirm this view. Detailed studies at sea gradually dispelled this idea and left only minor continental fragments in the oceans, such as the Seychelles and Rockall Bank. The difficulties with the surface waves were resolved by Ewing & Press (1956).

Reviews written during the war (Bullard 1940a,b) add nothing to that of 1939, except the statement that the continental shelf had been built outwards; this also occurs in Bullard & Gaskell (1941). We should have realized that this view was incompatible with the results of dredging which showed Mesozoic rocks near the outer ends of the submarine canyons off the east coast of the United States.

In 1954 the Royal Society held a discussion about the floor of the Atlantic Ocean. The time was well chosen; the first deep-sea seismic results were available giving thin sediments and a shallow Moho. There were also a few measurements of heat flow.

G. M. Lees, in the best 1920s manner, contrasted "geophysical conceptions" with "geological evidence", and maintained that foundered continents were buried beneath the lavas and sediments of the ocean floor. I replied (Bullard 1954) with what now seems remarkable restraint. My main theme was the importance of establishing beyond doubt the continuity of the continental and oceanic Mohos, which Lees had denied and which would exclude his hypothesis of buried continents beneath the oceans. I also stressed the desirability of showing whether the Hercynian structures of western Europe and Newfoundland ran out beyond the continental edge. I must have had in mind the idea that they might have been truncated by the separation of North America from Europe, but I did not mention it; I was still sitting on the fence. I did not go any further in a review published two years later (Bullard 1956).

A most interesting paper was published by Hess (1954) in the Proceedings of the Royal Society Symposium. In it he discusses the nature of the mid-Atlantic Ridge and considers various possibilities. One of them is the existence of a rising convection current in the mantle beneath the ridge: a large rising mass of basalt breaks through to the ocean floor and carries blocks of peridotite with it. The ridge stands high because of the high temperature of the rising basalt, because of the serpentinization of the basalt and because of "lifting" by the upward convection current. It is easy to see here the germ of the idea of sea-floor spreading which is first clearly stated nine years later (see below). There is, however, no mention of plates or of any general outward motion of the crust; the argument is directed exclusively to phenomena near the ridge crest. Rather oddly, there is no mention of the very similar speculations of Holmes (1929, 1944) which are discussed in a later section of this review.

The 1954 symposium also contains a remarkable paper by Rothé (1954). He shows that the earthquakes of the deep Atlantic are concentrated along the crest of the mid-Atlantic Ridge and that they run around South Africa into the Indian Ocean, which suggests that the ridge is connected to the ridges of the Indian Ocean. His diagram is reproduced in Figure 3. Shortly after this, Ewing & Heezen (1956) stressed the worldwide continuity of the ridge and of its remarkable central valley along which the earthquakes lie. These discoveries were the first clue from which plate tectonics developed, but before describing this we turn back to the vindication of Wegener by paleomagnetism.

### *Paleomagnetism*

The study of the magnetization of rocks has led to the discovery of a surprising variety of phenomena which have been of critical importance for the understanding of the relative motions of portions of the earth's surface. It is impossible to describe the history of these discoveries in detail here. Excellent accounts have been given by Cox & Doell (1960) and Irving (1964).

Knowledge of rock magnetism goes back to classical times [the references have been collected by Gilbert (1600)], but nothing that is relevant to our purpose was done till the work of Bruhnes (1906), who discovered reversely magnetized rocks. He and his successors [see Koenigsberger (1938) for references] showed that: (a) modern lavas are magnetized in the direction of the present field, (b) Tertiary lavas are magnetized roughly in the direction of the present field or in the reverse direction, and (c) older lavas are often magnetized in directions making large angles with the present field. The results were quite complicated and some hindsight is involved in putting the conclusions so simply. It is remarkable that such fascinating and obviously significant phenomena should have been so widely ignored for so many years. I first heard of them in 1939 from Basil Schonland who was interested in the work of Gellertich (1937) on the Pilansburg dykes in South Africa. We discussed the results a good deal, largely in an effort to explain them without assuming reversals of field. We did not, so far as I remember, read the earlier literature or propose any program of work. We were both occupied with other things, I with heat flow, he with thunderstorms.

Towards the end of the war Patrick Blackett and I spent much time discussing the shape of science after the war (Bullard 1975). One of the topics we discussed was the origin of the earth's magnetic field. Blackett decided to test the hypothesis that a rotating body is spontaneously magnetized. For this purpose he developed a very sensitive magnetometer. When the theory collapsed he turned his attention to the use of this instrument for measuring the magnetization of rocks (Blackett 1956). In this he was joined by a number of graduate students, among whom was Keith Runcorn. They embarked on a large program of measurement and soon had very striking results confirming and greatly extending the prewar French and German work.

The results raised many questions which had to be answered before a convincing interpretation could be made:

(a) Were the rocks really behaving as fossil compasses and indicating the direction

