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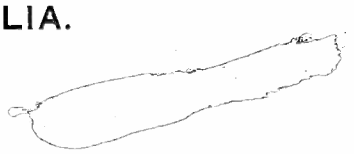

DEPARTMENT OF NATIONAL DEVELOPMENT.

BUREAU OF MINERAL RESOURCES
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RECORDS.

1956/37

SUMMARY OF CONTINENTAL DRIFT SYMPOSIUM, HOBART, MARCH 1956.

by

M. A. Condon & A. A. Opik.

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A symposium on the present status of the Continental Drift hypothesis was sponsored by the Australian Academy of Science and organized by Professor S.W. Carey at the University of Hobart from 6th to 9th March, 1956. The Bureau of Mineral Resources was represented by M.A. Condon, A.A. Öpik and G.A. Thomas.

The meeting recommended publication of the papers and discussion together, although money is not available at present. A brief summary of the papers and discussion is presented. Discussions during the meeting were tape-recorded; the records will be distributed and contributors will be able to produce a concise summary. The following notes are preliminary, composed from brief notes and memory, and will be completed later when the tape-recorded material is available.

After brief opening remarks by the Chairman, Professor M.L. Oliphant, Professor Chester Longwell of Yale University presented his estimate of the status of the Continental Drift hypothesis, namely that we lack the critical evidence, geological and geophysical, to reach a decision. The hypothesis must stand up to any scientific test which may be applied to it, otherwise it needs to be modified or abandoned. Of the tests which may be applied, Longwell mentioned the following: 1. The strength of the materials of the crust and mantle in the physical environment existing and likely to have existed during any postulated drift.

2. Demonstration of present drift if any. Longwell stated that since radio time signals became available for astronomical fixing of longitude the drift which had been suggested between Greenland and Europe has been demonstrated to be non-existent.

3. Contiguity of similar biota and fossil assemblages. Longwell stressed the need for caution in the use of this tool either to imply former contiguity or, on the basis of dissimilar assemblages, separation. This evidence may be suggestive or confirmative but is not likely to be conclusive.

4. Physical fits or matches of various kinds: a. Shape fit. Carey (1955b) has demonstrated the validity of the shape-fit of South America and Africa, one of the classic indications in favour of Continental Drift which had been questioned by Lees (1953). Because nearly all other parts of the continental masses have no large re-entrant there is no other shape-fit which can be demonstrated as indubitable. The use by Carey of geometrically correct methods of testing and demonstrating the fit indicates the only sort of evidence likely to be generally recognized.

b. Structural fit. When a shape fit has been established or proposed, it must be tested by the fit of major structures of the same age. It is not sufficient that orogenic belts are shown to coincide in position and direction; they must be demonstrated to have similar stratigraphic and structural components. Sheet deposits are unsuitable for demonstration of fit although they may be used to support the fit of linear structures. This makes demonstration of the structural fit of

Gondwanaland difficult; apart from the Andean and Tasman orogenic belts which are generally marginal to reconstructions of Gondwanaland the orogenic belts are Precambrian and therefore less susceptible to precise dating. Also they have not been mapped adequately.

c. Climatic fit. The spread of Permian glaciation over South America, South Africa, India and Australia and the associated flora was one of the facts out of which the Continental Drift hypothesis grew. It was regarded as impossible to explain this spread in terms of present continent distribution, and polar position and obliquity. It is known that with present continent distribution and pole position, continental glaciation of tropical India would necessitate global glaciation. Therefore one or more of present conditions must have been different. Physicists aver that the obliquity of the axis cannot change. Recent palaeomagnetic work indicates that the position of the axis relative to the land masses has changed throughout post Proterozoic time. No polar position can bring all the areas of Permian glaciation within 40° of the pole either under the present continent distribution or under any of the Gondwanaland constructions. This points to the need for precise dating of these glacial events.

d. Palaeomagnetism: Recent work has established that, relative to the continental masses from which samples were taken, polar positions have changed. Polar positions determined from formations in North America are different from those determined from British formations.

Longwell summarized the position by stating that the only evidence definitely in favour of Continental Drift so far demonstrated is the shape-fit of South America and Africa. All other evidence at present was not indubitable.

There was little discussion of Longwell's summary as it was agreed that many points raised would be treated more fully by later speakers.

Professor Lester King of the University of Natal presented a new reconstruction of 'Laurasia'. In this reconstruction the Atlantic Ocean remains only as a narrow gulf opening to the south, Greenland's east coast is in line with the east coast of U.S.A. and opposite the Siberian coast east of the Lena River delta. King claims the following stratigraphic-structural fit: Taconic orogeny of Appalachians and Urals; Erian phase of the Caledonian orogeny in East Greenland, Spitzbergen, Norway, Britain and Ireland; Triassic of Verkhoyansk Range (Siberia) and United States Range (N.W. Greenland); Variscan orogeny of S.W. United States, West Indies, possibly Azores, Brittany. He prefers separate continents of Laurasia and Gondwanaland.

