# A REVIEW OF THE CONTINENTAL DRIFT HYPOTHESIS

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**The author discusses the Wegener hypotheses and some possible causes of continental drift.**

Late in 1926 a Symposium on Wegener's theory of continental drift was held in New York by the American Association of Petroleum Geologists. In the course of a spirited and fruitful discussion some of the leading geologists of America and Europe expressed their considered opinions on an extremely complex group of problems, and the Association has now happily made these contributions available to a wider audience (**1**). Meanwhile Dr. A. L. du Toit, an independent advocate of continental drift, had completed a report on a comparative study of South Africa and South America made possible by five months intensive work in Brazil, Argentina and Uruguay (**2**). Still more recently in his Presidential Address [1] to the Geological Society of South Africa, du Toit has compared these two land masses from the point of view of metallogenic provinces (**3**). Finally, Mr. E. B. Bailey in a masterly address to Section C of the British Association at Glasgow has given us an unrivalled comparative study of the Palæozoic Mountain systems of Europe and America (**4**). The time is therefore opportune, not only for a review of these publications but for a general survey of all the relevant data and hypotheses.

THE WEGENER HYPOTHESES.—Advocates of the Wegener group of hypotheses assume that during Palæozoic time the continents were assembled together more closely than they are at present. Antarctica, Australia and India were grouped against southern and eastern Africa, and these, with South America against western Africa, formed a single large continent—a compressed equivalent of the more familiar *Gondwanaland* of orthodox geology. Similarly, in the north, North America and Greenland formed with Europe and Asia a continental block which has since become known as *Laurasia*. To the combinations of these two, Wegener gives the name *Pangæa*: his conception of it is illustrated in Fig. 1.

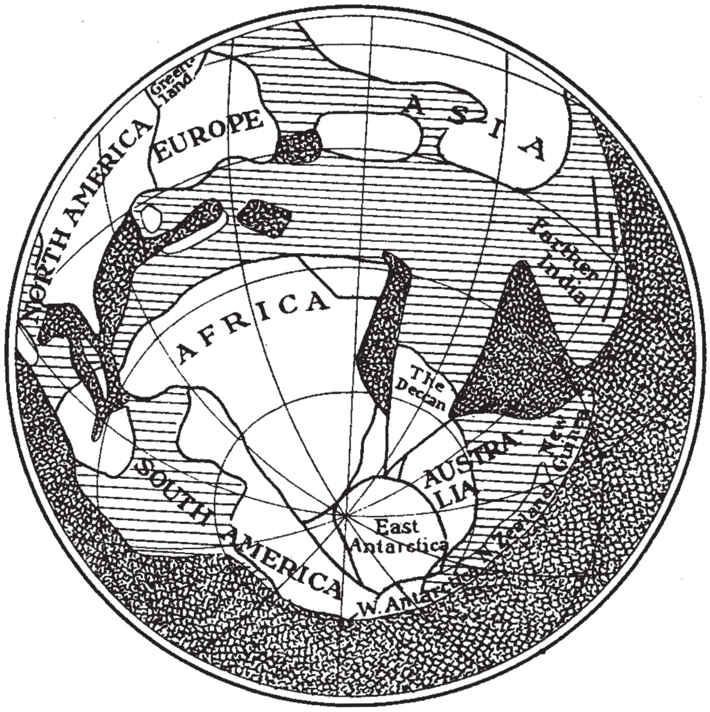


FIG. 1.—WEGENER'S CONCEPTION OF THE WORLD IN THE CARBONIFEROUS PERIOD.
Heavily shaded portions indicate deep seas; horizontal lines shallow water; unshaded portions dry land.
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[1] Abstracts appear elsewhere in the April issue of *The Mining Magazine*, 1929.

It will be observed that in this unfamiliar looking map the South pole is situated near Natal; a second assumption is therefore that there have been extensive geographical changes in the position of the poles. The present distribution of the continents is regarded as a result of fragmentation by rifting, followed by a drifting apart of the blocks; Gondwanaland having broken up during the Mesozoic, and Laurasia during the Cainozoic. The continental blocks are visualized as slabs of granitic and gneissose rocks which, being rich in *si*lica and *al*umina are mnemonically referred to as *sial*. The blocks of sial "float" in a substratum of basic or ultrabasic rock which, being characterized by *si*lica and *ma*gnesia is called *sima*. The lighter sial of the continents projects on an average nearly 5 km. above the heavier sima of the ocean floor. The two chief forces to which Wegener appeals to engineer the drifting process are differential gravitational forces which act on the protruding blocks of sial. They are respectively (*a*) the Eötvös *pohlflucht* or equatorial drift tending to move the continents towards the equator; and (*b*) a westward drift of the continents due to tidal friction.