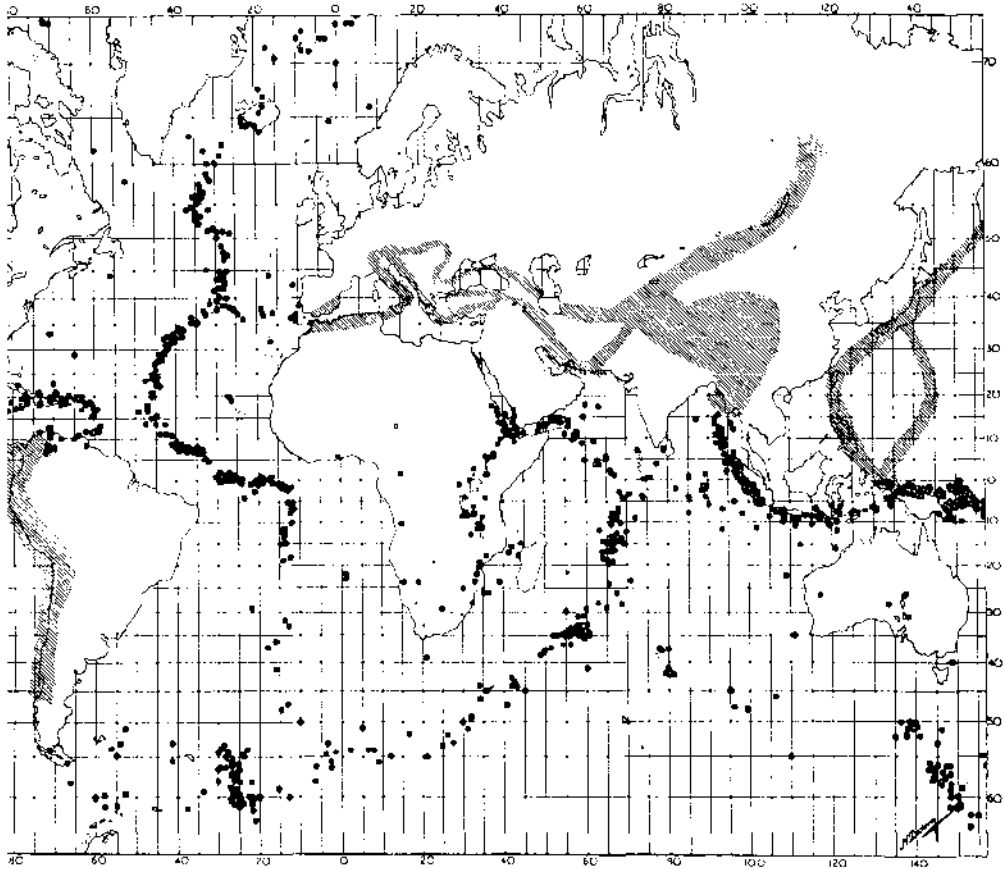


Figure 3 Rothé's map of the earthquake epicenters of the Atlantic, showing their alignment along the axis of the mid-ocean ridges (from Rothé 1954).

of the field in the past? The fold and conglomerate tests of Graham (1949) showed that a large class of rocks, particularly basalts and red sandstones, were satisfactory.

(b) Does reversed magnetization [of which striking examples had been found by Hospers (1951, 1953, 1954) in Iceland] always indicate a reversed field at the time of magnetization? This was for a long time a controversial issue which was greatly complicated by a lava, found by Nagata (1951, 1953) on the Haruna volcano in Japan, which magnetized itself backwards on cooling through its Curie point. It gradually became clear that such rocks are extremely rare. The universal normal magnetization of recent rocks (except for Nagata's rock) also suggests the rarity of self-reversal. The matter was settled 12 years later by the demonstration that the reversals were simultaneous in widely separated places (Cox, Doell & Dalrymple 1963a,b, McDougall & Tarling 1963) and that the sediments of the ocean floor also

showed reversals (Harrison & Funnell 1964). The proof by Ninkovich et al (1966) and others that the sediments show the same sequence of reversals as had been found in the lavas left no further doubt. A bibliography of the extensive literature on reversals is given by Bullard (1968).

(c) Do the results for older rocks indicate polar wandering or continental drift? If in the past the earth's field was, like the present field, roughly similar to that of a dipole, then the results clearly indicate that the magnetic pole has moved relative to the earth's surface. The work of Blackett, Runcorn, and others showed that these movements gave motions that progressed steadily with time. For some years it was not clear whether the pole moved in the same way relative to each of the continents (polar wandering) or whether different tracks were obtained from different continents, which would indicate relative movement of the continents (continental drift). The first results to suggest strongly that the continents did not all follow the same track were those of Runcorn (1956) for Europe and North America which are shown in Figure 4. In a statistical sense the difference is clear and is in the expected direction but at the time many, including myself, were not fully convinced. I feared that there might be unknown systematic errors which were different for the two continents. For me full conviction came when Blackett et al (1960) collected the world wide data and presented it in a way that clearly indicated the reality of continental drift. A little later Irving (Runcorn 1962) showed that the Permian and Carboniferous poles derived from Australian rocks were about as far as they could be from the European and North American poles. The agreement of the results from lavas and sediments was also important.

The clarity which was finally achieved in the interpretation of paleomagnetism should not obscure the complexity and difficulty of the route by which it was attained. To establish the facts of continental movement and field reversal in the face of doubts raised by the existence of many unstable rocks, the existence of self-reversing rocks and the complexity of the relations between movements of the continents and the pole is a major achievement. Other matters that required attention were the possibility that the field might in the past have differed greatly from that of a dipole (Cox & Doell 1961), that the magnetic poles might have departed greatly from the poles of rotation, and that the earth's radius might have changed. All this necessarily took some years to elucidate; the surprising thing is that by 1960 the case was substantially complete. Runcorn (1962) gives a good account of the state of knowledge at that time.