King discussed the evidence for a movement of the upper Palaeozoic glaciation across Gondwanaland from Lower Carboniferous to Permian. He asserted that the last Palaeozoic glaciation in South America was Carboniferous in age; in South Africa, Upper Carboniferous; in India, Lower Permian and in Australia, Lower Permian. In all this glaciation is followed by Coal Measures. From this King reasoned that Gondwanaland moved across the pole in Carnoniferous-Permian time.

In discussing King's paper, Voisey pointed out that Ewing (1956) has obtained Lower Cretaceous in cores from mid-Atlantic showing that that ocean was in existence then. Deeper cores may indicate the age of the Atlantic or at least determine a limit on the break up of Laurasia.

Opik questioned the age-identity of the Appalachians and Urals orogenies. He also questioned the possibility of having a single ice cap of the dimensions indicated. The proximity of East Greenland and Siberia in Palaeozoic or Mesozoic times is an impossibility.

Mr. E. Irving of the Australian National University spoke on Palaeomagnetic data. He described the underlying principles of the method, the type of magnetometer used and problems and difficulties in sampling and testing. In the northern hemisphere there is growing consistency in results. A significant movement of the pole is indicated in both British and North American samples. The trace joining the determined pole positions from Upper Proterozoic to the present for British samples is east of that for American samples. Both traces have the same form, running from the pole to the east coast of Asia, swinging across the equator in the western Pacific to the vicinity of Hawaii in the Upper Proterozoic. In the British results the Carboniferous and Permian poles positions are off the main trace and the Carboniferous pole is nearer the present position than is the Permian Pole. It is thought that the movement of the pole position is sporadic rather than continuous. In a general way, known climates from Proterozoic to Present in Britain and North America agree with the pole positions so far determined. In testing continental reconstructions pole positions must be kept in correct spatial relationship to the sample locality. Many more samples from all parts of the earth are required before it can be demonstrated that there are significant differences in pole positions relative to various present continental masses. Consistent results from widely separated parts of each continent are as important as different results from separate continents.

In the southern Hemisphere only three formations have been tested - the Karoo Dolerite, the Tasmanian Dolerite and the Deccan Trap. These give pole positions which are widely separated and not related to Northern Hemisphere pole positions for the post-Palaeozoic.

In discussing Irving's paper, Carey pointed out the possible implication of the divergence of the trace of pole positions in respect of samples from Britain and North America; the indicated divergence is not sufficient to explain the whole of the shift of North America relative to Europe but it is in the right sense and may be sufficient if the movement is mainly a rotation, not merely a lateral shift. Plotting Irving's determinations on Carey's reconstruction of Gondwanaland, in correct relationships to the sampling locality, reduces the spread of the results but does not group them.

Professor J.C. Jaeger warned that surface samples are unsatisfactory as they give anomalous results. This has been demonstrated by comparison of surface samples with bore core and mine samples in the same locality and rock type.

[Palaeomagnetic reasoning is based on the dipole, the dipole being in the centre and connected with the matter of the core. This tacitly assumes that the division of the earth into crust, mantle and core existed from the beginning and is not subject of evolution, contrary to the assumptions in the models of Goldschmidt, Gutenberg and Wiebert, Ramsay, and Kuhn and Rittman. Kuhn and Rittman claim that there has not been time for even an approximate differentiation of the Earth's matter. Absence of a metallic (iron) core is probable which may render the dipole less stable, especially in the past. (A.A.O.)*]

Professor S.W. Carey (University of Tasmania) spoke on the tectonic approach to Continental Drift. He introduced the idea of an orocline - a bending of an orogenic belt (Carey, 1955a).

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* Square brackets indicate comments by M.A.Condon and A.A. Opik which were not presented at the symposium, mainly because discussion had to be stopped to keep to programme.

He recognizes an orocline by the juxtaposition of an orogenic belt curved in plan and, on the convex side of this bend, an angular area of deep sea with its apex pointing to the bend. Of this nature are the Alaskan Orocline and the Baluchistan Orocline. If this movement has occurred it should be possible to demonstrate that movement by reversing it in the direction and amount indicated by the angular area of deep sea. Carey stated that unwinding the 'Alaskan Orocline' brings North America into contact with Europe, and unwinding the 'Baluchistan Orocline 'brings Laurasia into contact with Gondwanaland in a single continent 'Pangaea'.