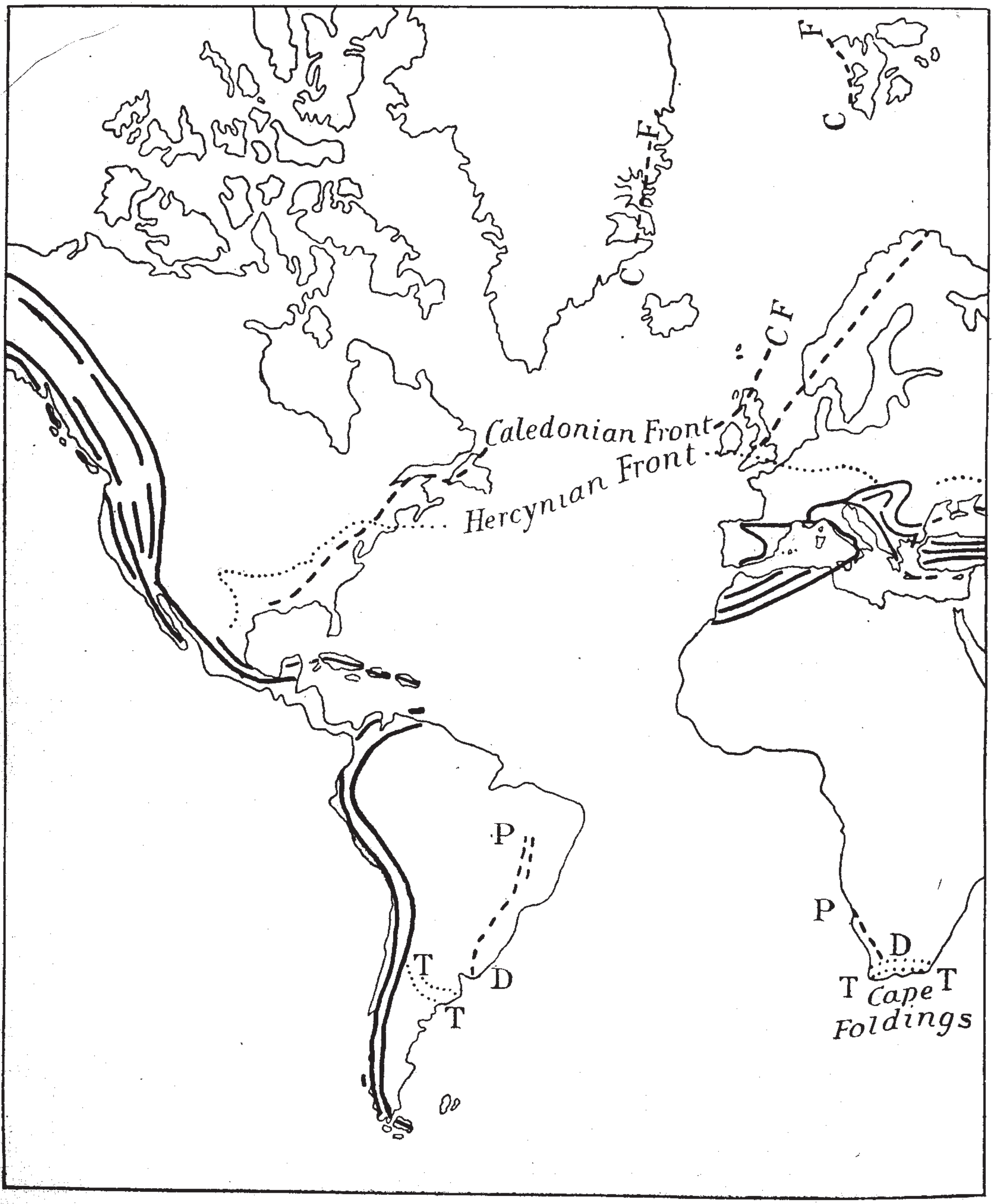
The equatorial drift is illustrated by the relative approach of Africa and Europe and by that of Peninsula India and Asia. Previously, for long geological ages, these more stable regions had been separated by an unstable, steadily down-sinking belt, this constituting the geosyncline known as the Tethys. Between the approaching continents the thick accumulations of sediment became compressed and folded, squeezed and metamorphosed, until by flowage and overthrusting they splayed out over the advancing blocks and rose in thickened contortions between them as the great Alpine-Himalayan mountain system. The New Zealand and New Guinea mountains are also interpreted as results of equatorial drift, the former having been folded while New Zealand was in "the prow of the movement," before it became detached from Australia and was left behind.

The most spectacular example of westerly drift is presented by the Americas with their great Cordilleran ranges facing the Pacific from Alaska to Patagonia. The mountains are regarded as the crumpled front edge of the sial. Lag effects on the eastern margins of the continents are seen in the island festoons of Asia and in the arcs of the Antilles between North and South America, and of the southern Antilles between South America and Antarctica.

It will be gathered that Wegener completely ignores the contraction hypotheses of mountain building; he asserts, in fact, that we have no proof that the earth is contracting. He also rejects the hypothesis according to which the Atlantic and Indian oceans are interpreted as occupying basins produced by the inbreaking of former continental areas, due to greater radial contraction than that suffered by adjacent columns of the crust. Like the similar doctrine of submerged land-bridges this hypothesis appears to be fatally at variance with the implications of both isostasy and seismology. Wegener does not deny that the regions in question were formerly land. What he denies is that the land can have gone down into the depths, and since it is no longer there he adopts the alternative conclusion that part of the land has glided away sideways relatively to the other part, leaving a region where the sial is thin, patchy, or perhaps altogether absent. In reconstructing former hypothetical contacts, allowance must be made, of course, for the fact that continuity of sial with sial does not necessarily mean continuous land. The lower levels of the sial platforms have always been more or less flooded by oceanic waters, as they are to-day in the Baltic and North Sea, and consequently there is no need to visualize Laurasia and Gondwanaland as having been permanently free from epicontinental seas.

THE OPPOSING LANDS OF THE ATLANTIC.—Most of Wegener's critics are concerned to discredit the significance of the original source of his inspiration—the apparent parallelism of the opposite shores of the Atlantic. Van der Gracht rightly lays little stress on the validity of geographical pattern as an argument. If drift has occurred at all it is mechanically impossible that the sial blocks could have moved without both internal and peripheral distortion. Nevertheless, if the Atlantic is really an enormously widened rift, then the remains of transverse structures that existed before the rifting and drifting began, should still occupy positions consistent with their presumed former continuity, though not, perhaps, as Wegener suggests, as closely "as the lines of a torn drawing would correspond if the pieces were placed in juxtaposition." Argand's conception of varying plasticity in the earth's surface layers is a valuable corrective to the exactly fitting coast-lines of Wegener's too dogmatic maps. Matching is to be anticipated, but that it will be as precise as has been claimed is not to be expected.

Schuchert presents a useful summary of the geological similarities and differences between the opposing Atlantic lands. He admits that Wegener is correct in connecting the Caledonian trends of Britain with those of Newfoundland (Fig. 2) but he denies that the Hercynian trends of Europe connect with the Appalachians. Against this view we may refer to Bailey's tectonic maps of



Europe and America, and his explanatory statement of the comparison (**4**). "For the last time let us take boat across the Atlantic, there to visit the American representative of the Hercynian System. We know exactly where to go. From New York southwards, the north-west front of the Appalachian complex consists of folded and often overthrust Palæozoic sediments that extend upwards into Coal Measures." The latter, he tells us, were "derived from the waste of the growing Hercynian Mountains, and we follow Bertrand in our thoughts to South Wales, the Ruhr and Upper Silesia." A most remarkable feature—recognized by Wegener and emphasized by Bailey—is the westward convergence of the Hercynian and Caledonian chains. On the east they are far apart in Poland and Lapland respectively. They come into contact in South Wales and Ireland, but the greater part of Ireland still lies between the two fronts. Across the Atlantic the geology of the Atlantic States "is summarized in the words *Where mountains cross*," and finally, "the Hercynian Mountain front steps clear of its Caledonian predecessor." The crossing begun in South Wales is completed in Pennsylvania.

It is equally startling to find that the overthrust Caledonian front has been discovered along the east coast of Greenland precisely where one would look for it if Greenland had formerly linked up the North-west Highlands with Spitzbergen. As shown on Fig. 2, the north-west margin of the Caledonian chain is missing from Norway. Was it torn away when Greenland broke loose and began its hypothetical drift to the west?