In a review lecture given at the first International Oceanographic Congress in 1959 (Bullard 1961) I said, for the first time, that I thought that the paleomagnetic evidence gave "a strong case for relative movements of the continents." I discussed what should be done to find confirmation in the oceans and suggested again the study of structural lines in western Europe and Newfoundland to determine if they are truncated by the continental edges. I also returned to the question of the arrangement of the sediments on the shelf, about which I had been in error in 1939, and suggested that a sedimentary basin may have been cut in half by the separation of Europe and North America. I said that "the mid-Atlantic ridge and particularly its central valley might be regarded as the place where the Atlantic is at present

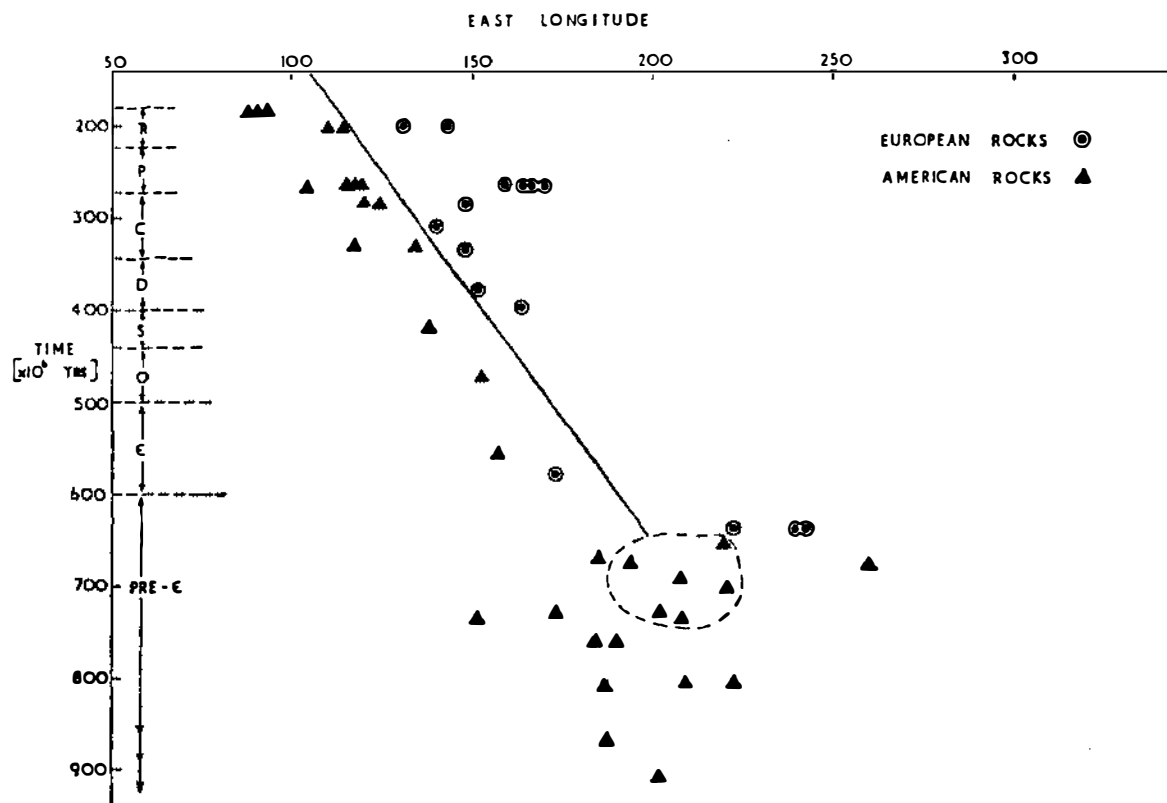


Figure 4 Variation of the longitude of paleomagnetic poles for Europe and North America. The longitude can be measured because the pole was then near the west coast of the Pacific [from Runcorn (1962), by permission from Academic Press].

widening.” The published account still shows some concessions to the possibility of alternative views; I remember the lecture as being more aggressive in support of movement.

In a lecture given in June 1963 (Bullard 1964) I at last came out in favor of continental drift without reservations and without balancing of probabilities for and against. The lecture goes over the usual arguments, with emphasis on the occurrence of displacements of hundreds of kilometers on transcurrent faults on land and of the even larger displacements shown by the offsets of the magnetic lineations off the coast of California (Vacquier et al 1961). A rather severe attitude is taken to opponents and the arguments against drift are called “ad hoc and far fetched”. The possibility that continental drift is produced by thermal convection is discussed much on the lines of Holmes (1944). A distinction is made between continental edges, such as that of South America, where the ocean floor is carried under the continent and quiescent edges where “the continent could . . . be transported as on a conveyor belt”. It is odd that, though there is a reference to Dietz (1962), there is no indication of the developments that were about to take place which transformed “continental drift into sea floor spreading” and “plate tectonics”. In spite of this deficiency the talk had, I think, some influence in moving opinion in England towards acceptance of continental movement. I was surprised by the amount of support shown in the subsequent discussion. E. B. Bailey said that he had been convinced by G. W. Lamplugh and had lectured on drift (to a nongeological audience at the Old Vic theater!) in 1910. Unfortunately neither published his views, though Lamplugh is mentioned by Holmes in the discussion following his paper (Holmes 1929), and Bailey (1929) went so far as to say, “Wegener may perhaps be telling us the truth”.

## SEA-FLOOR SPREADING

By the early 1960s there was strong evidence of continental movement, of the absence of sunken continents beneath the oceans, and of the youth of the oceans. The consequences of these ideas had not, however, been thoroughly explored and there existed no comprehensive description of the development of oceans or continents. The development of a coherent picture covering a great range of phenomena was the work of the 1960s, but the origin of some of the ideas goes back much further.

Thirty years before, Holmes (1929) had published a paper in which he discussed the geological effects of radioactive heating in the mantle. He suggested that there will be a rising convection current under a continent which will stretch and thin the continental crust to form a new ocean, a blob of continental material being left in the center to form the mid-ocean ridge (Figure 5a). The two halves of the continent are carried away, riding on the horizontal limb of the convection current and leaving a new ocean between them. On the far side of the two continental halves the moving material meets another convection current and both turn vertically downwards with a thickening of the continent and the formation of an ocean deep. Essentially the same account is given 15 years later in Holmes (1944) except that the ridge is now basalt and not a blob of sial (Figure 5b). Idealized forms for the motion were considered and the forces calculated by Pekeris (1935) and Hales (1936).