As well as the bend in the orogenic belt and the angular area of deep sea (regarded by Carey as a tension rift) this concept would require a shear beyond the area of the tension rift. A shear of the magnitude required would almost certainly be expressed as a major transcurrent fault rather than as a shear fault system or compressional fold system. For such a fault Carey proposes the name "megashear", and distinguishes it from a transcurrent fault by limiting the name to those lateral faults of more than 100 kilometers displacement. He indicated a number of faults which he believes to be "megashears" San Andreas Fault (post-Jurassic movement of 130 Km.) New Zealand Alpine Fault (post-Jurassic movement of 500 Km.) Carey implied that the large fault running east west through the West Indies is the "megashear" connected with the 'Alaskan Orocline'; he did not indicate the "megashear" associated with the unwinding of the 'Baluchistan Orocline'.

Carey then indicated that all the block rotations indicated in his "Pangaea" reconstruction are clockwise in the northern hemisphere and anticlockwise in the southern. [This of course pre-supposes that it is known which blocks moved and which did not - in terms of present position it could be the reverse rotation (M.A.C.)]

Carey asserts that, in the northern Hemisphere at least and as between north and south hemispheres this concept leaves no choice as to where the present continental blocks should go, and of course changes the shape of the blocks. If the concept is correct in principle it would explain some of the difficulties which have arisen from previous reconstructions of Laurasia. Although this was the most pertinent paper presented, Carey did not leave adequate time for discussion.

Oliphant agreed that an elliptical sheet symmetrically disposed about the equator and with its long axis normal to the equator could achieve the exaggerated kidney shape of Carey's Pangeia reconstruction, as a result of forces due to rotation.

[In both the "Alaskan" and "Baluchistan Oroclines" the short limb of the orocline is regarded as remaining stationary while the other very long limb and the whole continental mass adjacent to it moves - in the one case the whole of the American continent from Greenland to Paragonia, in the other the whole of eastern Asia from India to Kamshatka. Tectonically this is unlikely unless the small limb is in contact with a stable block; such a buttress is not indicated. (M.A.C.).]

[The "unwinding of oroclines" is not actual geology but constructive geometry. The curvatures of the mountain belts are hardly synchronic and cannot be referred to single pivots. The unwinding results in ideal linear mountain belts, the linearity being necessary for the hypothesis. Some of the curved belts may be real oroclines, some partly oroclines, and some represent originally curved geosynclines. The present South Sandwich Trench is already an 'orocline' although it is not yet folded. No linear mountain belt can result from it. Other curved trenches include the Peru-Chilean and the Marianas-Bonin Trenches. Many of the 'oroclines', consequently may follow pre-existing pattern and may not be "unwound". Linearity is a single case and the probability of its being the general shape is very small indeed.

The background of the unwinding of the oroclines and,

consequently, of the creation of the oroclines, is the Continental Drift. It is clearly a monistic hypothesis. The oroclines should be examined, each on its own merits, their sequence examined and correlated with the geological time scale. The orocline idea may lose in clarity if linked with Drift.

The Tethys as shown on Carey's models is not the Tethys of Suess. The rocks along the northern and southern borders of Carey's Tethys were deposited in the Tethys. The postulated primordial invagination of the Pangaea could not have been preserved as "Tethys" till the Mesozoic and even Tertiary. (A.A.O).

Professor K.E. Caster (University of Cincinnatti) spoke on Siluro-Devonian faunal provinces, particularly of South America and South Africa.

He recalled that Siluro-Devonian faunas may be separated into an "Austral fauna" found in central South America and the southern tip of Africa, and a Boreal fauna found in northern South America, North America, Europe and Australia. He believes that the nature of the Austral fauna (thin-shelled forms, no corals) indicates cold water environment.

He believes that the source area of sediments of the Palaeozoic Andean geosyncline was to the west.

He reported the discovery of Middle Devonian tillite in north-east Brazil, and of Lower Devonian or Upper Silurian tillites in the Parana Basin of South Brazil at the base of the Furnas Sandstone and within that formation.

Condon pointed out that the faunal characteristics taken by Caster to indicate cold water environment are more characteristic of stagnant water with limited oxygen. Caster agreed and added that the faunas do occur in black shales.

The Austral Devonian fauna forms a single province represented in South America and South Africa and contiguous when the continents are fitted together. The lithology is said to be most similar, and so is the sequence. But is this in itself evidence for, or against, drift? Sediments are derived from lands; similar lands produce similar sediments, with no regard to the distance between the regions in question. The faunas are marine and, consequently, former proximity of the regions is not an inevitable conclusion. Identities of age, lithology, fauna and environment occur even in antipodal positions (e.g. Pander Greensand in northern Western Australia and the Tremadocian glauconite sandstone in the Baltic) and have no significance in the proving or disproving of mobilism. (A.A.O.)