Turning now to the South Atlantic with du Toit as our guide,[1] we find a similar set of tectonic coincidences. Pre-Devonian folds known as the *Brazilides* trend from Minas (Brazil) to Maldonada (Uruguay). Across the Atlantic are the post-Nama foldings extending from Lüderitz to Caledon (PD in Fig. 2). Not only is there a general lithological resemblance between the two belts of folded strata, but in each area the latter are invaded by similar granites and succeeded by similar successions of Devonian and Gondwana formations, including late Carboniferous tillites. In both the Cape and the Argentine, mountain-building set in again about the beginning of the Triassic. The *Gondwanides* of South America are crumpled and overthrust to the north; so are the corresponding Cape foldings of South Africa (TT in Fig. 2). Just as in the northern hemisphere a crossing begun on the European side is completed on the American side, so here the foreshadowed crossing beyond the estuary of the La Plata is accomplished behind Cape Town.

Reference should be made to du Toit's book for a presentation of a most remarkable series of parallels (stratigraphical, palæontological, tectonic, magmatic, and climatic) in the geological history of the opposing lands of the South Atlantic, the whole assemblage of data pointing persuasively—unless Nature be a misleading witness—to the probability of a formerly closer union. An attempt has been made by Schuchert both in the Symposium (**1**) and in a review (**5**) of du Toit's book to explain the resemblances in terms of orthodox geology; to deny that they have the far-reaching significance that is claimed for them; and so to deal "a crushing blow" to the drift hypothesis. This adverse criticism du Toit has no difficulty in answering effectively (**6**). It is worth noting, however, that these doughty antagonists differ less than might be imagined. Avowed iconoclast as he is towards the Wegener hypothesis, Schuchert feels "obliged to conclude that the continents do actually move extensively" in order to explain the crustal shortening implied by mountain structures, and he quotes the impressive statement of Termier that the mountains of Central Asia represent a crustal foreshortening whereby 3,600 miles have been reduced to 1,845 miles. Van der Gracht naturally points out that if lateral movements of the order of 1,800 miles be admitted, then continental drift even on the scale visualized by Wegener becomes fully possible. On the other hand du Toit quite reasonably differs from Wegener by ruling out actual contiguity of the present continental borders as unwarranted. He is content to assume that the distance between the opposed shore lines was never less than 250–500 miles, the intervening space being then, of course, continental, with or without temporary invasions of shallow seas.

This departure from the closely fitting shores of Wegener is justified by the geological evidence, and is essential from the point of view of isostasy. Seismological evidence makes it clear that the Atlantic floor differs from that of the inner Pacific in having a thin and patchy covering of sial. Indeed,

[1] See map, *The Mining Magazine*, April, 1929, p. 247.

the S-shaped central swell of the Atlantic transmits surface seismic waves at almost the same velocity as a belt of sial of continental thickness would do. Now this is just the surface structure that would be expected to result from the stretching that would necessarily accompany the drifting apart of two continental areas. Between the separating slabs of sial there would be a belt that would become increasingly thinner, by flowage in the lower levels, and by fracture and faulting in the brittle upper levels (**7**). Molengraaff and Taylor (**1**) both regard the mid-Atlantic swell as the cicatrix of the main fractures that led to separation, and van der Gracht is inclined to agree. If now we imagine the continental slabs to be closed up again, it will readily be realized that the swell and the thinned-out sial of the adjoining ocean floor would be crowded together into a broad belt of continental thickness forming a land mass that would intervene between the present coast lines and prevent their contact by some hundreds of miles.

Similarities Between Mineral Provinces.—In du Toit's Presidential Address (**3**) he views South America no longer as a remote continent, but as a close relation which may be expected to have mineral deposits akin to those of South Africa. Expectation is fulfilled on a spectacular scale in the case of the diamondiferous deposits. Occurrences of late Cretaceous kimberlite and alnöite have been found in Rio de Janeiro and Minas, and the famous deposits of the Diamantina area are now known to be eruptive breccias filling gigantic pipes or fissures like those of the Saltpetre Kop type in the Cape. Further north the diamondiferous area of the Guianas is mirrored across the ocean by those of Liberia and the Gold Coast. Here too there are gold deposits on each side and it is further noteworthy that the belt of Silurian and Devonian rocks of the Lower Amazon Syncline appears to be continued through the Gold Coast into the Sahara. These and many other striking relationships go far to support du Toit's contention that "it is surely more than mere coincidence that the world's diamondiferous deposits, situated near to or upon the coast, should be all but confined to the regions lapped by the South Atlantic."

Du Toit next compares the manganese occurrences of Brazil with those of Griqualand West, and he shows that even so academic a problem as that of continental drift may have practical bearings in the field of mining. Minerals found in a particular geological setting on one side of the Atlantic may not unreasonably be looked for in similar settings on the other side.

It is worthy of notice in this connection that it has recently been shown by Arthur Bray (**8**) that the source of the Gold Coast banket lay to the south-east, and that the deposits themselves represent either a river-delta or a littoral shingle-beach. Thus where the Atlantic now lies there must formerly have been a great river, possibly transporting gold from a continent that has since either subsided far beneath the waves, or drifted away to the west where, perhaps, part of it is still to be recognized in the auriferous tracts of the Guianas and Brazil.

It may be objected that in the north the Hercynian tin ores of Cornwall are not repeated in the Appalachians. But here a little reflection shows that the apparent exception is hardly one for surprise. The Caledonian ranges are everywhere practically devoid of tin, and if the earlier Palæozoic magmatic activity failed to concentrate the element, then a later Hercynian reworking of the same belt could hardly be more successful. Tin occurs in Europe where the Hercynian ranges stand alone, but as soon as the older mountains are crossed it is found no more. The case of pitchblende provides a contrasted example. The Hercynian pitchblendes of Joachimstal and St. Ives are mirrored across the Atlantic by the uraninites of Connecticut and the Carolinas. But the uraninites of Connecticut are of two different ages, one corresponding to the Hercynian of Cornwall and the other to an earlier, probably Lower Devonian, period of ore-genesis.

Where ranges approach and cross, it is not to be expected that the later tectonic structures and magmatic accompaniments will be exactly of the same age, type or composition as those found well away from the earlier site of orogenesis. Moreover, the earlier structures will necessarily be strongly modified by the passage across them of a second earth storm. These reflections are occasioned by Krenkel's recent declaration that the mountain systems of South America and South Africa differ both in tectonic arrangement and age (**9**). The detailed reply to this criticism may safely be left to du Toit, but it may be pointed out that for still another reason the criticism cannot be fatal to the drift hypothesis, for successive stretches of the Alpine-Himalayan system differ in structure and age just as widely and yet are visibly continuous.