A number of writers adopted and modified the views of Holmes: among these were

Griggs (1939), Meinesz (1948, 1952), and Heezen (1960) (Figure 5c). The main attraction of such ideas was, and is, that they provide an adequate force to split the ocean floor and a mechanism for carrying the split continental pieces away from each other. They also account very naturally for the striking fact, established by Menard (1958), that the central valleys of most mid-ocean ridges are rather accurately halfway between the two continental edges. Some doubt was still felt about the possibility of the downturning of the currents and both Carey (1958) and Heezen (1960) avoided the difficulty by supposing that the earth is expanding to accommodate the extra width of the oceans.

The developing ideas on convection currents were brought together in a masterly paper by Hess (1962b) and in brief notes by Dietz (1961, 1962). The main difference

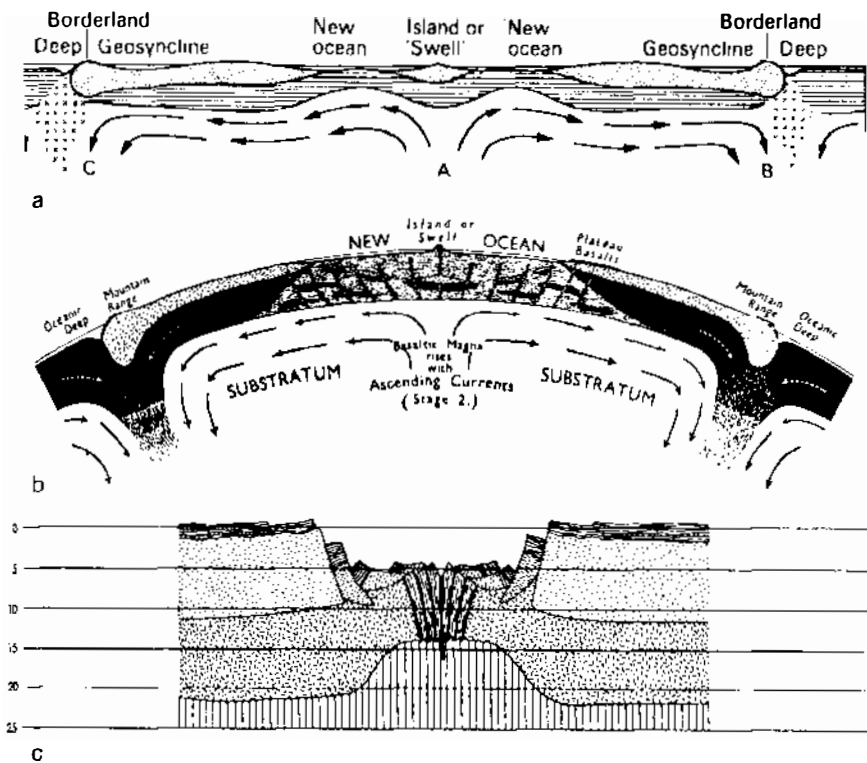


Figure 5 The development of ideas about sea-floor spreading. (a) Holmes (1929): note the blob of sial forming the mid-ocean ridge and the vertically descending currents at the leading edges of the continents. (b) Holmes (1944): as in 1929 but with a basaltic ridge (by permission from Thomas Nelson & Sons Ltd.) (c) Heezen (1960): an early stage in the formation of an ocean on an expanding earth with fracture and intrusion on the ridge axis but no convection currents. (Heezen also gives a diagram with convection currents but with no movement of the continents.) (By permission from *Scientific American*) (d) See next page.

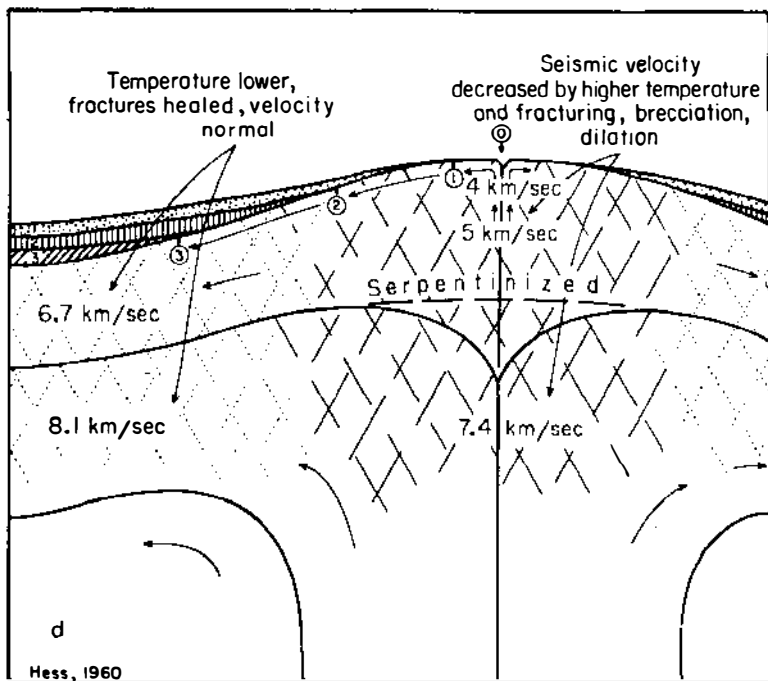


Figure 5d Hess (1962b) showing a convection current rising to the ocean floor under the axis of the ridge and splitting the crust.

from the ideas of Holmes is that the oceanic crust is supposed to be broken to form new sea floor (Figure 5d) whereas Holmes shows it being stretched and thinned (Meyerhoff 1968, Dietz 1968, Hess 1968). The effect of these papers was great; the phrase "sea-floor spreading" (devised by Dietz) put the emphasis on the site of the creation of new crust and took it away from the motions of the continents. Both Holmes and Hess were professional petrologists (as well as many other things) and both of them provided petrological mechanisms to assist motion, in addition to the thermal buoyancy forces. For Holmes it is the basalt-eclogite transformation in the downgoing limb of the current; for Hess it is serpentinization of periodotite in the up-going limb by water from the mantle. The importance of these processes is still in doubt.