Mr. E.D. Gill (National Museum, Melbourne) spoke on Siluro-Devonian faunal provinces, particularly of Australia and New Zealand.

The Siluro-Devonian fauna of Australia and New Zealand is a Boreal fauna allied to those of Bohemia and North America. It has no similarity to the faunas of South Africa and southern South America.

Mr. G.A. Thomas (Bureau of Mineral Resources, Canberra) presented a paper by himself and J.M. Dickins on the "Correlations and affinities of the Permian faunas of Western Australia."

Thomas discussed the correlation of the Western Australian faunas with the Russian Permian through the Salt Range

faunas. On this basis the Sequence above the glacial sediments is indubitably Artinskian. Kungurian and Tartarian faunas are indicated higher in the sequence. Below the Artinskian only one sequence contains diagnostic ammonoid fossils (the Holmwood Shale of the Irwin Basin) but the other marine fossils are more primitive in type than the Artinskian but not of Carboniferous species or affinities. Faunal affinities of the fauna of the Western Australian glacial formations are with Eastern Australia including Tasmania, with the Umaria fauna of Peninsular India and with Argentina. Later faunas show more affinities with the Timor and Salt Range faunas than with eastern Australian faunas.

He emphasized that the main value of the faunas was in establishing the precise age of the deposits since similarity of faunas could not be taken to indicate contiguity, nor difference in faunas wide separation.

Caster asserted that the ammonoids were unacceptable [to him] as zone fossils. Only fusilinids could be so used. Thomas pointed out that the selected ammonoid species had been tied (in the Salt Range) to the fusulinids of the Russian type area, and the restricted range of those ammonoid species had been demonstrated. The supporting assemblage of brachiopods, pelecypods, bryozoa and corals added weight to the validity of the correlation. The discovery of Upper Carboniferous immediately under the glacial sediments in the Fitzroy Basin was further confirmation of the Lower Permian (Sakmarian) age of the glacial sediments.

Professor Caster presented a paper by Dr. R. Maack (Curitiba, Parana, Brazil) entitled "Vereisungs-perioden und Vereisungsspuren in Brazilien."

Maack states that in Brazil there is evidence of cold climate from Proterozoic to Permian. The main unconformities are pre-Devonian and post-Palaeozoic.

The basal tillite of the Furnas Sandstone is thought to be Silurian.

A map showing directions of ice movement on a Gondwanaland reconstruction indicate Maack's belief in multiple centres of accumulation with radial movement, but indicate movement in directions quite contrary to previous presentations.

Another map presented a reconstruction of South America and Africa with many structural lines running from one to the other.

Condon pointed out that the structural lines presented looked too straight to be natural and asked if the source of those structural lines was known. Caster was not able to supply that information.

Caster questioned the Silurian age of the Furnas tillite, which he prefers to regard as Siluro-Devonian.

[Devonian glaciation (as presented by K. Caster and R. Maack) in South America confirms the Devonian glaciation already known from the Falkland Islands. (A.A.O.)]

Mr. R.L. Bowen (Fulbright Scholar, Melbourne University) spoke on some Permian localities in Eastern Australia.

Bowen described terrestrial glacials from Victoria and Halletts Cove, South Australia, illustrated by slides. Tillite, compound tills, varves and glacial pavements were shown.

The glacial and marine Permian in Central Tasmania were derived from the west. This is indicated by the regional change in lithology from clastic to calcareous from west to east. The

volume of Permian sediments required the erosion of 15,000 feet of a mountain range 50 miles wide.

Carey objected to the mountain range - he would prefer a lower and wider source area. Bowen pointed out that most of the pebbles were of types common in the Lower Palaeozoic of Western Tasmania, and although probably not derived from the western Tasmania area almost certainly derived from the western extension of the same orogenic belt.

[This definitely places a south-eastern margin of the Permian Australian continent. The eastern margin is established as the western margin of the Sydney Basin; the western margin as the eastern margin of the Perth, Carnarvon and possibly the Canning Basins of Western Australia. (M.A.C.)]

[The interpretation of the Permian glaciation in Tasmania by Mr. Bowen is convincing: no actual ice-cap existed in Tasmania; the ice was on land (including mountains) immediately to the west. No conclusive picture for the Gondwana glaciation results, however: it might have been a number of isolated caps and centers, one of them being west of Tasmania; or a central ice-cap was surrounded by advanced and isolated smaller caps (like for example the present Iceland or the Alps in the last Ice Age). (A.A.O.)]