Among radioactive minerals possibly the most significant coincidence of occurrence is displayed by thorianite. The name immediately suggests Ceylon, and according to Wegener's reconstruction of Gondwanaland, Ceylon formerly lay to the east of Madagascar. Thorianite, as it happens, is known to occur only in Ceylon and in one other place. That place is Madagascar! If Fig. 1 is to be believed in principle then some day the mineral may also turn up in Western Australia and Antarctica.

CRUSTAL STRUCTURE AND ISOSTASY.—In Professor J. W. Gregory's contribution to the Symposium (**1**) he does not positively object to the drift hypothesis, but he maintains his long-held opinion that the main cause of the present distribution of land and sea is to be found in uplifts and subsidences due to the shrinking of the earth, the latter process being only in part a consequence of cooling. This is, of course, the view of the older orthodox geology, and it should be clearly realized that those who hold it must be prepared to face geophysical difficulties just as serious as any with which the advocates of continental drift can be confronted.

The comparative study of seismograms, and especially the outstanding work of Dr. Harold Jeffreys in this difficult field (**10**), has shown that in addition to the veneer of sediments over the continents there are three layers to be considered in connection with the propagation of earthquake waves beneath the continents. These are:—

(*a*) *The upper layer*, identified with granite and gneiss, and having a normal thickness of about 10 km.

(*b*) *The intermediate layer*, having a normal thickness of about 20 km. or a little more. Jeffreys cautiously favours an identification with tachylyte, a suggestion which I have adversely criticised, pointing out that quartz-diorite or diorite would be equally consistent with the seismic wave velocities (**11**).

(*c*) *The lower layer*, or *substratum*, continuing downwards with no important break to a depth of 2,900 km. It forms by far the greater part of the shell of the earth (surrounding a probably metallic core) and it is everywhere present beneath the higher layers of both continental and oceanic regions. Its nature is in doubt. It has been regarded, by different authors, as crystalline or glassy dunite, or as eclogite passing down into peridotite. It may be added that it is probably for the most part in a glassy state; that its temperature is such that although it is rigid, it is devoid of permanent strength except possibly near the top; and that it is and has been the main source of basic and ultrabasic magmas, including the plateau basalts.

Observations on the Pacific floor are few, but they are consistent with the presence of an outer layer of gabbro. A recent analysis of data made by Hiller (**12**) gives for certain surface waves the following velocities: 3.69 km./sec. through the Pacific floor; 2.87 through Eurasia; and 3.58 through the Atlantic floor. In the Atlantic swell the velocity is 2.9, but elsewhere the Atlantic floor is clearly far from continental. The supposedly sunken lands are not there, except for a thinned-out layer of sial that is probably very variable in thickness, reaching its maximum in the central swell. Yet now there is undoubtedly ocean floor where once there was land, and the former sial of that area must still exist somewhere.

The physical conditions that would bring this picture into harmony with the view that the Atlantic has been formed by inbreaking involve: (*a*) An increase in density of the former sial from 2.7 to 3 (in round figures) in order that it could sink in accordance with isostasy; and (*b*) a change of mineral facies to one which would act towards seismic waves nearly as gabbro would do—these changes not affecting the adjacent lands.

We know of no kind of process, metamorphic or otherwise, that could lead to such results. The alleged tachylyte layer of Jeffreys could do a good deal in this direction by crystallizing in the heavy eclogite facies, but as the seismic evidence for the Atlantic speaks for the presence there of a floor of gabbro, a solution on these lines appears to be ruled out. Moreover the association of island volcanoes and plateau basalts with the formation of the Atlantic and Indian oceans suggests deep-seated conditions of fusion rather than of crystallization.

There is, however, one process by which continental sinking could be brought about. This involves removal of the intermediate layer and of much of the granitic layer by magmatic currents initiated in the still deeper substratum. A kind of magmatic denudation acting on the lower surface of the former continent is visualized, the material being sufficiently fused or otherwise capable of flow to be transported partly under America and partly under Europe or Africa. To a limited degree this may be a genuine process, especially in generating geosynclines; but by itself, it leaves all the collateral problems of mountain-building and climatic changes unsolved. Moreover, if it be a real method of sinking former continents, then it implies a process capable of stretching and transporting the continents themselves. Van der Gracht (**1**) hints at this when he asks, "Is not possibly the whole process [of continental drift] more similar to ice floating on flowing water than to a raft sailing over a currentless pool?" Apart from this, and a suggestion by the same author of "a plastic outflow of the interior continental masses toward their margins," the possibilities arising from currents in the substratum appear to have been entirely overlooked during the Symposium.

LATE CARBONIFEROUS GLACIATIONS.—Clearly it is at least as difficult to sink continents as it is to tear them forcibly apart, and in the absence of a clear geophysical lead one must choose between the alternatives of vertical or lateral displacement on their individual merits in relation to other problems. Here continental drift has undoubtedly more than one decided advantage. The opponents of drift have no way of explaining the distribution of the late Carboniferous glaciations of Gondwanaland, which accordingly continues to be the basis of Wegener's most powerful argument. The presence of extensive tillites of the same geological age in regions such as South America, South Africa, India, Tasmania and Western Australia (including many widely distant parts of the present tropics) constitutes a hopeless riddle unless we assume with Wegener that the glaciated lands were then grouped about South Africa, which in turn is assumed to have been very near the South Pole. With this arrangement the ice-sheets all fall within an area of about the same size as that glaciated in the Northern hemisphere during the Quaternary ice-ages. As to the actual date of the late Palæozoic glaciation there is still some doubt. Most British geologists regard it as late Carboniferous, but Schuchert (**26**) has recently forcibly maintained his view that it was Middle Permian. In both readings of the stratigraphical evidence it is recognized that there were somewhat earlier and later glaciations in New South Wales than can be recognized elsewhere.

In a recent book of fascinating interest Dr. C. E. P. Brooks has made a valiant attempt to demonstrate that these astonishing climatic events "were the logical result of the distribution of land, especially high land, and sea during that period, the poles being supposed to have kept their present positions" (**13**). For this purpose he adopted as a working hypothesis "a great plateau in the interior of Gondwanaland, rising gradually to an elevation of 10,000 feet." This assumption of great height certainly eases the meteorological problem, but it has no geological justification. Moreover, it doubles the difficulty of the physical problem, for now we should have to explain, first a great thickening of the sial of Gondwanaland and then its total disappearance from the very extensive oceanic areas that now intervene between the existing southern continents.

Brooks himself is not satisfied that his geographical solution explains the glaciation of India, and the non-discovery, so far, of satisfactory evidence of Antarctic glaciation during the late Carboniferous or early Permian is certainly more damaging to this attempt at a solution than it is to the less rigid hypothesis of continental drift. In the geographical solution the belt of Upper Carboniferous coalfields that stretches from North America through Europe and on to China naturally cannot be regarded as representing the tropical swamps of Köppen and Wegener (**14**). Nevertheless these authors seem to be justified by the weight of criteria, and if so, there remains in the field no alternative to some form of continental drift.