The next stage in the development of ideas was the interpretation by Vine & Matthews (1963) of the magnetic lineations on the sea floor. Mason (1958) and Mason & Raff (1961) had described magnetic lineations on the sea floor off California. These lineations implied that the sea floor was magnetized in parallel stripes. The stripes were found to have no relation to the topography of the sea floor or to the form of the buried basement. They were crossed by faults showing displacements of over 1000 km (Vacquier et al 1961). These very striking phenomena defied explanation for some years; for example, Bullard & Mason (1963), in a paper

written in the summer of 1960, had no useful interpretation. A little later Vine & Matthews (1963) suggested that Hess' idea that a strip of new ocean floor was continually being formed on the axis of the mid-ocean ridge would provide a double tape recording of the intensity and the reversals of the earth's magnetic field. Each magnetic stripe was magnetized when that piece of ocean floor was formed in the central valley on the ridge axis. Matthews and Vine came to this idea while analyzing magnetic surveys they had made in the Indian Ocean. The first clue was the discovery of reversely magnetized sea mounts on the ridge (Cann & Vine 1966). Suggestions closely parallel to those of Vine and Matthews were made in a paper written independently and at about the same time by L. W. Morley of J. T. Wilson's department in Toronto. By regrettable errors of editorial judgment both *Nature* and *Science* rejected this paper. Some extracts from it and a commentary were published much later by Lear (1967).

In 1965 Hess, McKenzie, Matthews, Vine, and Wilson were all in Cambridge and rapid progress was made. Wilson devised "transform faults" to explain the offsets of the ridge axis and the magnetic pattern. It had generally been supposed that the offset sections of ridge axis were moving apart and that the faults were analogous to the "transcurrent faults" seen on the continents. Wilson (1965) suggested that the sections of ridge axis were not moving apart and that the motions of the faults were those of two plates moving away from each other and away from the ridge axis (Figure 6); with this arrangement only the section of the fault joining the two ridge axes is active. On the older view it is unclear how the fault can end; the similar difficulty with transcurrent faults on land, such as the Great Glen Fault in Scotland or the San Andreas fault in California, had usually been met by saying that they "ran out to sea," i.e. passed beyond the ken of geologists.

In March 1964, while these discoveries were being made, the Royal Society held a discussion on continental drift, the proceedings of which were later published as a book (Blackett, Bullard & Runcorn 1965). A number of people from the USA attended and there were some complaints that the meeting had been packed with believers and that the opponents had not been given a proportionate representation. This was not a subtle plot; the fact was that almost everyone working in the field in England had become convinced a year or two before a comparable near unanimity was reached in the USA.

In 1965 Vine moved to Hess' department in Princeton where he produced two papers (Vine & Wilson 1965, Vine 1966) which left no doubt of the correctness of the explanation of the magnetic stripes. Soon after this, Sykes (1967) showed that the earthquakes on the ridge axis and on the transform faults had the expected types of

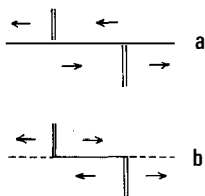


Figure 6 Faults on the axis of a mid-ocean ridge (adapted from Wilson 1965): (a) a transcurrent fault; (b) a transform fault (the dashed section is inactive).

focal mechanisms. These successes convinced the major groups working on marine geology and great effort was expended in analyzing the considerable stores of unpublished magnetic data and in obtaining new data. The result was several maps showing the age of the basement beneath the sediments for a large part of the oceans and an extension of the history of reversals back into the Jurassic. In this very substantial undertaking, the work at the Lamont Geological Observatory, especially that of Heirtzler et al (1968) and Le Pichon (1968), was particularly significant.

The turning point for opinion in America was marked by a conference in New York in November 1966, sponsored by NASA (Phinney 1968). At this the worldwide evidence from the magnetic lineations and the earthquake epicenters on the ocean ridges was presented. The effect was striking. As we assembled on the first day, Maurice Ewing came up to me and said, I thought with some anxiety, "You don't believe all this rubbish, do you, Teddy?" At the end of the meeting I was to sum up in favor of continental movement and Gordon Macdonald against; on the last day Macdonald was unable to attend and no one else volunteered to take his place. I attempted to say what I thought Macdonald would have said, but it was unconvincing and I left it out of the published account. In my summary I pointed out how far we had gone from the traditional geological interest in the continents and their mountain systems and recommended a return to these problems. I also stressed the importance of deep-sea drilling in the verification and extension of ideas about the sea floor.

## PLATE TECTONICS

The transition from sea-floor spreading to plate tectonics is largely a change of emphasis. Sea-floor spreading is a view about the method of production of new ocean floor on the ridge axis. The magnetic lineations give the history of this production back into the late Mesozoic and illuminate the history of the now aseismic parts of the ocean floor. This naturally directed attention to the relation of the sea floor to the continents. There are two approaches: in the first, one looks back in time to earlier arrangements of the continents; in the second, one considers the current problem of the disposal of the rapidly growing sea floor.