Dr. R.O. Brunnschweiler spoke on some modern ideas of Alpine folding.

The plan view of Alpine nappe sheets requires over-thrusting towards the north, not underthrusting from the north as required by Carey.

The Alpine structure is extremely asymmetrical with the large folds recumbent to the north.

The complicated shape in plan of the orogenic belt is explained by piece-meal movement of blocks rather than by movement of the European and African continents en bloc.

Carey explained that the underthrusting which he had referred to was crustal underthrusting which could produce the surface overthrusting which of course was evident. In his view the folding of the Alpine belt is rheidity folding, not elastic or plastic folding.

Brunnschweiler, apparently assuming that rheidity folding is indicated only by gneiss, stated that the material of the folds is un-metamorphosed sediments, which had not been deeply buried. [The geometry of the Alpine folds, however, is that of flow-folding so well shown by Carey (1954). It is not possible to develop this sort of fold by elastic strain]

[The plan-folding of the orogenic belt by movement of small blocks is even more difficult mechanically than Carey's concept of their formation as drag folds between two major continental blocks moving in opposed senses and producing a stress couple. The possibility that has not been examined sufficiently is that the present plan shape is fundamentally the shape of the original geosynclinal trough and that no very great lateral movement of the two sides of this trough are required to produce the rheidity folding which if regarded as elastic strains require very great horizontal contraction. M.A.C.]

Dr. J.W. Evans (Director of the Australian Museum, Sydney) spoke on "Insects and Continental Drift."

Evans described many of the insects and fresh water fish found on the high plateau of Central Tasmania as being of Palaeozoic or Mesozoic characteristics. They require a moist, cool

environment. Their present distribution is Tasmania, Alpine south eastern Australia, New Zealand and Chile. It is not possible that these insects could have achieved their present dispersal by [northern] land bridges because of the impassible climate barriers (dry belts, and particularly hot dry belts). If almost any of the Gondwanaland reconstructions is accepted the present dispersal of these primitive insects is readily explained.

Dr. Opik pointed out that Antarctica during much of the time since the Palaeozoic has been free of ice but would generally be cool and moist. With present distribution of continents and oceanic ridges it is not impossible to imagine a cool, moist migration route for these insects and fish.

[It is perhaps of significance that biological material and interpretations of geographic distribution are not compatible with palaeontological materials and interpretations. Biology has to consider hundreds of thousands of species; interpreters work with higher categories of the system (as families, orders) which have been considered abstractions since Linne. The study of the distribution of such "abstractions" cannot give reliable results about actual migration and spreading.

Of course, the distribution of wingless land insects and such is better understood when a certain amount of drift is assumed - for example if Australia were sometimes nearer to Antarctica in times of optimum climate. Extreme mobility of continents is unnecessary - small shifts in a few crucial spots would help our limited understanding. On the other hand we certainly do not know all the possibilities for migration of past and present life (A.A.O.)]

[The absence of these insects and fish from South Africa, even in the fossil record, argues against the present pattern having been produced by contiguity of continents at present separate; if anything it agrees with Dr. Opik's migration suggestion as Africa is the only continent not joined to Antarctica by a submarine ridge (M.A.C.)]

Professor H.N. Barber (Botany, University of Tasmania) spoke on "Plant geography and its interpretation."

He stressed the very great volume of data available on plant distribution and the need for a statistical summary of this data before conclusions are drawn. Any other method involves selection of material which may unduly influence the deductions.

There is good statistical evidence of long range dispersal of plants quite apart from any possible consideration of continental drift. He indicated dispersion of species characteristic of New Zealand and of Tasmania throughout the Pacific Ocean region and around the southern Hemisphere. Birds are undoubtedly responsible for some of this dispersal.

He mentioned the fact of important cytological differences within what is regarded as a species, suggesting another possible source of error in the data of dispersion.

It was pointed out that birds could not be held to account for the dispersal of Permian plants.

Mr. Gill presented a paper by Professor Sturton on Monotremes and Marsupials.

There are similarities between the monotreme and marsupial faunas of Australia and South America but the similarities are convergent or parallel rather than identical.

It would seem reasonable to expect a closer

relationship between Australian and Asian faunas than between Australian and South American.

[The dispersal of these vertebrates and the virtual absence of any land bridge between Asia and Australia since the appearance of the mammals, and perhaps indicates the possibility of a southern land bridge as suggested by Dr. Opik.(M.A.C.)]

Professor S. W. Carey spoke on the geological background of Wallaces Line.