Additional evidence of positive value has been assembled by Harrassowitz (**15**) in his recent monograph on laterite. Fossil laterite profiles with more or less complete associations of laterite, bauxite, and kaolinite deposits have been found in the Upper Carboniferous of the United States (Kentucky and Ohio); Scotland (Ayrshire); Germany; Bohemia; Russia (South of the Moscow Basin) and China (Shantung). The inference that the equatorial zone of the time is roughly indicated by this belt is irresistible. It is, moreover, consistent with the correlation of the corresponding coal belt with tropical swamps. If the climatic conditions had then been such that laterite could have been produced over a wider zone than is possible to-day, then it is certainly remarkable that the only known remains should lie along a belt that would have been near the equator if the coast of Natal had at the same time been situated near the site of the South Pole.

Although the attempt to solve the problem made by Brooks fails to carry conviction—it is frankly put forward with diffidence—it remains an extremely interesting example of how palæogeographical and meteorological data can be correlated and interpreted. Its methods, considered along with those of Dr. G. C. Simpson (**13**), are likely to prove valuable in pointing a way towards possible explanations of certain scattered inconsistencies that still remain. One of these is provided by the position of the Squantum tillites, near Boston, Massachusetts. Wegener himself discusses this special difficulty in his contribution to the Symposium. If the beds in question are truly of glacial origin, they appear to stand in flagrant contradiction to his views, since they occur near his Permo-Carboniferous tropical belt. He pleads for an independent and impartial investigation of the problem, but he adds, not without justification, that the glacial hypothesis of these puzzling beds is also hopelessly in conflict with the adjoining palæoclimatic evidence of the time. Neither the drift nor any other theory can be expected to explain interpretations that are mutually destructive.

Schuchert, for example, in a recent review of the subject, writes (**26**): "The presence of tillites means either very high mountains or a great lowering of the snow line, and both of these physical conditions react decidedly on the organic world, for which there is no evidence in the Pennsylvanian or Permian of North America." He nevertheless accepts the glacial origin of the beds, and considers that they were formed by alpine glaciers that originated in a high mountain range that lay to the south-east. He believes that the Coal Measures flora and the insects indicate a warm climate, but that there was a later change towards much cooler conditions, culminating in a world-wide glacial climate in the Middle Permian." It follows therefore that the difficulty may be partly one of faulty correlations, and that the Squantum tillites represent deposits from a Himalaya-like range that was a local centre of glaciation at the time when Gondwanaland was buried under a great continental ice-sheet. If this could be established it would be an interesting proof of a world-wide temporary lowering of temperature, the origin of which would therefore be cosmic. It would, moreover, leave Wegener's deduction unshaken as to the Polar position of Gondwanaland. Glaciers can occur in the tropics, but ice-sheets covering millions of square miles over a much longer period of geological time could surely occur only around one or other of the poles.

THE RELATION TO MOUNTAIN BUILDING.—Bailey Willis (in the Symposium) raises the objection that the sial must be weaker than the sima if the mountains of Western America are to be interpreted as a result of resistance encountered by the westward drifting sial of the American continents. Bowie points out that if the sima have no strength, as postulated by Wegener, the continental front could not be crumpled up into mountains; for, to use Longwell's simile, the sima would then yield like water before a floating raft. These three authors (**1**)—and many others—have thus drawn attention to a serious inconsistency in Wegener's discussion of the mechanism of drift. From the point of view of crushing strength the surface representatives of sial are certainly weaker than those of sima. For crystalline basalt or gabbro (grain size here is of little importance) the crushing strength is $12 \times 10^8$ dynes per sq. cm., whereas for granite the corresponding value is $8 \times 10^8$.

But these results only touch the problem superficially. In the first place the mountains of Western America represent not sial crushed against sima, but a vast geosynclinal belt of sediments crushed between two relatively approaching jaws of sial. Now a belt of thick sediments must necessarily be weaker than the bordering continental blocks. During the growth of the geosyncline the subsiding sial floor must have become thin as a result of the stretching or outflow of the lower levels of the sial (**7**). In either case the feebly radioactive lower levels are thinned very much more than the more strongly radioactive upper levels. The latter are deeply buried beneath a thick load of sediments which themselves are often more radioactive on an average than the original sial, since they are necessarily derived from the upper levels of the sial of the adjacent lands. Thus, in short, the continental blocks undergoing denudation at the top become uplifted, less radioactive, cooler, and therefore stronger; while the subsiding belt becomes filled with sediments, more radioactive, hotter, and therefore weaker. Granted a certain amount of drift, there is consequently no difficulty in promoting mountain building; the eastern block would necessarily squeeze the geosynclinal belt against the western block and mountains would be raised by compression and splaying out over the borderlands. The fact that only a narrow strip of the western block (the *Cascadia* of Schuchert) can now be recognized may mean that most of it has been buried beneath deep-seated overthrusts from the east. According to this conception, much of the Atlantic could have been formed by the advance of the eastern block across the site of the Cordilleran geosyncline, and only later would North America as a whole have been enabled to push forward against the Pacific floor.

It must not be overlooked, of course, that a sedimented continental shelf may also be relatively weak, and may be piled up into mountains against the oceanic obstacle.

Here a second point arises suggesting that the superior crushing strength of crystalline basalt (= dolerite or gabbro) may be a misleading guide to its actual behaviour under stress differences in the earth. It is a well-known fact of metamorphic geology that old dolerite dykes in Pre-Cambrian gneissic areas have commonly been recrystallized to hornblende-schists, garnet-amphibolites and even eclogites while the adjacent gneisses show no sign of any comparable degree of recrystallization. The proof appears to be complete that a long-continued stress difference acting at moderate depths and temperatures finds dolerite more responsive than granite. In so far as rock-flowage can occur by recrystallization—a process well exemplified under more familiar conditions by the flowage of glaciers—dolerite or gabbro are certainly to be regarded as weaker than granite. Consequently, as the granitic rocks of the continents are pressed against the gabbro floor of the ocean, the rising stress difference will first overcome the opposing strength of the gabbro which will then be continuously deformed by flowage while the continental edge remains effectively strong.

It is next of importance to notice that the effect of a powerful stress difference on a thick floor of initial gabbro would not be to produce folded mountains of hornblende-schists and eclogites. The change of density from 3 to 3·3 or more and the simultaneous action of isostasy would lead, on the contrary, to marked subsidence, and oceanic deeps would result (**16**). On this hypothesis we should expect to find deeps along peripheral belts of the ocean floor where the compression is known (from the mountainous edges of the sial), to have operated recently; precisely, in fact, where they do occur.