At the same time as the ideas of sea-floor spreading were developing, there was a revival of interest in what had been Wegener's strongest argument in favor of drift: the geometric, structural, stratigraphic, and paleoclimatological fits which suggested the former existence of the giant continent of Pangea. The early work on the geometric fits was, as has already been said, sloppy, amateurish, and, in some degree, dishonest. Fits were shown in sketches which implied unrecognized, or at any rate unstated, distortions. Among the reasons for this were the expectation that the fits would not be close and the difficulty of making fits objectively in the precomputer age.

The first man to make careful geometric fits was Carey (1955, 1958) who worked with transparent caps which could be moved over the surface of a globe. The resulting fits around the Atlantic were much better than anyone expected; the impact of his 1958 paper was less than it might have been because the maps were

published only on a small scale in a duplicated conference report which contained much other material of a more or less controversial character connected with Carey's theoretical views on the expansion of the earth.

Influenced by Carey's results, and stung by Jeffreys' oft repeated disbelief in the closeness of the fits, I began to consider how one could best make fits that were, in some defined sense, best fits using the data from maps, which are much more reliable and detailed than any globe. In this I was joined by Everett and Smith. We realized that the movement of a continent could be defined by three parameters and that the problem was to choose a criterion of goodness of fit and use it to determine the parameters. By analogy with least squares fitting it is convenient to express the criterion as the finding of the values of the three parameters that minimize a symmetric function of the coordinates of points on the two continental edges after one of them has been displaced. A vague memory of undergraduate mechanics led me to Routh's *Rigid Dynamics* and to Euler's (1776) theorem that any motion of a sphere over itself can be regarded as a rotation about some pole of rotation. The problem was then to find the position of this pole and the amount of the rotation (many other choices of parameters are possible, for example the "Euler Angles," but our choice seems to have established itself). The results for the fit around the Atlantic were spectacular, the root mean square gaps and overlaps being only about 50 km. The paper describing this work (Bullard, Everett & Smith 1966) was given at the Royal Society symposium in 1964. It had an impact much greater than we had expected; the reason was, I think that we had provided professionally drawn diagrams on a large scale (perhaps fortunately, computer plotters were not available to us) and had confined ourselves rigidly to the geometric problem. Our intention was to produce the facts about the geometric fits of the continental edges and to leave all other questions, such as the corresponding structural and stratigraphic relations, to be considered by the local experts who could use our map as a base for their investigations. We thus avoided arguing many controversial questions and did not allow the opponents of drift to concentrate on these and to ignore the irrefutable geometry. We stressed certain arbitrary features of our reconstruction, such as the abolition of Iceland, the retention of Rockall Bank as a continental fragment, and the rotation of Spain. All these have since been justified by other evidence and greatly strengthened our position. The subsequent geochronological and structural studies have also been favorable to our fit (e.g. Hurley & Rand 1968). The present summary provides an opportunity for me to say that it seems that Everett and Smith have had less than their share of credit for this work. The fit around the Atlantic has gradually, with repeated quotation and reproduction, become "Bullard's fit" (an undetected editorial change once led me to join in this injustice); in fact, the work was almost all done by Everett and Smith and I only provided the methodology and wrote the paper. In one sense our deliberate concentration on the geometric fits was an undesirable limitation which inhibited us from thinking of wider questions. We should at any rate have fitted the ridge axis and earthquake epicenters to the continental edges and thus connected with the work of Hess. We intended to do this but did not do so since we wished to get the paper finished in time for the report of the Royal Society conference (Blackett et al 1965). The corresponding fit of the continents

around the Indian Ocean cannot be done by geometry alone and is a much harder task; it has been performed by Smith & Hallam (1970) and by McKenzie & Sclater (1971), following an earlier and unsatisfactory attempt by Wilson (1963). The Atlantic now needs re-examination in the light of new bathymetric and magnetic data, particularly those from the Arctic Ocean.

The work on continental fits is concerned with the movement of large portions of the earth's surface which may be thought of as rigid, aseismic, spherical caps or plates carried hither and thither by convection currents. Some of these plates are so large that it is not easy to believe that they behave as rigid bodies. That they do so was demonstrated by McKenzie & Parker (1967) and by Morgan (1968) (who introduced many of the most convincing arguments and coined the phrase "plate tectonics"). Numerous checks are possible from the focal mechanisms of earthquakes and from the geometry of magnetic lineations and transform faults (Le Pichon 1968).

The recognition of large, rapidly growing, aseismic plates raises an acute problem in their disposal. There are only two possibilities: either the earth is expanding rapidly or sea floor is being destroyed. There are many objections to rapid expansion, though it still has its supporters, and plate destruction is the generally accepted process.

If plates are being destroyed, the likely sites are along the broad earthquake belts associated with island arcs. It has long been known that these earthquakes are distributed on planes dipping beneath the arcs at about  $45^\circ$  and reaching depths of up to 700 km. These deep earthquakes were discovered by Turner (1922), but decisive evidence was only available much later in the work of Stoneley (1931) and Scrace (1931). The inclined plane of earthquakes was at first interpreted as a thrust fault, which in a sense it is (Visser 1936, Benioff 1954), and only much later as a downgoing plate diving under the continent, heating, and becoming incorporated in the mantle. It is frequently referred to as the Benioff zone.

Surprisingly this inclined plane of earthquakes is not mentioned by Dietz, Hess, or Heezen in the early papers on sea-floor spreading or in the report of the NASA conference (Phinney 1968). Most of these papers have the downgoing limb of the convection current going vertically down. I do not know who was the first to show the solid plate bending over and moving obliquely down beneath the continent. There is an almost insensible gradation from the rival theories of thrust fault and tectogene of the 1930s to the descending plate. The idea is clear in Oliver & Isacks (1967) and in Elsasser (1967). The real existence of the cold, descending plate was demonstrated when it was shown that it transmitted seismic waves with higher velocity and less attenuation than the surrounding mantle. This was first suggested by Katsumata (1960) in a paper published in Japanese. Isacks & Molnar (1971) have collected all the available information.