Carey traced the growth of the concept of a line through the East Indies separating Asian and Australasian fauna and flora. The present indication that rather than a line it should be regarded as a zone in which some Asiatic species are advancing at the expense of Australasian and some Australasian species are advancing at the expense of Asian. It almost certainly indicates that the two biota, formerly separate and distinct were brought together comparatively recently. Carey suggests that this demonstrates the collision of Gondwanaland and Laurasia (or rather of the peripheral blocks of those disrupted continents).

Carey indicated how Wallace's Zone runs parallel to the line which he believes separates the Australasian Tertiary geosyncline from the Asiatic. To demonstrate the differences, Carey showed Umbgrove's maps of the distribution of the systems from Jurassic onwards: Jurassic and Triassic are confined to the Australasian side of his line. He also pointed out that structurally the two geosynclines are mirror images.

Condon pointed out that in the Asiatic geosyncline nothing older than Eocene is exposed: therefore it is not proper to assert that Mesozoic is not present. It is generally accepted that the Asiatic geosyncline is younger than the Australasian, but this is no reason to assume that they did not develop more or less concentric to their respective continents and that in the area between New Guinea and Borneo they were parallel although rarely contemporaneous. The fact that the one was generally submerged when the other was exposed is sufficient reason for the absence of intermixing of the biota except in Wallace's Zone.

Professor Lester King spoke on the origin of the great sub-oceanic ridges.

King would define the area of the sub-oceanic ridges as that within the 2000 fathom isobath. He states that these ridges are in isostatic equilibrium, and are composed of continental rocks including tholeiitic basalt. Some ridges particularly in the Pacific Ocean are composed of olivine basalt.

He regards these ridges as remnants of continental rock isolated during the drifting apart of the continents and therefore necessarily to be included in any reconstruction.

He exhibited reconstructions of Laurasia and Gondwanaland including the ridges. The mid- Atlantic ridge fills the gulf shown in his reconstruction of Laurasia shown earlier in the Symposium.

Carey stated that the 2000 metre isobath is the proper line to comparing shapes of continents; consideration of the volume of sial involved and the width of an equal volume with vertical sides reaching to the surface indicates that the isobath to be used is generally shallower than 2000 fathoms.

[This is the first time a proponent of drift has admitted the possibility of the mid-ocean submarine ridges being composed of continental rock. This eliminates Voisey's contention that demonstration of pre-Jurassic rocks on these ridges would indicate that

one or other of the continents on either side had not drifted across that area. (M.A.C.)]

Professor A.H. Voisey (University of New England) in formally criticizing the hypothesis of Continental Drift, reiterated his contention that as Lower Cretaceous sediments have been proved on the mid-Atlantic ridge it is possible that pre-Jurassic sediments may exist there also and if they do the American continent cannot have drifted across that area of ocean floor.
[King appears to have eliminated the grounds for that contention, although Carey is not convinced of the significance of the ridges. (M.A.C.)]

Voisey criticised the contention of the present supporters of the drift hypothesis that there is no evidence of continental growth by accretion of new material by geosynclinal sedimentation and orogeny. He described the Tasmen Geosyncline in some detail as evidence of such growth. [He missed the very important point that all geosynclinal sedimentation includes more or less basic volcanic material which is added to the volume of continental material at the expense of the sima. This is indubitable evidence of the growth of the continents regardless of whether or no the marginal geosyncline has a floor of continental or oceanic material. (M.A.C.)]

Professor Carey, replied to criticisms already published and presented at the Symposium.

He stated that he has demonstrated the shape fit of South America and Africa. [This is so although in doing so he ignores the submarine ridges].

He stated that faunal affinities of the areas brought together in his reconstruction are strong. [This is such a large statement that one would have preferred the demonstration of a few of these faunal affinities.]

It has been objected that the sima is too strong relative to the sial to allow passage of continental matter: under similar stress conditions the sial would be expected to fail before the sima. Carey stated that the likely mechanism - convection in the crust and mantle - is most likely to develop at the continental margin where rates of heat loss are most different. The surface layer of the sima and the whole of the sial would together tend to move and as it moved the convection current would move with it to continue the movement with it.

[This statement contains a fallacy common to many of the arguments used in favor of the hypothesis - it treats of only one part of a closed system; if a whole continent is considered it will be self-evident that the convection current with respect to the base of the continent will be centifugal and although there may be stresses produced tending to break up the continent if such a break did occur each part would quickly establish its own centifugal convection and there would be no movement apart of the two masses by this means. (M.A.C.)]

Professor J. C. Jaeger (Australian National University) summed up his impressions of the evidence.

He stated that any or all of the reconstructions should be tested as fully as possible in particular areas and especially in areas where the assumptions controlling the particular reconstruction may be examined. Thus the reconstruction of Laurasia would be best tested in the area of south western Europe, where Carey's concept of

the rotation of the Iberian Peninsula can be tested also. King's reconstruction of Laurasia is best tested by the fit of Greenland.