In the Symposium it was recognized by Singewald that no one can say whether the ocean floor be folded or not, and van der Gracht pointed out that even if a mountainous bulge were produced in sima it could not possibly be maintained. My own view, for the reasons outlined above, is that the two kinds of material, *sial* and *sima*, adopted by Wegener to explain the two dominant levels (continental and oceanic-floor) of the earth's solid surface, serve equally well to explain the marked upward and downward departures from those levels. Compression and overthrusting lead to the uplift of plateaus and mountain ranges, when acting on deeply sedimented belts of the sial; and to equally marked subsidence (producing oceanic deeps) when acting on sima having the chemical composition and, to begin with, the mineral composition of an olivine-gabbro. The metamorphosed, folded and overthrust tract of heavy sima would be pressed down or would sink into the substratum, so making way for the continents to advance. Thus we get a definite clue to a possible means of "engineering" the drifting process.

A further clue is obtained from the early geological history of the Urals, which has been clearly described by von Bubnoff. The Urals are bounded on the west by the Russian shelf and on the east by the Siberian shelf, and until the close of the Silurian the Ural tract was itself part of the two stable regions which it now separates. At this time radiolarian rocks appear along a meridional belt, accompanied by basic lavas and intrusions of peridotites and gabbros, suggesting, as von Bubnoff says, "a great split invaded by foreign rocks from below." This particular geosyncline continued to develop until the end of the Devonian when compression and folding occurred, accompanied by granite intrusions. The evidence here points to distension as the cause of geosynclines rather than to magmatic denudation of the base of the sial layers. Now, if the triangular Russo-Fennoscandian block began to move away from the Ural belt at the end of the Siberian, one would reasonably expect evidence of contemporaneous mountain-building on the further sides. The movement would have components towards both the Scandinavian or Caledonian geosyncline and the Caucasus geosyncline. Fitting the expectation, each of these repositories of early Palæozoic sediments suffered acute orogenesis at the same time as the first Uralian geosyncline began to open. Lateral continental movement on a limited but demonstrable scale is thus proved to have taken place, leaving a rift behind and raising mountains in front. On the other side of the Urals the Siberian shelf probably moved towards the rigid block of Angoraland, closing up the Yenisei geosyncline that lay between. Here, however, we cannot as yet be certain that the two events were strictly simultaneous.

A careful study by Dr. G. M. Lees of the Oman mountain arc, a range of middle Cretaceous age that trends across the easternmost corner of Arabia, and strikes out abruptly against the Indian Ocean at Ras Madhraka, leads him to the conclusion that its geological history is inconsistent with Wegener's scheme (**17**). In so far as the latter involves a movement of Africa to the west this criticism is certainly valid, and, as we shall see, Wegener has certainly insisted far too strongly on the dominance of a drift to the westward. The Oman arc seems to imply a Cretaceous movement of the Afro-Arabian block towards the east or north-east. At that time there was no Red Sea and India was probably still more or less attached to Africa and Arabia. It may therefore be suggested that the lost continuation of the Oman arc originally lay along the northern shore line of the primordial Indian block (of which only a southern triangular remnant is now visible). After the formation of the Oman arc the Afro-Arabian and Indian blocks parted company, and only the latter continued moving in the north-easterly direction. The missing part of the arc may therefore have been pushed far into Asia, and if so it possibly now lies deeply hidden below the great plateau of Tibet (e.g. between the figures " 1 " and " 2 " under " Kouen " in Fig. 3). This interpretation is—naturally perhaps—not considered by Lees, but clearly, if it be true in principle, it solves a very real difficulty along lines that involve the acceptance of some form of continental drift hypothesis.

In the Himalayas and the elevated region that stretches away for nearly a thousand miles to the high walls of the Kuen Lun and Nan Shan we have a magnificent and formidable example of a phenomenon that is precisely the reverse of that involved in the formation of oceanic basins on sites that were previously continental. Over this vast region, which was formerly below sea level, the sial has been greatly thickened, and in the higher parts even doubled. It is clearly impossible that contraction could have produced such an effect on this gigantic scale; yet indubitably it has happened. According to the continental drift hypothesis a great northward extension of Peninsular India has been bodily thrust under the Tibetan plateau. Argand's bold and eloquent picture of the structure, here reproduced from his *Tectonique de l'Asie* (**18**) shows graphically that the mechanism portrayed thickens the sial to the appropriate order required for isostatic equilibrium. Wild though this conception may seem, it is nevertheless in harmony with the extravagant geological history of Tibet and the Himalayas; it explains the mysterious disappearance of the Oman arc; and it recognizes the almost incredible northerly drift of India that is implied by the facts (*a*) that in late

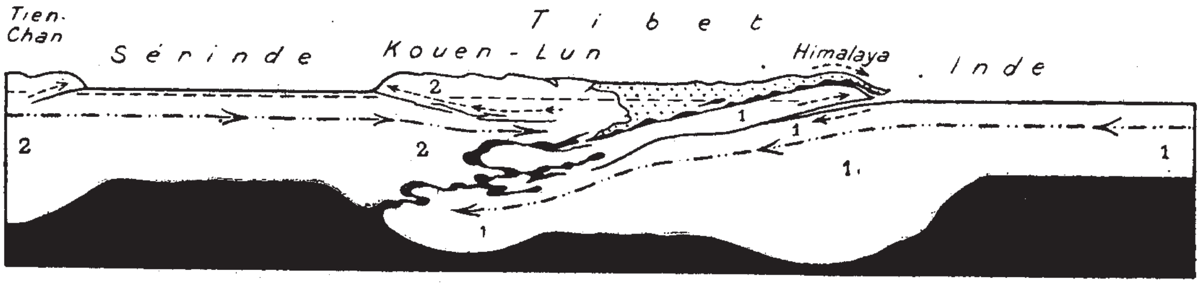



FIG. 3.—SECTION ACROSS INDIA AND CENTRAL ASIA TO ILLUSTRATE THE HYPOTHETICAL UNDERTHRUSTING OF ASIA, 2, BY GONDWANALAND, 1. Horizontal scale, 1 : 14,000,000. Vertical scale greatly exaggerated. Black represents *sima* supporting the *sial* (white) of the continental blocks. The dotted portion indicates the accumulated sediments of the Tethys which formerly lay between Gondwanaland and Asia. From Emile Argand (**18**, p. 349).

Carboniferous times India was glaciated from the south; whereas (*b*) from the Eocene onwards it became the active site of laterite formation.