After 1967 the subject of plate tectonics blossomed into a major scientific enterprise with an enormous bulk of publications. It would be difficult to pursue the details and perhaps not very useful; there are many textbooks on the current state of the subject (e.g. Le Pichon et al 1973). The main lines of work have been: (a) the application of the ideas to particular areas (e.g. the Red Sea, the Mediterranean, the Bay of

Biscay, the Galapagos Islands); (b) the explanation of the topography and heat flow of the ridge as a result of cooling of the outward moving plate; (c) the vindication of the ideas of sea-floor spreading by the results of the JOIDES drilling; and (d) the development of the theory of convection in the mantle (this is a subject of extreme difficulty and still has a long way to go).

## THE OBJECTIONS

In an earlier section the main objections raised in the 1920s and 1930s to Wegener's views are listed. It is interesting to see how these have been met in the theory of plate tectonics: (a) The magnetic lineations and the results of deep-sea drilling make it clear that the separation of Greenland from Europe occurred in the Cretaceous not in the Quaternary. (b) Nothing has been heard for a long time of Washington's (1923) claim that the rocks on the two sides of the Atlantic do not match and there is much evidence that they do match (e.g. Hurley 1972, Miller 1965, Stoneley 1966). (c) It seems likely that from the Devonian to the Permian there was a large continental mass bearing some resemblance to Wegener's Pangea but all recent reconstructions show a wide ocean between the south coast of Asia and the Australian-Indian block, which was attached to South Africa (Smith & Hallam 1970). It is probable also that Eurasia was split into several plates; only the most westerly, which included everything west of the Urals, are attached to North America and Africa. The large continent was a temporary thing; it is clear that North America and Europe were separate in the lower Paleozoic and it seems likely that the continents have a long history of splitting, movement, and collision. There is nothing exceptional about the formation of the Atlantic in the Mesozoic; Africa is splitting from Arabia today and the Ur-Atlantic closed 450 m.y. ago. (d) Thermal convection seems to provide a plausible mechanism for movement; this is discussed below. (e) The continents do not push through the ocean floor, they are carried on the horizontal limb of a convection current. The ocean floor achieves its motion relative to the leading edge of the continent by diving beneath it, the solid oceanic plate being bent over and broken in the process. As would be expected, the formation of mountains is not a simple process. Some, such as the Andes, derive a large part of their material from the melting plate which supplies andesitic lavas. The Himalayas are the result of a collision of two continental blocks. Most mountains also contain material derived from the floor of the deep ocean (ophiolites), perhaps in some places scraped from the descending plate.

In recent years the ideas of plate tectonics have met with very general acceptance but there are still a number of respected figures who are convinced of the fixity of the continents. This note is concerned with the history of ideas and it would be inappropriate to attempt a detailed refutation of the opponents of the ideas presented. It is, however, desirable to show the nature and origin of the objections and how they fit into the scheme we have presented. The objectors may be divided into three classes: (a) some paleontologists who believe that what is known of the distribution of animals and plants in the past is incompatible with the proposed motions of the continents; (b) Sir Harold Jeffreys who believes the motions to be incompatible with

the known rheological properties of matter; and (c) Dr. V. V. Belousov who is concerned that the proposed scheme does not provide for the great vertical motions that are observed in the sedimentary basins of the continents.

The main proponents of fixed continents among the paleontologists are D. I. Axelrod and A. A. Meyerhoff. I am clearly incompetent to discuss the paleontological evidence and will merely say that the absence of a consensus of paleontological opinion after 60 years of discussion does not encourage me to look there for decisive criteria. References to the extensive literature will be found in Axelrod (1963) and Meyerhoff & Meyerhoff (1972). It is desirable that someone or some group should produce a review of the paleontological and related paleoclimatological evidence and arguments adduced by the objectors. The papers are long and indigestible and contain a number of statements which seem implausible, such as the claim that there was glaciation at the equator in the Pleistocene.

Meyerhoff's attack on the central evidence for movement (paleomagnetism, the magnetic lineations, the continental fits, and the earthquake epicenters and focal mechanisms) seems to me unconvincing. Like Wegener, he argues too hard. For example, he says, correctly, that published maps show bad correlation of magnetic anomalies between the south end of the Rykjanes ridge and the Azores, but he does not pause to consider whether this is really so or whether the published observations are not dense enough to show the real continuity in the presence of numerous transform faults. In fact, observations published subsequently to Meyerhoff's paper show satisfactory continuity in this area (Vogt & Avery 1974). There is a methodological difference between Meyerhoff and his opponents which is of general interest. There are places where there are excellent, well-separated linear magnetic anomalies stretching for hundreds or thousands of kilometers and where correlations and symmetry are irrefutable. But there are other areas where complicated things have happened and where the anomalies are close together, unclear, and are broken up by transform faults. The believers in movement attempt to extend the ideas derived from the simple areas into the more complicated ones and thereby understand complex events that are unclear from the local evidence. The unbelievers seize on the doubts about the difficult areas and use the difficulties to shed doubt on the work elsewhere, which is for most people perfectly clear. The difference is not peculiar to magnetic survey. Most subjects have, and need, their lumpers and their splitters; the people who like to see a simple pattern against which to judge the complexity of the detail and the people for whom nature is too complex for any theory and the details are their own reward. It is, I fear, inevitable that the first group reach the great generalizations (see e.g. Watson 1968).