Rock magnetism is a blunt tool at present: errors may be caused by rock heterogeneity, weathering, reversals of field, and magnetic instability and until the effects of these upsetting factors can be recognized and eliminated the only practical way of getting a useful result is to make the work statistical, not only in relation to a single rock stratum but as regards the sampling of the rocks of a particular period of time and as regards the geographical spread of such rocks. It is of equal importance to demonstrate that results within a continent are consistent for a given period as to show that there is a difference between continental blocks. [This introduces once more the need to emphasize the requirement of precise stratigraphic dating of all samples and of comparing results from as short a period as possible in the stratigraphic environment of the material. For instance it would generally be possible to compare results from the various stages from the Mesozoic on but only from epochs in the Palaeozoic. Precambrian samples need to be treated with reserve unless linked to a radioactive dating. (M.A.C.)]

Dr. A. A. Opik (Bureau of Mineral Resources, Canberra) summed up as follows.

The presence of large mountain belts is sufficient evidence that large portions of the earth's crust have been shifted horizontally, that Continental Drift is a probability and may be investigated scientifically. Currents in the substratum of the crust are a logical conclusion in view of the reality of uplift and subsidence. However, the result of such currents cannot be folding or continental drift; friction is greatest when the crust is at rest above a "convection cell".

It must be emphasized that any proposed reconstruction is only a model and that demonstration of any particular kind of fit for any particular part of the model demonstrates only that that fit is possible but not that it necessarily did occur in nature. Only when the model demonstrates a large number of possible fits does the likelihood of the model approximating the natural events and occurrences become reasonable. They can be tested and improved not only by search for supporting evidence but, even more, by disclosing impossible fits. In this respect the main requirement is a refinement in the dating of geological events; events to be compared must be synchronous and unless the dating is precise and reliable no comparison is possible since no matter how good the apparent result it must be conditional on the correctness of the datings.

Wegener originally started from two facts: 1) the geographical homology of the coasts of Africa and South America, and 2) the occurrence of floras and faunas of temperate or tropical climate in the present Arctic and Antarctic latitudes. Beginning with these facts, the hypothesis of Continental Drift was, and still is, applied to explain palaeoclimatological problems. At the same time it serves to explain phenomena of inorganic geology and distribution of fossil and living organisms on the earth. On the contrary, Öpik regards such a monistic explanation of geological history, based on a single cause, as an assumption not warranted by the present state of knowledge. Mobilism (drift) and fixism (no-drift) when accepted axiomatically can be "proved" by the same set of geological facts, or selection of facts.

On the models presented at this symposium there are several obvious misfits which indicate that those models need revision: the Appalachians and Urals do not belong together, but the Appalachians and the east coast of Greenland may. In King's model, Greenland's east coast is shown in proximity to the eastern part of the northern Siberian coast. Greenland and Siberia in Palaeozoic time consistently belonged to different faunal provinces. Such a drift of Greenland disrupts the Appalachian-Caledonian provinces and contradicts the unity of the great basalt belt (Disco-Cap Brewster-Iceland-Faeroerne, etc) of Tertiary and younger age. King's and Carey's models leave the North

Atlantic open; Carey's model preserves the relationship of Greenland to America and Europe - this is preferable (Opik, 1940). The treatment of the treatment of the trends of Novaya Zembla, Paekhoy, Urals, Taimyr as seen in the models is not warranted by the state of knowledge. There are other possibilities. The Timan Mountains, according to Shatsky, consist of late Precambrian folded rocks, and cannot be connected with Caledonids or Variscids. The unreliability of the dating of the events in the upper Palaeozoic in the Southern Hemisphere prevents any conclusions being drawn.

The present position of Australia presents no difficulty in explaining its Palaeozoic history. In the models its positions in the Arabian Sea or on the edge of Gondwana make difficulties and even impossibilities, as for example the distribution of Acado-Baltic and Pacific Cambrian faunas. [It may be necessary to investigate the possibility of Australia being composed of two parts (eastern and western) originally geographically remote in Palaeozoic times and united by drift along the 138th meridian and forming part of Gondwana. Postulated impossibilities in the models beget more impossibilities.]

Another great difficulty in the models of Wegener 1924, du Toit, 1937, King 1950 and Carey, is the fit of Australia and Antarctica in which the Adelaidean Geosyncline, which should be open is fitted against the Antarctic shield. The westward bend of the geosyn-cline at Kangaroo Island indicates a gap between Australia and Antarctica in the Cambrian, and evidently in later times. The Cambrian fauna of South Australia is unique and cannot be connected with the Andine Cambrian.