THE DIRECTIONS OF THE DOMINANT MOVEMENTS.—Several of the authors of the Symposium (**1**) differ from Wegener with regard to the direction of drift. The contribution made by F. B. Taylor (who advocated a drift hypothesis two years before Wegener delivered his famous 1912 address) is worthy of serious attention in this respect. He draws an apt analogy between continental sial-blocks and continental ice-sheets, and pictures the crustal movements as having been radial and dispersive from both polar regions. Like du Toit (**2**) he regards the land of the northern hemisphere as being surrounded by an orogenic ring, nearly closed except across the Atlantic, and he describes the arcuate ranges as marking the terminal regions of "currents in the crust." The ring is obvious if one follows on a globe the following ranges: Alpine-Himalayan system—Asiatic Island festoons—Alaskan ranges—Cordillera of Western North America—Ranges of Honduras and the West Indies. Within it lie the great disruptive basins of the North Atlantic and Arctic oceans. Similarly in the south, as was first clearly recognized by Dr. J. W. Evans, the movements appear to have been more or less radially outwards from Africa towards the Pacific, the South Atlantic, Indian and Southern oceans being the corresponding disruptive basins left between.

Considerable movement from Asia towards the Pacific is indicated by the echelon structure of the island festoons. The analysis of these remarkable arcs by Tokuda (**19**) and his Japanese colleagues, and his success in imitating the structures by simple but illuminating experiments, point to a series of outward movements from continent to ocean. The orogenic forces cannot have been applied from above, and therefore movement of a substratum towards the Pacific seems to be implied. In each case the maximum amplitude of movement has been towards the middles of the arcs, becoming gradually less towards the more stable buttresses (Kamchatka, Yezo, Formosa, etc.) intervening between successive arcs. This conclusion agrees with the original surmise of Suess and indicates that Wegener's view of the island festoons as lag structures left behind during the westward drift of Asia cannot be maintained.

E. C. Andrews (**20**) has made a valuable comparative study of the mineralization of the lands bordering the Pacific, and particularly of Australasia, East Asia and North America. Despite the fact that on the American side the tectonic elements are closely pressed together, he has made it clear that there has been a marked outward growth of each of these continental areas. With advancing time, from the Pre-Cambrian onwards, the ore-deposits of each region appear successively further and further towards the present periphery, as if there had been a sub-continental outward spreading of ore-carrying material. Speaking generally, the ore-deposits are related to zones of mountain building and igneous activity—often in arcs—with the youngest nearest the ocean and the oldest farthest within the continents.

The movements involved in the changes that have affected the face of the earth since the close of the Palæozoic, thus appear to involve a breaking up of Laurasia and Gondwanaland with in each case a radially outward drift of the individual parts towards the Pacific and the Tethys. But this is not all. It is also necessary to assume a general drift, probably involving the whole of the crust, with a northerly component on the African side sufficient to remove Natal from the neighbourhood of the late-Carboniferous South Pole, and Britain from the late-Carboniferous tropics. This analysis results in a picture very different from Wegener's, but it is believed to be an accurate representation of the general tendency of the movements that seem to be required by the evidence.

SOME POSSIBLE CAUSES OF CONTINENTAL DRIFT.—As early as 1875, Suess deduced from his early studies of Alpine structure that "A mass movement, more or less horizontal and progressive, should be the cause underlying the formation of our mountain systems," but not unnaturally his conception of the process was cloudy. Bailey Willis in his well known text-book "Geologic Structures" draws a sound deduction when he writes (p. 131) "the evidences of movement noted in rock structures are so numerous and on so large a scale that it is clear that dynamic conditions exist from time to time; that is to say, conditions of very active movement. These require the development of unbalanced forces and, since rocks are exceedingly rigid and exceedingly strong, these unbalanced forces must be very great." Our next tasks are to deduce from the evidence what kind of unbalanced force is implied, and to try and identify a natural process competent to wield it effectively.

There can be no doubt that the reluctance on the part of many geologists to accept the straightforward testimony of the rocks in favour of continental drift is due to the fact that no gravitational or other force adequate to move the continental blocks in the required directions has been recognized. The two chief forces discussed by Wegener have been already mentioned (p. 206). Considering the drift from the poles towards the equator, Jeffreys has recently shown that, assuming the absence of strength in the substratum, the present viscosity that resists the movement is such that it would take about 3,000 million years for the whole crust to become symmetrical—as regards the distribution of continents—about the equator. With a less viscous substratum the time required would of course be less ; and just after the separation of the moon, the sial regions of the earth must have been symmetrical about the equator, for, had they not been, a very short time would have sufficed to make them so. As the earth is believed to have passed through the whole range of mechanical conditions from those attending the birth of the moon to those of the present day, it follows that the continents should never have got away from the equatorial belt at all. Thus it appears that the very distribution of the continents relative to the equator is itself an indication that some unrecognized agency has been at work to move the continents into the positions they now occupy.

The potential westerly drift due to tidal friction is wholly incompetent to move one continent relative to another, since the strength of the ocean floor would first have to be overcome. I am indebted to Dr. Harold Jeffreys for the information that tidal friction would have to be ten thousand million times as powerful as it is to produce the effects ascribed to it, and that incidentally it would then produce the fatal but unavoidable effect of altogether stopping the rotation of the earth in about a year. The special interest of this *reductio ad absurdum* is to show that if continental drift has occurred, the motive force cannot be of external origin (as tidal friction is) but must arise within the earth itself.

This conclusion shows that Taylor goes astray in the Symposium when he suggests that the " crust-moving force was of external origin." His particular hypothesis is that tidal forces would be adequate to explain the phenomena provided that the moon had been captured by the earth during the Cretaceous period. Unfortunately, even if this extravagant claim could be justified, we should be as far as ever from an explanation of the Hercynian, Caledonian, and older systems of folded mountains.

Joly's well known hypothesis of thermal cycles should also be mentioned here. According to this there is produced periodically a fluid substratum, which (if it were less viscous than the existing substratum) would undoubtedly facilitate slipping between the crust and the interior. Tidal drift aided in this way, would be likely to affect the whole crust, and could not do much in the way of initiating differential movements between the different parts. Joly makes a short contribution to the Symposium, and his most interesting point is that an aggregate of continents such as Wegener's hypothetical Pangaea could not exist permanently on account of the generation and accumulation beneath it of heat of radioactive origin, some means of escape for which would have to be provided. Unfortunately it has to be recognized that the theory of thermal cycles in its present form has not fulfilled the high hopes that it originally encouraged. I have elsewhere given a list of reasons for thinking it unsatisfactory and these need not again be repeated (**22**).