The objections of Jeffreys are of a different kind. He is the deviser of much of the theoretical framework of geophysics and has convinced himself that motion is impossible on mechanical grounds. He says that he is not an expert in magnetism but, since motion cannot occur, the observations must have been misinterpreted and that it is not his business to find out what has gone wrong. His objections are of long standing. In the first edition of his celebrated book, *The Earth* (Jeffreys 1924), he shows that the forces proposed by Wegener are inadequate by many orders of magnitude to produce significant motions. This is repeated in all subsequent editions and

is, beyond question, correct. The first edition also argues that the widespread fields of gravity anomalies of one sign show that strengths of the order of  $10^8$  dyn/cm<sup>2</sup> must exist at depths between 100 and 400 km; this would prohibit motion.

The second edition of 1929 adds the statement that the African and South American coastlines will not fit; this is correct and is repeated in all subsequent editions. However, it is irrelevant as Wegener's diagram, and all the later ones I have seen, do not fit the coastlines, they fit the continental edges at the outer margin of the shelf (it must be confessed, though, that Wegener's diagram is in places far from clear). For the continental edge the fit is excellent.

The third edition of 1952 and the fourth of 1959 were the first that had to cope with the ideas of convection in the mantle put forward by Holmes. Jeffreys considers that if such currents exist they could break the crust but believes that the broken blocks would pile up and stop the surface movement before it had gone far. He still thinks that the material to a depth of 600 km is likely to have a strength of around  $3 \times 10^8$  dyn/cm<sup>2</sup>, though he has lost his main earlier reason for believing this. The widespread gravity anomalies which previously needed strength at depth to support them could now be due to the density differences in the convecting system. Vertical motions of the surface, not accompanied by gravity anomalies, could also be related to motions in the mantle. Of this and related matters Jeffreys says only, "The suggestion of systematic convection currents complicates the question", which indeed it does.

In the 1970 edition of *The Earth* the previous argument is repeated including the odd statement that the main evidence for drift is paleontological. In a note on the last page it is admitted that the continents around the Atlantic do fit and that this might be due to the break-up of a large continent formed early in the earth's history (this cannot be true as the broken rocks are of all ages up to lower Mesozoic). The main objection to motion is still the belief that materials within the earth have a finite strength. This belief is based largely on the experiments of Lomnitz (1956). These experiments were conducted at atmospheric pressure and room temperature on time scales of up to a week. There seems no reason to suppose that they have any relevance to the properties of materials within the earth and the resulting "law" is in fact contrary to experiments with ceramics at high temperatures and pressures where viscous creep is observed. Theory provides a number of mechanisms which are important at high temperatures and some of which give zero strength. The whole question has been discussed by McKenzie (1968) who gives references to both experiments and theory.

Our last objector is Belousov (1968). He makes the important observation that plate tectonics does not account for the large vertical motions that have taken place in and around the continents; for example, in the North Sea. This is true and needs to be said; the pendulum has swung so far that the traditional geological interest in vertical motions, rising mountains, and deepening sedimentary basins has been largely replaced by discussion of very large horizontal motions. It is certainly time to return to the older problems, but to do so it does not seem necessary to throw away what we have learned about horizontal motion. Belousov (1962) calls continental motion a "completely sterile idea" which is only a reasonable opinion for

one who discounts all the evidence for large horizontal motions. It is only fair to say that Belousov (1962) is a translation of a book written in 1956; I do not know if he would be so vehement today. Belousov believes that continental crust can be converted to ocean floor; the process involves great petrological difficulties. He also says that the equality of the continental and oceanic heat flows excludes the possibility of horizontal movement; the position of plate tectonics in this matter is certainly unsatisfactory in that the equality is not easily explained and appears as a kind of accident. The difficulty is the more surprising as the variation in heat flow across an ocean is in good agreement with the expected cooling as the plates move away from the ridge.

## CONCLUSION

The development of some of the ideas of plate tectonics goes back to the work of Wegener in 1912 and Du Toit and Holmes in the 1920s. Its development as a viable theory has involved fundamental changes in Wegener's ideas about the kinematics and mechanics of the motions, but it is still Wegener's theory in the sense that it is concerned with the movement of large plates over horizontal distances of thousands of kilometers. Its wide acceptance in the 1960s and 1970s is in large part the result of the study of paleomagnetism and the sea floor. The introduction of lines of investigation that were not part of the stock-in-trade of most students of the earth has raised great difficulties in communication, the more so because the new work was for some years largely carried out by people whose training had been in physics. Physicists are not the easiest people with whom to carry on a discussion; they have a high opinion of their own abilities and are apt to believe in Lord Rutherford's classification of the sciences: "There's physics and there's chemistry, which is a sort of physics, and there's stamp collecting". The surprising thing is that in the last 20 years common ground has been found among the very heterogeneous group that makes up the students of the earth; a common language and a degree of mutual tolerance have been developed. Nothing succeeds like success and the success has drawn some of the brightest young men to the study of the earth. The object of this essay is to give an account of the rather devious route by which the success was achieved.

Just as I was finishing, the news came of the death of Maurice Ewing. It was he, more than any other man, who provided the fuel for the revolution in earth science. I once asked him "Where do you keep your ship?". He replied, "I keep my ship at sea". Over nearly 40 years he and his colleagues kept their ships at sea and provided the major part of our new knowledge.

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<sup>1</sup> This list of references is unsatisfactory in that, although it is long, it leaves out many important papers. I have concentrated on the earlier period since bibliographies and textbooks are easily available for the work of the last ten years. The list is also unsatis-

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factory in another way: it contains too many of my own papers. This superfluity is due, not to a superabundance of contributions to the subject, but to my having used the reviews that I have written since 1939 to describe the development of thought. I might have used other people's reviews, but I thought it more interesting and conducive to a coherent account if I followed my own successive steps to understanding.

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