Palaeoclimatology should be excluded from consideration, because the dominant cause of climatic changes is the variability of solar radiation. Terrestrial causes are masked by the larger cosmic causes.

Biological evidence, fossil and recent, is ambiguous and cannot support any hypothesis. But geophysical and geotectonic evidence may be used to explain the distribution of life in space and time.

A choice of models is already possible (several models by Wegener, by du Toit, by L. King, by S. W. Carey). Opik prefers the model with disconnected Laurasia and Gondwana and a sea between them; geological events are easier to understand from an assumption of smaller rather than large land masses. A Gondwana, for example, six to ten times as large as Australia would never have had a central ice cap, because of atmospheric dynamics and absence of supply of winter precipitation. A single ice divide as shown in King's model is even more improbable.

Mr. M. A. Condon (Bureau of Mineral Resources, Canberra) also summed up:

He agreed with Dr. Opik that any reconstruction is only a model to be tested for its approximation to the actual globe. Proponents of the Continental Drift Hypothesis such as Professors Carey and King put a great deal of thought and time into developing their ideas. In fairness to themselves and their ideas they should demonstrate instead of affirming various fits. Carey's demonstration of the shape fit of Africa is a good instance of this. Apart from the question of the mid-Atlantic ridge (which probably would not appreciably affect the fit) this demonstration will convince almost everyone of the validity of that shape fit while still allowing discussion of its geological significance.

It has been shown here, once more, that the geographical dispersal of plants and animals both marine and terrestrial is not understood sufficiently to be used to argue the former contiguity or separation of different areas on the basis of similarity or difference in faunas or floras. It is important at this stage that workers in other fields do not get a false impression of the established validity of the Drift Hypothesis so that they cease to think

in any other terms relative to reasons for the present dispersal of organisms.

The value of faunas and floras in the fossil record is in their use in dating the geological events which are the subject of comparison in the models. Only those events which are reliably dated by modern palaeontological-stratigraphical work can be accepted in this regard. Using unreliable data invites adverse criticism not only of the model but of the hypothesis as a whole.

Carey mentioned several large faults as being examples of "Megashears". Bowen has questioned the course of the San Andreas Fault. The New Zealand Alpine Fault, geometrically, may equally well be interpreted as a vertical fault of a gently north-plunging large syncline. He did not assert that this is so but pointed out the possible (and simpler) alternative interpretation.

Any model should among other things eliminate Atlantic type coasts, particularly with respect to events post-dating the postulated time of break-up. All models to date have shown New Guinea on the periphery of Gondwanaland. Recent work in the highlands suggests that the fundamental structure, in the Palaeozoic, Mesozoic and Tertiary troughs is meridional. The common acceptance of topography as indicative of structure needs to be questioned.

One of the more convincing results of Carey's Pangaea model in conjunction with the palaeomagnetic results is the removal of the northern continent from the Arctic Circle; in this respect the model agrees with geological evidence of the absence of continental glaciation in the Northern Hemisphere (apart from India) in upper Palaeozoic. Even on this model however it is not possible to enclose the areas of Permian continental glaciation within any 35° circle. (The 35° circle is based on astro-physical estimation that with the present obliquity of the earth's axis the advance of the polar ice-cap beyond latitude 50° would result in a global glaciation). With the present continent distribution the areas of Permian continental glaciation can be enclosed in two such circles, widely separated. This again calls for precise dating of these events since it appears that on any model the Antarctic circle must have occupied two different areas in about Lower Permian time.

Professor Longwell made the final summary. He stated that biological evidence can be confirmatory only; that some palaeoclimatic evidence was very strongly in favour of some re-arrangement of continental masses; and that present reconstructions ignored some established geological ideas.

In respect of this Longwell mentioned that the source of Mesozoic-Tertiary sediments of the Californian trough have been shown by regional studies of variation of lithology to have been to the west of California.

Longwell concluded that while this symposium had not established or disposed of the hypothesis, it gave promise through new techniques and improved dating of events that any model may be examined critically as to its agreement with physical and geological data.

REFERENCES

CAREY, S. W., 1954 - The rheid concept in Geotectonics.
Geol. Soc. Aust. J., 1, 67-117.

_____, 1955a- The orocline concept in geotectonics.
Pap. Proc. roy.Soc. Tas., 89, 255-288.

_____, 1955b - Wegener's South America-Africa assembly,
fit or misfit? Geol. Mag., 92, 196-200

EWING, 1956 -

Geol. Soc. Amer. Spec. Pap. 62.

LEES, G. M., 1953 - The evolution of a shrinking earth.

Quart. J. Geol. Soc. Lond. 109, 217-257.