In the light of this preliminary survey it should now be clear that what is needed to move the continents about, as they appear to have moved, is a mechanism operating beneath the continents capable of stretching or splitting them and of dragging the parts away from each other. The radially outward movements of Laurasia and Gondwanaland suggest at once that a system of overwhelmingly powerful convection currents was generated beneath each great land mass. Mobility of the substratum is here undesirable ; only currents in a highly viscous glass could get a sufficient " grip " on the continental under-surfaces to exert the requisite drag upon the overlying material. The possibility of the existence of such currents was recognized by A. J. Bull eight years ago in a paper (**23**) that has not received the attention it deserves. More recently Jeffreys (**21**) has made the important statement " that the viscosity found for the lower parts of the shell (i.e. of the substratum down to a depth of 2,900 km.) is not enough by itself to prevent convection currents."

All, then, that is necessary to start such currents is that adjacent regions of the substratum should be unequally heated, and that, as in Joly's hypothesis, more heat should be generated at some depth beneath the continents than can escape through the overlying rocks by conduction. A slow but massive current (which might be reinforced by magmatic differentiation as it progressed) would then rise up beneath any region underlain by material having a greater heat output than that of the surrounding regions. One example of such a region would be a large continent (implying a radioactive cover) surrounded by oceans (free from such a cover). As the ascending currents approached the base of the crystalline crust they would turn over and exercise a powerful drag on the under-surface in radially divergent directions. The complementary downward currents would become strongest beyond the continental edges.

Each part of the continental mass would be enabled to move forward by the fracturing and foundering of the heavy ocean floor immediately in front, probably accompanied by over-riding of the ocean floor along thrust planes more or less lubricated by magmatic injections from the substratum. The sites of the ascending currents would become disruptive basins. Here the accumulation of excess heat responsible for the process would be discharged by the development of a new ocean floor and the current would consequently fade out. Meanwhile mountain building would have been accomplished on the continental margins or on the sites of former geosynclines and thus a totally new heat distribution would arise which would gradually generate a correspondingly different set of convection currents. The squeezing out of the more mobile parts of the substratum to form crustal magmas is an attractive side issue that opens up a new vista of possibilities in petrogenesis, and gives a hint as to the origin of basalts.

A point of great physical importance is that the currents move both their own boundaries and the sources of the heat responsible for their existence. Thus on every kind of scale from saggings of the crust to ocean basins, or from broad domes to great mountain systems, the vertical distribution of radioactivity in any one region may be periodically varied as geological history proceeds—specially hot regions tending to be opened out so that they become sites of rapid cooling; and specially cooled regions tending to be closed up or pushed down into the depths.

The case of a geosyncline lying between two continents, and filled with sediments more radioactive than the surrounding sial, will clearly give rise to a very complex interplay of opposing currents, such as may be necessary, for example, to explain the extraordinary phenomenon of the western basin of the Mediterranean between the Alps on the north and the Atlas on the south. The foundering of blocks of the compressed borders of the ocean floor is also a process which will contribute towards the constantly changing distribution of the radioactive elements. In general, convection currents on the gigantic scale here envisaged provide a physically sound mechanism for bringing about alternate accumulation and discharge of heat in any one region. The process is consistent with the proved simultaneous occurrence of tension and compression, this being a combination that speaks strongly against the validity of both the contraction hypothesis and the hypothesis of world-wide thermal cycles.

The convection currents hypothesis has also a great advantage over that of thermal cycles in so far as its alternations of compression and tension are not periodic on a world-wide scale. In this respect the ingenious mechanism conceived by Joly is far too ideal to match the facts of geological history, and my own first efforts to bring in a little more variety by adding peridotite cycles to the original basaltic cycles (**24**) equally fail to meet the requirements. The very complexity and the incalculable variety of the interactions of convection currents hold out a distinct promise that a theory based on them is much more likely to be ultimately successful than either of its predecessors. Moreover, it provides an answer to those critics of Wegener who wonder—like Schuchert, Longwell, and White (in the Symposium)—what forces can have conspired to hold the sial together in the great land-mass of *Pangaea* until Mesozoic time. There is of course, neither proof nor probability that there ever was a single *Pangaea*, and it is reasonably suggested by van der Gracht that there may have been a pre-Carboniferous "Atlantic" that was closed up during the Caledonian orogenesis. He is careful, indeed, to commend Wegener for not leading us into a discussion of remote periods concerning which our knowledge is still very meagre. The difficulty was nevertheless a real one so long as only gravitational forces were considered. Granted convection currents, the continents may open out and reclose in an endless variety of patterns.

This is not the place to develop the physics of the process that is here advanced as a contribution toward the solution of the tectonic problems of geosynclines, mountain-building and (at least in considerable part) of continental drift. So far as I can judge, there is no direct evidence adverse to the assumptions made, and a preliminary attempt to evaluate the shearing stresses involved shows that they are of the right order to do the work required and to do it in the given time. The confident assertion that it is "impossible" can, at any rate, no longer be brought against the hypothesis of continental drift. Merely to prove Wegener wrong is no longer an important issue.

There still remains, however, one very serious difficulty. On p. 344 I indicated that in addition to the radially outward movements of Laurasia and Gondwanaland there appeared to have been a general drift of the whole crust over the interior with a marked northerly component on the African side. Convection currents may explain the former; they cannot, unfortunately, have much bearing on the latter. But it must be remembered that there are other processes at work besides those that are due to gravitation and heat. No one has yet solved the problem of terrestrial magnetism to the general satisfaction, and until there is a solution it would be hazardous to speculate too far as to the possibilities of forces that may be set up by the interaction of magnetic and electric fields. Meanwhile, until these are adequately explored—and they are undoubtedly of the kind called for to solve this final riddle—no one can say that the crust may not be able to move relative to the poles. I am assuming the truth of the orthodox opinion that the poles themselves do not shift to any considerable extent relative to the earth as a whole, and that the real problem to be solved is therefore that of a bodily movement of the whole crust which can be superimposed on those more easily intelligible movements here ascribed to convection.

It is perhaps fitting to close on a note of perplexity, tempered, however, with the assurance that the circumstantial evidence of geology is not likely to be leading us far astray so long as we read it aright. One valuable feature of the continental drift hypothesis is that it is everywhere arousing interest in world-geology and in the geophysical methods of exploring the depths; methods which, by X-raying the earth, will sooner or later put the hypothesis to severe and searching tests. Meanwhile, we may perhaps be forgiven for attempting an interpretation of such data as we possess. Pierre Termier in an eloquent address on the same subject as this paper (**25**) well expresses the mental urge with which some of us, steerage passengers on the good ship Earth, are afflicted. "The least ignorant among us," he says, "the most daring, the most restless, ask ourselves questions; we demand when the voyage of humanity began, how long it will last, how the ship goes, why do its decks and hull vibrate, why do sounds sometimes come up from the hold and go out by the hatchway; we ask what secrets do the depths of the strange vessel conceal, and we suffer from never knowing the secrets."

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